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The Effect of State Medical Marijuana Laws on Social Security Disability Insurance and Workers' Compensation Claiming

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ABSTRACT

We study the effect of state medical marijuana laws (MMLs) on Social Security Disability Insurance (SSDI) and Workers' Compensation (WC) claiming among working age adults. We use data on beneﬕ claiming drawn from the 1990 to 2013 Current Population Survey coupled with a differences-in-differences design to study this question. We ﬕnd that passage of an MML increases SSDI claiming. Post-MML the propensity to claim SSDI increases by 0.31 percentage points, which translates to 11.3% relative to SSDI claiming propensity in our sample (2.7%). Point estimates for WC are imprecise.

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1 Introduction

Social Security Disability Insurance (SSDI) and Workers' Compensation (WC) are two of the largest social insurance programs in the United States. Each year these programs cost the U.S. government and employers nearly $208B (Baldwin & McLaren, 2016; Social Security Administration, 2016).[1] Indeed, in terms of annual expenditures, these programs are larger than unemployment insurance ($39B), Supplemental Security Income ($58B), and Temporary Assistance to Needy Families ($33B) (U.S. House of Representatives Committee on Ways and Means, 2016).[2] However, SSDI and WC are smaller in terms of expenditures than the two major U.S. public health insurance programs – Medicare ($676B per year) and Medicaid ($570B per year) (Centers for Medicare and Medicaid Services, 2017).[3]

Although SSDI and WC are costly, they are highly valued by workers and their families as these programs offer critical earnings support when workers become disabled or experience on-the-job injuries and illnesses, and therefore cannot work to earn income. Given their high costs, policymakers are grappling with strategies to support the SSDI and WC programs without placing undue financial burden on taxpayers and employers. As with many social insurance programs, SSDI and WC can potentially dis-incentivize labor market participation as they provide income without the requirement of work. Thus, assessing factors that influence the propensity to claim SSDI and WC is imperative for understanding how public policies affect U.S. labor markets. Finally, determining the existence and extent of policy spillovers, e.g. from public health policies to SSDI and WC programs, is important from a broader regulatory standpoint and can allow economics to better inform policy.

Beginning with California in 1996, U.S. states have implemented laws that legalize the use of marijuana for medical purposes ('MMLs') for patients with specific 'qualifying' health conditions. The objective of MMLs is to offer patients access to a medication that can be used to mitigate symptoms associated with chronic and acute health conditions. As of 2018, 29 states and District of Columbia (DC) have implemented an MML. The appropriateness of these state laws is fiercely debated as their effects are likely to vary across individuals based on the ways in which marijuana obtained through MMLs is used. Supporters of these laws argue that access to medical marijuana will confer substantial health benefits to

[1] The authors inflated the original estimates to 2017 dollars using the Consumer Price Index. The original estimates are $136B (2015 dollars) for SSDI and $62B (2014 dollars) for WC.

[2] Inflated from 2016 to 2017 dollars by the authors using the Consumer Price Index.

[3] Inflated by the authors from 2015 dollars to 2017 dollars using the Consumer Price Index. We note that there are other public insurance programs in the U.S. E.g., state-financed insurance programs and insurance programs for specific workers such as Veterans.

patients suffering from burdensome physical or mental symptoms which are not effectively treated by conventional medications and procedures. Opponents worry that MMLs provide an avenue to access marijuana for recreational, not medical, use and MMLs will foster marijuana addiction, misuse of other substances, and substance use-related social ills (e.g., crime, healthcare costs, traffic accidents, reduced productivity in the labor market) with, at best, marginal health benefits for the small number of legitimate medical users.

While the clinical literature on marijuana is nascent, the available studies suggest a role for medical marijuana in symptom management for many common health conditions. Indeed, randomized control trials show that medical marijuana can effectively treat symptoms associated with anxiety, chronic pain, depression, psychosis, sleep disorders, and spasticity (Joy, Watson, & Benson, 1999; Lynch & Campbell, 2011; Hill, 2015; Whiting et al., 2015; National Academies of Sciences & Medicine, 2017). Patients state that they use medical marijuana to manage symptoms related to health conditions (Nunberg, Kilmer, Pacula, & Burgdorf, 2011; Troutt & DiDonato, 2015; Reiman, Welty, & Solomon, 2017). Further, the majority of patients report using the product instead of medications prescribed by their healthcare provider and that medial marijuana better mitigates symptoms (Nunberg et al., 2011; Troutt & DiDonato, 2015; Vigil, Stith, Adams, & Reeve, 2017).

There is substantial overlap in the health conditions that are relevant for SSDI and WC claiming, and medical marijuana. For instance, in 2015, the three most common impairments among SSDI disabled worker recipients were musculoskeletal system disorders (e.g., back injuries), neurological disorders (e.g., multiple sclerosis), and mental health disorders (e.g., anxiety) (Social Security Administration, 2016). In terms of WC, qualification for these benefits requires that a worker is injured or becomes ill on the job, for example, through back injuries due to over-exertion or work-related traffic accidents that lead to chronic pain. All of these conditions could also qualify a patient for legal access to medical marijuana in many states that have passed an MML (Bradford & Bradford, 2016; Sabia & Nguyen, 2016). The commonality of relevant health conditions offers *prima facie* evidence for a relationship between benefit claiming and medical marijuana.

Since medical marijuana would provide a new treatment option to patients whose health conditions prompted temporary or long-term separations from work, passage of state MMLs could lead to spillover effects for SSDI and WC programs. Given the controversy regarding the extent to which MML passage leads to changes in medical versus recreational marijuana use, and the ensuing changes in management of symptoms associated with health conditions, the direction of any spillover effect is *ex ante* unclear. We directly address this question: we

explore the extent to which passage of an MML affects SSDI and WC claiming. We draw data from the Current Population Survey (CPS) on reported benefit claiming between 1990 and 2013 and estimate differences-in-differences regression models. We find that passage of an MML increases SSDI claiming. In particular, post-MML the propensity to claim SSDI increases by 0.31 percentage points or 11.3%. Our relative effect size is large as SSDI claiming is fairly uncommon: just 2.7% of our sample claims SSDI. Coefficient estimates for WC claiming are positive, which suggests that MML passage increases the propensity to claim, but are imprecise. Our results are robust across a range of specifications.

Our study contributes to three distinct economic literatures. First, the study adds to the large literature on the health and social effects of MMLs by considering unstudied outcomes: SSDI and WC claiming. Second, we add to the small, but growing, literature highlighting the relationship between specific medical treatments, and work propensities generally and use of social insurance specifically (Garthwaite, 2012). For instance, previous work has found an increase in sick leave and disability insurance claiming in response to aggressive monitoring of prescription opioids (Kilby, 2015) and the removal of Vioxx, a pain medication discontinued due to fatal side effects (Butikofer & Skira, 2016; Garthwaite, 2012). In contrast, use of sick leave decreased when workers obtained access to anti-retroviral treatments (Habyarimana, Mbakile, & Pop-Eleches, 2010) and minimally invasive surgery versus more aggressive procedures (Epstein, Groeneveld, Harhay, Yang, & Polsky, 2013). Further, labor supply increased among workers provided with AIDS treatment (Thirumurthy, Zivin, & Goldstein, 2008) and several studies show that mental healthcare treatment can increase labor supply and productivity (Berndt et al., 1998; Berndt, Bailit, Keller, Verner, & Finkelstein, 2000; Timbie, Horvitz-Lennon, Frank, & Normand, 2006; Andersen, 2015). Third, our findings contribute to the economic literature on policy spillover effects. Previous studies show that MMLs have spillover effects to public insurance programs (Bradford & Bradford, 2016, 2017). Moreover, there is a broader economic literature on policy spillovers. For instance, economists have assessed the extent to which minimum wage increases may influence use of welfare programs (Page, Spetz, & Millar, 2005; Reich & West, 2015), the effect of state WC program changes on SSDI claiming (McInerney & Simon, 2012), the effect of public insurance expansions for adults on enrollment in children's public insurance programs (Hudson & Moriya, 2017), and the effect of raising the retirement age on SSDI rolls (Duggan, Singleton, & Song, 2007).

The paper proceeds in the following manner: Section 2 provides a review of the SSDI and WC programs, the related economic literature, and the possible pathways through which MML passage may affect claiming. Section 3 outlines our data, variables, and methods.

Results are reported in Section 4. Extensions and sensitivity analyses are reported in Section 5. Finally, Section 6 provides a discussion of the findings and their potential policy relevance.

2 Background, related literature, and mechanisms

We next briefly review the SSDI and WC benefit programs, provide a discussion of marijuana regulation and the economic literature exploring MML effects, and outline mechanisms through which MML passage may lead to changes in SSDI and WC claiming.

2.1 Benefit programs background

2.1.1 Social Security Disability Insurance

Social Security Disability Insurance (SSDI) 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 U.S. Social Security Administration (SSA). The objective of SSDI is to provide income supplements to workers who face substantial restriction in their capacity to work due to disability. Benefits are temporary or permanent, depending on the nature of the worker's specific disability. Beneficiary payments are based on the worker's 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'),[4] (ii) the impairment is expected to last at minimum 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, 2016).[5] 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. Applicants must undergo a medical screening process to determine if they are eligible for SSDI benefits. Denied claims can be appealed. The time period between the initial SSDI application and the final decision can

[4]The amount of earnings considered to be SGA varies by disability. For example, according to the SSA, in 2018, the minimum monthly SGA earnings requirement for non-blind workers is $1,970 and $1,180 for blind workers (these earnings are net of impairment-related work expenses).

[5]This final requirement can be waived in some cases.

extend from six months to several years. Successful applicants are eligible for Medicare Parts A, B, and D two years after receiving benefits.

In 2015, 8.9M disabled workers received SSDI benefits with an average monthly payout of $1,166 (Social Security Administration, 2016). The number of claimants has risen substantially over time. Autor and Duggan (2006) show that the share of working age adults claiming SSDI increased from 2.2% in 1985 to 4.1% in 2005, an 87% increase. This increase in claiming occurred while there was no corresponding increase in self-reported disability (Duggan & Imberman, 2009). Explaining this apparent paradox has received substantial attention from economists. Autor and Duggan (2006) document several potential causes: (i) liberalization of the medical screening process attributable the Social Security Disability Benefits Reform Act (1984), (ii) the aging 'baby boomer' generatation, (iii) women entering the labor market, and (vi) the increasing value of SSDI benefits vis-a-vis potential labor market earnings of lower skill workers over the past several decades. In particular the authors argue that SSDI has become a substitute for work for many lower skill individuals.

Regardless of the cause for rising claiming, the financial solvency of the SSDI program is not secure. Estimates in the early 2010s indicated that the program would be insolvent in 2016 (Board of Trustees of the Federal Old-Age Survivors Insurance and Federal Disability Insurance Trust Funds, 2016). However, the Bipartisan Budget Act (2015) temporarily reallocated funds to the SSDI program, which has extended the solvency projection to 2023.

2.1.2 Workers' Compensation

Workers' compensation (WC) laws compel employers to provide employees who sustain injuries or illnesses in the workplace or in any other location while the employee is acting in the 'course and scope' of employment.[6] Workers who incur such injuries and illnesses are administered specified cash benefits, healthcare, and rehabilitation services, and – in the case of a worker's death – survivor benefits to dependents by the employer. Approximately 91% of the U.S. workforce was covered by a WC program in 2014[7] and workers become eligible for WC when they enter covered employment. Injured or ill workers receive temporary total disability benefits while they are recovering and cannot work.[8] These workers either return to work after they have recovered from their injury or illness, or, if they do not recover,

[6]Broadly, the course and scope of employment includes activities conducted on the employer's premises or directly related to completing tasks related to a worker's employment.

[7]Authors' calculation using data drawn from Baldwin and McLaren (2016) and the Bureau of Labor Statistics Local Area Unemployment Database. Details available on request.

[8]Workers who expect that they will be out of work for more than 12 months can also apply for SSDI. SSDI benefits are reduced for those workers who claim both WC and SSDI.

they are evaluated for permanent disability benefits. Some workers may not fully recover from their injury or illness, but may be able recover a sufficient amount such that they can participate in modified work and may be eligible for permanent partial disability benefits.

Distinct WC laws cover different groups of workers. State WC laws cover most private employees while there are federal programs that insure specific groups of workers including federal civilian employees, long shore and harbor workers, and high-risk groups of workers (e.g., coal miners with black lung disease, veterans). State WC programs insure the largest share of covered workers (Baldwin & McLaren, 2016). Each of the 50 states and the District of Columbia have a WC law that covers private workers. While there is substantial heterogeneity across states, these laws compel private-sector employers to provide WC coverage.[9] There are are some exemptions for small employers and for specific classes of workers (e.g., agricultural workers and domestic employees). We consider all forms of WC in our analysis.

The initial WC law in the U.S. was passed in 1908 and covered specific federal civilian workers (Baldwin & McLaren, 2016). New Jersey and Wisconsin were the first states to pass a WC law in 1911, and most states had implemented a WC program by 1920. WC benefits are largely, with few exceptions, financed by employers. Although there is variation in WC wage-replacement across states and federal laws, on average, this rate is approximately two-thirds of the worker's pre-injury gross wage. Healthcare benefits are available immediately to the injured worker but cash benefits are received after a waiting period (typically three to fourteen days away from work). Workers receive benefits regardless of who was at fault. In return for guaranteed benefits, workers are generally not permitted to bring a tort lawsuit against their employers for damages related to the work-related injury or illness.

In 2014, WC covered approximately 132.7M U.S. workers (Baldwin & McLaren, 2016). Total WC benefits paid in 2014 were \$62.3B, which were comprised of \$31.4B in healthcare payments and \$30.9B in cash payments for non-work time due to injury or illness. WC costs to employers totaled \$91.8B in 2014 (Baldwin & McLaren, 2016). Employers argue that WC costs place undue financial strain on their businesses which stifles growth, and advocate for policies that reduce such costs (e.g., lower premiums).[10]

[9] Some employers, who are concerned with overall labor costs, may shift WC-attributable costs to employees in the form of lower wages or other non-wage forms of compensation.

[10] https://www.riverbender.com/articles/details/beiser-reforms-to-workers-compensation-aims-to-bring-down-costs-increase-competition-20436.cfm#.WTARgevyuM8; and http://www.nydailynews.com/news/politics/n-y-drop-workers-comp-rates-saving-400m-employers-article-1.3168581; accessed June 1st, 2017.

2.2 Federal and state regulation of marijuana

Marijuana possession and distribution are illegal under federal law. Indeed, at the federal level, marijuana is classified as a Schedule I drug, part of the class of substances with 'no currently accepted medical use and a high potential for abuse.' In addition to marijuana, other Schedule I drugs are ecstasy, heroin, and lysergic acid diethylamide (LSD). Over time, there have been bipartisan efforts to re-schedule marijuana given established differences between marijuana and other Schedule I drugs in terms of their addictive and psychoactive properties. More recently, re-scheduling efforts have also highlighted the growing evidence base that marijuana has a medical use, which is contrary to Schedule I status.[11]

Given the federal prohibition on marijuana for medical purposes, some states have taken steps to legalize medicinal use of this drug. The first state to offer legal protection for medical marijuana users was California. In 1996, Proposition 215 (the Compassionate Use Act) was passed. This Act allows individuals, who receive a 'recommendation' from a medical doctor,[12] suffering from a wide range of health conditions to legally use marijuana to treat the symptoms associated with these conditions. Patients are able to access marijuana legally through home cultivation or state-approved dispensaries. Several other states quickly followed California in passing MMLs: in 1998 Oregon and Washington passed MMLs, and in 1999 both Alaska and Maine implemented a law.

29 states and the District of Columbia have implemented an MML. Common health conditions that qualify patients for legal use of marijuana include cachexia, cancer, epilepsy, HIV/AIDS, muscle spasms, multiple sclerosis, and non-specific pain (Bradford & Bradford, 2016; Sabia, Swigert, & Young, 2017). Many of these conditions overlap with conditions that qualify a worker for SSDI and WC benefits, offering premise for our study.[13]

We list the effective date for each state MML through January 2018 in column 1 of Table 1. We leverage law effective dates collected by Sabia and Nguyen (2016) in our study.[14] These laws offer the variation with which we identify treatment effects in our differences-in-differences models (outlined later in the manuscript). In our main analysis we focus on the passage of any MML, regardless of the law specifics.

[11] `http://www.washingtontimes.com/news/2017/apr/8/bipartisan-bill-would-rescheduled-marijuana-schedu/` and `http://www.thecannabist.co/2017/04/07/marijuana-federal-rescheduling-schedule-i/76885/`; accessed May 28th 2017.

[12] Medical doctors can recommend, but not prescribe or dispense, medical marijuana to their patients.

[13] We note that, while not the focus of our study, as of January 2018 seven states (Alaska, California, Colorado, Maine, Massachusetts, Nevada, Oregon, and Washington) and DC have legalized marijuana for recreational purposes. Several other states are considering such legalization.

[14] We consulted the website ProCon for the most recent years. Details available on request.

2.3 Economic evidence on the effects of state MMLs

The economic MML literature is substantial and it is beyond the scope of our study to comprehensively review this active area of research. Below, we briefly discuss the studies most relevant to our research question, focusing on the effects of these laws on use of marijuana and other substances, health, and labor supply.

A number of studies test the effect of MML implementation on adult marijuana use.[15] A limitation of the literature at this point is that there are no large-scale labor or health datasets, to the best of our knowledge, that allow researchers to separate medical from recreational use of marijuana.[16] Thus, the available studies provide an estimate of the changes in overall marijuana use following passage of an MML.

Chu uses administrative data and shows that MML passage leads to a 10% to 20% increase in arrests for marijuana-related possession and substance abuse treatment admissions (Chu, 2014, 2015). Pacula et al. (2015) show that passage of an MML leads to a 14% reduction in marijuana-related substance abuse treatment admissions using the same dataset as Chu (2014) and no change in self-reported marijuana use among a sample of young adults in the National Survey of Youth 1997. The authors note, however, that passage of an MML that allows for dispensaries increases both marijuana-related admissions to substance abuse treatment and self-reported marjuana use among young adults, while passage of an MML that requires patients to register with the state reduces such admissions and use. Leveraging data from the National Survey on Drug Use and Health (NSDUH), Wen et al. (2015) show that passage of an MML leads to a 14% increase in any prior month marijuana use and a 15% increase in near daily marijuana use. Choi (2014), re-affirms the relationship between MML passage and marijuana use established by Wen et al. (2015) in the NSDUH.

In addition to influencing use of marijuana, MMLs also change the use of other drugs and alcohol. Anderson, Hansen, and Rees (2013) show that, following passage of an MML, fatal traffic accidents decline 8% to 11%, with larger effects for accidents that do not involve alcohol. Subsequent analyses offer mixed evidence on the effect of MML passage on alcohol use: Wen et al. (2015) find that measures of alcohol misuse *increase* post-MML while Sabia et al. (2017) document that alcohol misuse *decreases*. Chu (2015) examines the effect of MML

[15] We note that economists have also studied MMLs in the context of youth and young adults (Anderson, Hansen, & Rees, 2015; Pacula, Powell, Heaton, & Sevigny, 2015; Wen, Hockenberry, & Cummings, 2015).

[16] While we acknowledge that there are some smaller surveys that collect data on these different forms of marijuana use from convenience samples, our point is that there are not large-scale repeated cross-sectional datasets suitable for standard policy evaluation methods (e.g., differences-in-differences as used in the studies we cite in our review of the literature) that collect this information. The literature that seeks to estimate the causal effects of MMLs on marijuana use using the above-noted methods faces this barrier.

on the use of cocaine and heroin, and finds that drug possession arrests and admissions to substance abuse treatment related to these substances fall post-MML. Choi, Dave, and Sabia (2016) document that smoking declines after MML passage. Specifically, post-MML tobacco consumption decreases by 0.3 to 0.7 percentage points. MML passage appears to spill over to perscription opioid use as well. Bachhuber, Saloner, Cunningham, and Barry (2014) examine mortality data and find that passage of an MML reduces the opioid overdose rate by 24.8%. Similarly, Powell, Pacula, and Jacobson (2015) document that MMLs reduce admissions to substance abuse treatment for opioid use and opioid-attributable overdose deaths.

There is also evidence that MML passage leads to changes in health outcomes that are plausibly linked with work-capacity and, in turn, SSDI and WC claiming. Sabia et al. (2017) find that following passage of an MML, days in poor physical and mental health decline while physical activity increases. Nicholas and Maclean (2016) document that, among older workers, reported pain declines and general health status increases following passage of an MML. MML passage is not generally linked with changes in the suicide rate, although there is some evidence that the suicide rate among younger men may decline post-MML (Anderson, Rees, & Sabia, 2014). Ullman (2017) shows that passage of an MML reduces work absences, suggesting that marijuana obtained through MMLs allows workers to better manage chronic health conditions that would otherwise impede work. Finally, heart attack-attributable deaths increase post-MML (Abouk & Adams, 2018).

Economic evidence further suggests that patients are using marijuana medically to treat symptoms associated with a wide range of health conditions, many of which are relevant for SSDI and WC claiming, following passage of an MML. Within Medicaid, a public insurance system for the poor, Bradford and Bradford (2017) show that following MML passage depression medications decline 13%, psychosis medications decline 12%, and pain medications decline 11%. Similar shifts away from traditional precription medications are identified within Medicare, a public insurance for older adults.[17] For example, following passage of an MML, perscriptons for anxiety medications decline by 5% (Bradford & Bradford, 2016).[18] Using the Medical Expenditure Panel Survey, Ozluk (2017) provides additional evidence that passage of an MML reduces prescription drug use. The author also shows that medical marijuana and prescription medications may be compliements for some patients.

Two studies investigate the effect of MML implementation on labor market outcomes. Using data drawn from the CPS Sabia and Nguyen (2016) conclude that passage of an

[17]Medicare also finances healthcare services for adults suffering from a set of serious illnesses.

[18]The authors document a similar decline in anxiety medications within Medicaid post-MML, but the estimate is not precise.

MML may decrease wages among younger males, but law passage is largely unrelated to wages among other groups of workers or any other labor supply outcomes examined by the authors. Nicholas and Maclean (2016) focus on older workers, defined as those 50 years and above, 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 (conditional on any work). The authors find no evidence that passage of an MML influences the unconditional probability of working among older adults.

Overall, the economic literature suggests that passage of an MML can influence substance use, health, prescription medication use, and labor market outcomes at least within some populations. To the best of our knowledge, no study has explored the effects of MML passage on SSDI or WC claiming. However, the existent literature – by documenting changes in substance use, healthcare services utilization, and health outcomes related to work-capacity – opens the door to the possibility that MML passage may influence SSDI and WC claiming through these channels. Our objective is to provide this evidence.

2.4 Mechanisms

Access to marijuana through MMLs can potentially lead to changes in SSDI and WC claiming in several ways. The pathways from MMLs to claiming likely vary based on whether users consume marijuana for medical or recreational purposes. We next consider the potential implications of both types of use for claiming.

MMLs, by increasing access to medical marijuana, could affect claiming by influencing symptoms associated with health conditions that qualify workers for SSDI and WC. As noted in the previous section, (i) patients state that they use medical marijuana to treat symptoms associated with health conditions related to SSDI and WC, and (ii) passage of an MML is linked with health conditions related to SSDI and WC. Further, there is a large body of empirical evidence to suggest that workers who are able to effectively treat symptoms associated with health conditions are less likely to claim SSDI and WC, or are able to exit SSDI and DI more quickly, than workers who are not able to treat such symptoms (Haugli, Steen, Lærum, Nygard, & Finset, 2001; Olofsson et al., 2010; Butikofer & Skira, 2016). However, the health effect of MMLs is *ex ante* unclear as the effect will be determined by a range of factors that likely vary across patients such as the underlying health condition, co-morbidities, and previous and concurrent treatment.

Patients may substitute marijuana for other treatments or use marijuana in combination with other treatments. The extent to which this substitution or co-use changes symptom

burden and, in turn, claiming behaviors will be determined by the relative effectiveness of marijuana vis-a-vis the patient's previous treatment and/or interactions between marijuana and other treatments. Effective medications reduce symptoms, which likely increases work capacity, but all medications impose side effects on patients which may reduce work capacity. The effect of using marijuana, rather than or in combination with other treatments, on work capacity and claiming behavior will be determined by both factors. Overall, relative effectiveness varies across health conditions for which marijuana can be legally used and hence the net effect of MMLs is difficult to predict. Further complicating predictions, there is heterogeneity in the effectiveness of medication – marijuana or otherwise – across patients due to differences in genetics, lifestyle, and so forth (Porter, 2010).

Individuals who use marijuana recreationally, and/or increase recreational use of other substances, following passage of an MML are unlikely to experience health gains and ensuing reductions in claiming.[19] Because substance abuse is not currently a qualifying condition for SSDI, we do not suspect that MML passage should have a direct effect on SSDI claiming through development, or worsening, of substance abuse problem.[20] However, MML-attributable susbtance abuse problems may exacerbate other health conditions which may lead to an SSDI claim. Moreover, if workers are intoxicated by marijuana used medically or recreationally while working, or experience 'hangover' effects from off-work use, it is plausible that such use may increase the risk of a work-related injury or illness, leading to an SSDI or WC claim (Goldsmith et al., 2015). There may be spillover effects from intoxicated/hungover workers to other (sober) workers, exacerbating the effects of MML passage on claiming. Finally, if MMLs lead to reduced productivity, and in turn lower wages (Sabia & Nguyen, 2016), then some margnial workers may opt to claim benefits as the relative costs and benefits of working and claiming change.

Overall, the potential effect of expanded access to marijuana through MMLs on claiming is unclear given the complex set of pathways that may act in conjunction, or in opposition, to one another. Moreover, MMLs may alter the propensity to claim and/or may alter the duration of a claim. Our objective in this study is to estimate the net effect of MMLs on

[19]We note that marijuana may be a substitute for alcohol, cocaine, and heroin (Anderson et al., 2013; Chu, 2015). Such a relationship between these goods might suggest that MML passage may reduce alcohol, cocaine, and heroin use which could improve health and reduce claiming.

[20]In 1996 the U.S. Congress removed substance abuse as qualifying conditions for SSDI. In our main analysis we use SSDI data between 2001 and 2013. Thus, substance abuse cannot directly qualify a worker for SSDI during our study period. While substance abuse cannot be used to qualify for SSDI, this condition does not exclude individuals from eligibility. Indeed, Moore (2015) provides suggestive evidence that a substantial share, 19%, of current SSDI beneficiaries have suffered from substance abuse at some point.

SSDI and WC claiming within a sample of working age adults. While understanding the specific mechanisms is clearly important, documenting whether or not there are spillover effects from MMLs to benefit claiming is a necessary first order question.

3 Data, variables, and methods

3.1 Current Population Survey

We draw data from the 1990 to 2013 Annual Social and Economic Supplement (ASEC) to the CPS from the Integrated Public Use Microdata Series (IPUMS) project (King et al., 2010).[21] The ASEC interviews approximately 150,000 U.S. residents 15 years and older each year on labor market, income, and health insurance outcomes each year in the month of March. The ASEC is a standard survey dataset utilized by economists to study both SSDI and WC claiming (Krueger, 1990; Autor & Duggan, 2007; Bronchetti & McInerney, 2012; Burkhauser, Houtenville, & Tennant, 2014). We include respondents 23 to 62 years.

The ASEC does not include information on marijuana use. Thus, we cannot estimate a 'first' stage regression, the effect of MML passage on marijuana use, and use this estimate to 'scale up' our reduced form estimates (effect of MML passage on claiming outcomes). Instead, our estimates are intent-to-treat (ITT). We note our lack of a first stage estimate as a limitation of our study. However, we return to the ITT nature of our estimates later in the manuscript with thinking through the plausibility of our effect sizes.

3.2 State-level medical marijuana laws

We use data on MML effective dates collected by Sabia and Nguyen (2016) to capture states' medical marijuana law environment. Specifically, using this information, we construct a variable coded one in state/year pairs with an MML in place and coded zero in state/year pairs when there is no MML. ASEC respondents are interviewed in March, but income information (including the information on benefit claiming that we leverage in our study) pertains the the previous calendar year ('income year'). We match state MMLs to the *income* year, which is one year prior to the *survey* year.[22] Thus, our study examines SSDI and WC claiming over the period 1989 to 2012. Given that the SSDI application process takes time,

[21] We choose to truncate the data in 2013 as the survey underwent a substantial re-design of the income questions in 2014 and our benefit claiming outcomes are based on a sub-set of the income questions.

[22] For example, a respondent to the 2010 ASEC has an income year of 2009 and a survey year of 2010.

we lag the MML variable one year.

In our main analyses we focus on the effect of any MML on benefit claiming. However, there is substantial heterogeneity in how states chose to regulate the use of medical marijuana (Sabia & Nguyen, 2016). We capture the average effect of implementing an MML through our binary law indicator. This effect may reflect, among other things, access to a new medical treatment for a specific set of health conditions, or public perceptions of marijuana as a new medical treatment option and risk of using marijuana recreationally.

3.3 Outcomes

We examine two claiming measures: any SSDI claiming and any WC claiming.[23] The ASEC is administered in March, but the income variables pertain to the past calendar year. Our SSDI benefit claiming variable has limitations which must be considered when interpreting our results. Specifically, the SSDI variable is derived from an overall survey item on Social Security Income receipt for the respondent herself or as combined payments received by the respondent and family members. Such payments may include SSDI and other forms of Social Security payments (e.g., Old-Age and Survivors Insurance). We wish to study SSDI benefits and not other payments. To isolate worker-received SSDI benefits we take several steps. (i) As noted earlier, we focus on a sample of prime age adults (23 to 62 years). Excluding younger and older adults can allow us, to some extent, remove dependent SSDI claimants and old age claimants. (ii) Beginning in the 2001 survey, respondents are asked to report up to two reasons for receiving Social Security payments. One of the possible reasons for receiving these payments is a respondent's own disability. In our main analyses we use data from 2001 to 2013 and include only those workers who report disability as their first or second source of Social Security payments in our classification of SSDI claiming.[24] In extensions to the main analyses we explore three alternative approaches to measuring SSDI. We report results using data from 1990 to 2013 in which we (i) construct our SSDI variables based on all Social Security income (which includes both SSDI and other Social Security payments) and (ii) requiring that the respondent report both Social Security payments (regardless of source) and a work-limiting disability at the time of the survey to be classified as receiving SSDI. The work limiting disability requirement may allow us to better capture SSDI payments (Burkhauser et al., 2014). Finally, we use data from 2001 to 2013 and construct similar

[23] We include all forms of WC.

[24] More specifically, we classify respondents who report Social Security income for other reasons as not receiving SSDI. More details are available on request.

variables as we utilize in our main analyses, but we include respondents who report SSDI as their first and not second source of Social Security payments.

After making exclusions for missing variables used in the analysis, we have 1,421,399 observations in our SSDI sample and 2,243,528 observations in our WC sample.

3.4 Controls

We control for respondent age, race, Hispanic ethnicity, and educational attainment.[25] We also include state variables to account for time-varying between-state heterogeneity that may be correlated with the probability that a state passes an MML and our claiming variables, and hence minimize bias in regression coefficient estimates due to omitted variables. To this end, we include the unemployment rate and hourly wages among prime age workers (23 to 62 years) based on the authors' calculations from the CPS Outgoing Rotation Group (ORG) and the poverty rate (University of Kentucky Center for Poverty Research, 2016).[26] We also control for labor market and social policies: minimum wage (i.e., either the state or federal wage, whichever is higher), state-to-federal Earned Income Tax (EITC) ratio, and maximum Temporary Assistance for Needy Families for a family of four from the University of Kentucky Center for Poverty Research (2016) and a prescription drug monitoring program (PDMP) (Ali, Dowd, Classen, Mutter, & Novak, 2017). Finally, we control for the Governor's political affiliation (Democrat or not) and the state population (University of Kentucky Center for Poverty Research, 2016). We follow Maclean and Saloner (2018) and treat the Mayor of DC as the *de facto* Governor. We inflate all nominal values to 2013 terms using the Consumer Price Index. We match the state-level variables to the ASEC income year using the MML matching procedure (described earlier in the manuscript).

3.5 Empirical model

We estimate the relationship between state MMLs and benefit claiming with the following differences-in-differences (DD) regression model:

$$B_{jst} = \beta_0 + \beta_1 MML_{st} + X'_{jst}\beta_2 + \rho'_{st}\beta_3 + \lambda_s + \gamma_t + \Omega_{st} + \mu_{jst} \quad (1)$$

B_{jst} is a benefit outcome for individual j in state s in year t. The MML_{st} is an indicator

[25] Results are robust to excluding the individual-level controls from the regression model.

[26] ORG data are obtained from the CEPR Uniform Data Extracts (`http://ceprdata.org/`; accessed June 1st 2017).

for a state MML. X_{jst} is a vector of personal demographic variables and ρ_{st} is a vector of time-varying state characteristics. λ_s is a vector of state fixed effects and γ_t is a vector of year fixed effects. Ω_{st} is a vector of state-specific linear time trends, which allow the outcomes in each state to follow a separate linear time trend. μ_{jst} is the error term. We estimate probit models and report average marginal effects rather than beta coefficients. We cluster the standard errors around the state (Bertrand, Duflo, & Mullainathan, 2004). All results are unweighted.[27] We pool men and women in our main specification.

4 Results

4.1 Summary statistics

Table 2 reports summary statistics. We report results for the full sample, and then for states that have and have not passed an MML by 2013 (the last year of our study period).

Roughly 2.7% of the sample reports receiving any SSDI income and 1.1% of the sample reports receiving any income WC. Thus, the prevalence of claiming is relatively low, which is not surprising as most workers do not require SSDI or WC benefits as they are not disabled or injured on the job. 16.2% of the sample has a state MML in place. When we separately consider our benefit claiming outcomes in the samples of states that passed and did not pass an MML by 2013, we see that the prevalence of SSDI claiming is higher in states that do not pass an MML while the prevalence of WC claiming is higher in states that pass an MML.

We report characteristics of individuals who report receiving SSDI and WC benefits in Table 3. SSDI claimants are older than WC claimants: 49 years vs. 42 years. Further, SSDI claimants are more likely to be female, more racially diverse but less ethnically diverse, and less educated than WC claimants. In terms of labor market outcomes, SSDI claimants worked fewer weeks in the past year than WC claimants (3.77 vs. 31.01) and have lower personal earnings ($865 vs. $19,161). Moreover, SSDI claimants have worse health as measured by a work-limiting disability than WC claimants: 86% of SSDI claimants report a work-limiting disability while 39% of WC claimants report this condition. We also report these characteristics for the sample of adults that does not claim either SSDI or WC in Table 3. Non-claimants are younger, more likely to be female, more highly educated, have higher labor force attachment, and have better health than claimants.

[27] Results weighted with CPS sample weights are not appreciably different.

4.2 Internal validity

A necessary assumption for DD models to recover causal effects is that the treatment group (i.e., states that passed an MML) and the comparison group (i.e., states that did not pass an MML) would have followed the same trends in the post-treatment period, had the treatment group not been treated. This assumption, referred to as the 'parallel trends' assumption, is of course untestable as treated states did in fact pass an MML, hence we cannot observe counterfactual post-treatment trends for these states. However, we can attempt to shed some suggestive light on the ability of our ASEC data to satisfy the parallel trends assumption. More specifically, we examine trends in our outcome variables in the pre-MML period. To do so, we center the data around the MML effective year. For states that did not pass an MML by 2013, we randomly select a 'false' effective date and center the data around that date. Thus, years prior to the effective year take on negative values, the effective year is coded as zero, and years after the effective date take on positive values. We truncate the data to the nine years surrounding the event. More specifically, we include all years more than nine years pre- (post-) event in the nine year bin. We plot unadjusted trends in any SSDI income (Figure 1) and any WC income (Figure 2).

While trends for the states that passed and did not pass an MML for WC claiming appear to move broadly in parallel in the pre-law period, the trends are more ambiguous for SSDI claiming. However, these figures capture unadjusted trends in the claiming variables while our regressions control for a rich set of time-varying individual- and state-level factors, including state-specific linear time trends, that may account for some, albeit linear, differences in trends. In terms of the post-law period, we see an initial departure between passing and non-passing states in the early years post-law: claiming appears to increase. However, the difference in trends is less obvious as time passes suggesting that there may be dynamics in any relationship between MML passage and our outcomes.

To dig deeper into the ability of the ASEC data to satisfy the parallel trends assumption, we next estimate regression-based testing. More specifically, using only data prior to the MML effective date (real or false), we estimate the following regression model using OLS:[28]

$$B_{jst} = \alpha_0 + \alpha_1 Treat_s * Trend_{st} + X_{jst}^{'}\alpha_2 + \rho_{st}^{'}\beta_3 + \delta_s + \eta_t + \epsilon_{jst} \quad (2)$$

We interact an indicator variable for states that pass an MML by 2013 ($Treat_s$) with a linear time-to-event trend ($Trend_{st}$; this variable differs across states depending on their

[28]We follow Popovici, Maclean, Hijazi, and Radakrishnan (2017) and estimate OLS to test parallel trends given challenges in interpreting interaction terms in non-linear models.

MML effective date), where the event is the passage of the MML. We replace the year fixed effects with time-to-event fixed effects, and all other variables are as defined previously. We exclude the state-specific linear time trends from this regression as they would be collinear with our key covariate in the model ($Treat_s * Trend_{st}$). If we cannot reject the null hypothesis that $\alpha_1 = 0$, that is that the states that did and did not pass an MML followed the same trend in years prior to MML effective date, then this pattern of results would provide additional support for our use of the DD model to study MML effects on claiming.

Results from our regression-based testing of the parallel trends assumption are reported in Table 4. Neither of the interaction term coefficient estimates are statistically different from zero; thus we cannot reject the null hypothesis that our claiming outcomes moved in parallel in treated and untreated states prior to passage of an MML. Re-assuring, the point estimates are small in magnitude. We note that the standard error estimates are somewhat large and prevent us from ruling out non-trivial differences in pre-treatment trends. However, we control for state-specific linear time trends in our regression models, which will account for specific types of trends.

4.3 Regression analysis of benefit claiming

Table 5 reports selected results generated in our DD regression models for SSDI and WC benefit outcomes respectively. Passage of an MML leads to a 0.31 percentage point (11.3%) increase in the probability of SSDI claiming and a 0.08 percentage point (7.5%) increase in WC claiming; although the latter estimate is not statistically different from zero. In terms of the economic significance of our estimates, while the estimated absolute effect sizes (i.e., percentage point) are potentially modest the estimated relative effect sizes (i.e., %) are arguably non-trivial. We suspect that the low prevalence of our claiming outcomes, 2.7% claims SSDI and 1.1% claims WC, produces non-trivial relative effect size estimates.

4.4 Heterogeneity in MML effects by age

Older adults are more likely to suffer from many of the health conditions whose symptoms may be effectively treated with medical marijuana and are more likely to claim SSDI and WC (see Table 3) than younger adults (Gordon et al., 2002; Morgan, 2003; Leske et al., 2008; Unruh et al., 2008; Nahin, 2015). Moreover, as noted by Sabia and Nguyen (2016), younger individuals are more likely to use marijuana obtained through MMLs for recreational, and not medical, purposes than are older individuals. These differences in relevant health conditions,

risk for claiming, and reasons for marijuana use open the door to differential relationships between passage of an MML and our claiming measures by wage. We next examine potential heterogeneity in MML effects by age. More specifically, we estimate Equation 1 in samples of adults ages 23 to 40 years and 41 to 62 years; results are reported in Table 6.

We document some evidence of heterogeneity by age group: passage of an MML increases claiming propensity for both SSDI and WC among younger adults, but not among older adults. Effects are precisely estimated among younger adults than older adults, and the magnitude (both absolute and relative) of the estimated effects is much larger among younger adults. Passage of an MML leads to a 0.32 percentage point (27.6%) increase in SSDI claiming and a 0.19 percentage point (16.8%) increase in WC claiming. Relative effect sizes are large as the proportions of younger adults that claim SSDI and WC are particularly low; 1.2% and 1.1% respectively. Among older adults, passage of an MML leads to 0.27 percentage point (6.6%) in SSDI claiming (although this estimate is imprecise), the coeficient in the WC sample is essentially zero and imprecise.

While our data do not allow us to explore why MMLs are linked with claiming among younger but not older populations, we can propose possible reasons. (i) Younger adults whose marijuana use changes with passage of an MML may be using this product recreationally and not medically. (ii) Medical marijuana may be an ineffective treatment, and indeed may worsen underlying health conditions, among younger adults. However, 95% confidence invervals overlap which prevents us from ruling out the possibility that effects are more comparable across age groups.

4.5 Heterogeneity in MML effects by sex

In our primary analysis we pool men and women, this specification implicitly assumes that the effect of MML passage is common across these groups. As reported in Table 3, men are more likely to claim both SSDI and WC than women. Moreover, men and women are differentially likely to experience, and seek treatment for, the types of health conditions for which medical marijuana may be an effective treatment. For instance, women are more likely to report mental health problems and seek related treatment than men (Center for Behavioral Health Statistics and Quality, 2016). We next test for different effects of MMLs on benefit claiming by sex by estimating separate regressions for men and women.

Results are reported in Table 7. Coefficients are positive in all regressions but are only precisely estimated in the male sample; indeed the effect sizes are generally comparable across the two samples. Passage of an MML leads to 0.36 percentage point (12.8%) and a

0.14 percentage point (10.2%) increase in SSDI and WC claiming propensity in the male sample. Within the female sample passage of an MML leads to a 0.25 percentage point (9.4%) and a 0.03 percentage point (3.8%) increase in SSDI and WC claiming.

4.6 Heterogeneity in MML features

Thus far we have considered the effect of any MML, regardless of its specific features. We next investigate the extent to which laws that allow for collective cultivation of medical marijuana for multiple patients ('group growing'), operating dispensaries, and non-specific pain as a qualifying condition influence claiming.[29] We also examine whether an MML that mandates that the state keep and maintain a medical marijuana patient registry system influences our claiming outcomes. As outlined by policy scholars, MMLs that allow legal access to marijuana (cultivation and dispensaries) may have greater effects on marijuana use (Pacula et al., 2015; Sabia & Nguyen, 2016) and may have important effects on the supply of marijuana used for recreational purposes purchased in the illegal drug market (Anderson et al., 2013). MMLs that allow non-specific pain as a qualifying health condition may promote recreational use rather than medical use as non-specific pain may be reported by, at best, marginal patients to access marijuana (Wen et al., 2015). Finally, requiring patients to register their marijuana use with the state (e.g., patients must register with the state to legally use marijuana) may deter non-medical users (Wen et al., 2015). Columns 3-6 in Table 1 provide the states that have passed each type of MML and the law effective date.

Results are reported in Table 8a. The coefficients are positive in all eight regressions. In terms of SSDI claiming, we find that passage of an MML that allows cultivation (home or group) and that includes non-specific pain as a qualifying condition lead to a statistically significant increase in SSDI claiming propensity by 0.35 percentage points (12.8%) and 0.40 percentage points (14.6%). On the other hand, MMLs that permit dispensaries are statistically linked with increased WC claiming: passage of such an MM leads to an 0.15 percentage point (14.0%) increse in WC claiming. However, we note that while the coefficients estimates vary across specifications, the 95% confidence intervals generally overlap.

A concern with our analysis of law heterogeneity is that the comparison group is not truly 'untreated'. For example, in regressions that include a control for an MML that permits operating dispensaries, the comparison group includes states that allow home cultivation,

[29] An MML feature that is potentially important for claiming is explicit protection for workers who use marijuana medically from being fired by their employer. Several states have passed an MML that confers such benefits (Hollinshead, 2013). These laws were passed very recently, 2012 or later, and we do not have sufficient post-law data to study the effects of this feature.

include non-specific pain as a qualifying health condition, and/or require patients to register with the state. A 'treated' comparison group may muddle interpretation of the estimated coefficients. Thus, we have re-estimated these regressions on the sample of states that are treated with a particular law feature and the 21 states that have not passed an MML as of 2018 as the comparison group. While the samples in these analyses may be selected, they do allow for an uncontaminated comparison group. Results, reported in Table 8b, are not appreciably different from the full sample results. An exception is that the coefficient estimate on cultivation in the SSDI regression is no longer statistically different from zero.

5 Robustness checks and extensions

5.1 Event study

A concern in analyses of public policies is that state legislatures, concerned with deteriorating health within the population, may implement policies to address these trends. In such a scenario, outcomes may lead to changes in policies rather than policies leading to changes in outcomes, a form of reverse causality at the state level. To explore this possibility, we estimate an event study (Autor, 2003). More specifically, we estimate a variant of Equation 1 in which we include in the regression model a series of variables for each time period before and after MML passage (policy leads and lags, respectively).

To construct our policy lags and leads we impose endpoint restrictions (McCrary, 2007; Kline, 2012): we assume that there are no anticipatory effects more than nine years in advance of the MML and that MML effects fade out after nine years post-MML.[30] We then construct indicators for each year pre- and post-MML. We omit the year prior to the law effective date. States that do not pass an MML by the end of our study period, 2013, are coded as zero for all indicators. In unreported analyses, we have excluded these states from the sample and results (available on request) are not appreciably different. Following Wolfers (2006), we exclude the state-specific linear time trends from the regression model. Event study results are reported in Table 9.

Overall the event study findings are in line with our main DD results. Some policy lead coefficient estimates in the WC regressions do rise to the level of statistical significance, however. The coefficient estimates that are precisely estimated carry a negative sign (i.e., three years in advance of MML passage). We argue that any anticipatory behavior on the

[30]Results are not sensitive to alternative endpoint restrictions.

part of states that may be reflected in these estimates works against our ability to detect effects in the DD model. Put differently, these lead estimates suggest that WC claiming is *declining* pre-MML while we find that MML passage either does not lead to changes in WC claiming or, in some specifications and samples, increases in such claiming. Examination of the policy lags estimates provides additional evidence that MML passage may lead to increases in claiming, but these effects appear to dissipate three to four years post-MML.

5.2 Alternative measures of SSDI claiming

In our main analyses we use SSDI data from 2001 to 2013 as we cannot distinguish between SSDI and other Social Security payments in earlier years. While focusing on the 2001-2013 period allows us to more accurately measure SSDI, we cannot leverage MML changes between 1996 and 2001 in these analyses. We next re-estimate Equation 1 using two alternative measures of SSDI over the full study period (1990-2013). (i) An indicator that captures any type of Social Security payments; this measure includes SSDI payments, Old-Age and Survivors Insurance payments, and such. (ii) We refine the measure defined in (i) by requiring that the respondent report Social Security payments (regardless of source) and report a work-limiting disability at the time the worker completed the ASEC. Requiring that the worker report a work-limiting disability may allow us to better capture SSDI payments (Burkhauser et al., 2014). We report results in Table 10. We also use data 2001-2013 and include income when a respondent reports own disability as the first, but not second, reason for receiving Social Security payments. Overall the estimated coefficient estimates generated using these alternative measures of SSDI claiming are comparable in sign to our main findings (positive). However, the magnitude of the estimated effects are smaller and the estimates are imprecise. We hypothesize the the null affects are attributable to the fact that we include non-SSDI claimants, who are not likely to alter their claiming post-MML, into our SSDI definition.

We also estimate our WC regressions on our main SSDI study period (2001-2013) to ensure that our null findings for WC are not driven by a specific time period. Results are reported in Table 11 and are not appreciably different from our main specification.

5.3 Alternative controls for between-state heterogeneity

In the analyses presented thus far we control for unobservable between-state heterogeneity through the use of state fixed effects and state-specific linear time trends. While a standard approach in policy analyses (Sabia et al., 2017; Horn, Maclean, & Strain, 2017), this

specification has some limitations. (i) If there are no time-varying unobservable state characteristics that are correlated with both a states' propensity to pass an MML and our claiming outcomes, then Equation 1 may 'throw away' variation in MML passage that could be used for identification. (ii) If state-specific linear time trends do not adequately control for the important sources of time-varying and unobservable state characteristics, then coefficients estimated in Equation 1 may be vulnerable to omitted variable bias.

To explore the implications of a possibly mis-specified regression model, we next estimate variants of Equation 1. More specifically, we (i) remove state-specific linear time trends, (ii) we include state-specific quadratic time trends, (iii) we include region-by-year fixed effects,[31] and (iv) we include additional time-varying state level controls (beer tax per gallon, cigarette tax per package, an indicator for whether or not the state has decriminalized marijuana, and the number of physicians).[32] Results generated in these alternative specifications are reported in Table 12. The results are broadly robust to these different approaches to controlling for between-state differences. However the magnitude and the precision of the estimates does vary across specification to some extent. We note that there are some complications in interpreting changes in coefficient estimates generated in non-linear models that include different sets of control variables (Norton, 2012).

5.4 Alternative classification of MMLs

We rely on a coding scheme developed by Sabia and Nguyen (2016) in our main analyses. However, policy scholars have proposed alternative coding schemes for MMLs (Pacula et al., 2015; Wen et al., 2015). Our review of these alternative coding schemes suggest that, while there is general agreement in terms of what states have passed an MML, there are non-trivial differences in the effective year for some states across these schemes (e.g., the state of Maryland).[33] We next re-estimate our regression models using coding schemes proposed by Pacula et al. (2015) and Wen et al. (2015). Results are reported in Tables 13. The coefficient estimates are similar across the alternative approaches to coding MML.

5.5 Benefit claiming levels

Thus far we have considered the extensive margin of claiming: whether or not an individual claims SSDI or WC. Next, we consider claiming levels: the amount of income respondents

[31] We use the four U.S. regions: Northeast, Midwest, South, and West.

[32] We convert the nominal taxes to real terms using the CPI. More details available on request.

[33] Details of the law comparison are available on request.

receive from SSDI or WC. We focus on the unconditional level of claiming as we are concerned that the conditional level may vulnerable to conditional-on-positive bias as we have shown that MMLs influence the composition of claimants (Angrist & Pischke, 2009). The SSDI and WC level variables are highly left-skewed. To account for skewness in the benefit level distributions, we use a Poisson generalized linear model (GLM) following a procedure outlined by Manning and Mullahy (2001).[34] Results are reported in Table 14. The results suggest that passage of an MML leads to a $46 (13.8%) increase in SSDI income and a $13 (15.2%) increase in WC income.

5.6 Smuggling

Patients must provide evidence of residence to obtain medical marijuana in all states that have passed an MML. However, individuals living in one of the 21 states that has not passed an MML may be able to obtain marijuana illegally if they reside near a state that has passed an MML. Cross-boarder effects have been documented in the case of other addictive goods such as alcohol and cigarettes (Lovenheim, 2008; Lovenheim & Slemrod, 2010). Our core models do not permit the possibility of such cross-boarder smuggling and we next test the effect of such behavior on our estimates. In particular, we include an additional variable in Equation 1 that takes a value of 1 if the state boarders a state with an MML and zero otherwise. Results are reported in Table 15. The point estimates in the models that control for cross-border smuggling are nearly identical to the main results.

5.7 Health outcomes

One possible pathway through which passage of an MML could influence the claiming outcomes we study is better management of symptoms associated with chronic health conditions. On the one hand, medical marijuana offers a new treatment to patients. If marijuana is an effective treatment, or is equally effective but offers a less burdensome side effects, passage of an MML may allow better symptom management and less need for SSDI or WC. Alternatively, if marijuana accessed via passage of an MML is used recreationally or if medical marijuana is not an effective treatment or has other health-harming attributes, passage of an MML may lead to worsening health.

We next study the effects of MML passage on two measures of health: the probability

[34]Specifically, we apply the modified Park test outlined by Manning and Mullahy. The results of this test implied that this model was most appropriate. Details available on request.

of a work-limiting disability and self-assessed health (an indicator for reporting excellent or very good health vs. poor, fair, or good). Results are reported in Table 16. We find no evidence that passage of an MML leads to changes in the probability of these outcomes: the coefficient estimates are small in magnitude and imprecise. The null finding for a work-limiting disability is not entirely surprising as medical marijuana, at best, can improve symptoms associated with health conditions, but is unlikely to affect health *per se*. Hence, respondents, even those whose symptoms are better managed with medical marijuana than other medical treatments, are unlikely to change their reporting of work-limiting disabilities as their underlying health condition(s) is not changed from marijuana use. We note that our null finding departs from Nicholas and Maclean (2016) who show that passage of an MML improves self-assessed among older adults. Collectively, these findings imply that MMLs have heterogeneous effects on self-assessed health.

6 Discussion

In this study we explore the effects of state medical marijuana laws (MMLs) on Social Security Disability Insurance (SSDI) and Workers' Compensation (WC) claiming. We find that passage of an MML increases SSDI claiming. In particular, post-MML the propensity to claim SSDI increases by 0.31 percentage points (11.3%). We find no statistically significant evidence that passage of an MML leads to changes in WC claiming, although coefficient estimates are positive. Results are stable across numerous robustness checks.

The effects that we estimate in our models are intent-to-treat and capture the net effect of MML passage on benefit claiming. As noted earlier in the manuscript, the net effect of an MML passage on our outcomes is an empirical question. There is likely a complicated set of pathways through which MML passage will influence claiming. These pathways vary across individuals who, due to an MML passage, opt to use marijuana (e.g., for medical or recreational purposes). For those individuals who use marijuana medically post-MML, the extent to which use of this medication helps or harms health will be determined by the particular health condition for which marijuana is used to treat, the patients' previous and concurrent treatment, and heterogeneity in how patients respond to different medications (Porter, 2010). Among those individuals who use the passage of an MML as a pathway to obtain marijuana for recreational purposes, the extent to which claiming is affected will be determined through different pathways. Overall, without speaking to the specific pathways, we find that MMLs lead to increases in SSDI and WC claiming among working age adults.

While our Current Population Survey data does not allow us to test pathways, we hypothesize that our findings are plausibly driven by the work-impeding side effects of marijuana used medically and through recreational use of marijuana, and that medical marijuana may be less effective in treating symptom burden among marginal claimants. As datasets that allow researchers to isolate medical use from recreational use become available, it will be interesting to revisit this question to better understand the specific pathways through which marijuana obtained through MMLs influences SSDI and WC claiming.

We can compare our intent-to-treat estimates with findings from the literature to assess whether or not our effect sizes appear to be of reasonable magnitude. (i) Wen et al. (2015) show that passage of an MML leads to a 1.32 percentage point (14%) increase in any past month marijuana use and a 0.58 percentage point (15%) increase in near daily use. Based on these estimates, one could argue that our effect sizes are plausible. For example, our findings suggest that an MML passage leads to a 0.31 percentage point increase in SSDI claiming, which is well below the absolute effect sizes estimated by Wen and colleagues. (ii) We can examine the share of the relevant population that uses marijuana. Using survey data from the National Survey of Drug Use and Health Azofeifa (2016) shows that 8.4% of U.S. residents 21 years and older reported any form of marijuana use in the past month in 2014. While these assessments do not provide definitive evidence that our effect sizes are reasonable, collectively they suggest that our estimates are not outrageously large.

While our study is novel in several ways, it is not without limitations. (i) We rely on survey data and there may be some reporting error in our claiming variables due to, for example, stigma associated with the use of social services. (ii) We lack data on marijuana use and therefore cannot estimate a 'first stage' regression. (iii) Our SSDI variable can only be reliably measured from 2001 onward, thus we are not able to incorporate all MML changes into our analysis of this outcome. (iv) As discussed above, we estimate intent-to-treat models when ideally we would also like to provide evidence on the treatment-on-treated. However, as we note earlier, an ITT estimate is a useful object as the MML, and not other features of marijuana use, is the lever available to policymakers.

Overall, the literature on the labor market effects of MMLs presents a quandary for policymakers. On the one hand, Nicholas and Maclean (2016) find that passage of such laws increases labor supply among older workers. On the other hand, Sabia and Nguyen (2016) find no evidence that MMLs enhance labor market outcomes, as measured by labor supply or wages, among working age populations. Indeed, among younger males, passage of an MML may reduce wages. Thus, the effect of MML varies across outcomes and populations.

Our study adds important insight on labor market effects: expanding marijuana access has negative spillover effects to costly social programs that dis-incentivize work.

Our findings add to the growing literature that evaluates the overall effects of expanded access to medical marijuana through MMLs. This literature documents that such expansions in access lead to both benefits and costs. Policy makers must carefully review this body of literature and determine how to make the most responsible decisions for their constituents. The optimal choice likely varies across states based on state preferences, demographics, underlying health status, labor market conditions, and so forth. Finally, 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 et al., 2005; Duggan et al., 2007; McInerney & Simon, 2012; Reich & West, 2015; Bradford & Bradford, 2016, 2017; Hudson & Moriya, 2017). 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.

Table 1: State medical marijuana laws 1996-2013

State	MML	MML Provisions			
		Cultivation	Dispensary	Non-specific pain	Registry
	(1)	(2)	(3)	(4)	(5)
Alaska	3/1999	n/a	n/a	3/1999	3/1999
Arizona	4/2011	4/2011	12/2012	4/2011	4/2011
California	11/1996	11/1996	11/1996	11/1996	n/a
Colorado	6/2001	6/2001	7/2005	6/2001	6/2001
Connecticut	5/2012	n/a	8/2014	n/a	5/2012
DC	7/2010	n/a	7/2013	n/a	7/2010
Delaware	7/2011	n/a	n/a	7/2011	7/2011
Hawaii	12/2000	n/a	n/a	12/2000	12/2000
Maine	12/1999	n/a	4/2011	n/a	12/2009
Massachusetts	1/2013	n/a	n/a	n/a	1/2013
Michigan	12/2008	12/2008	12/2009	12/2008	n/a
Montana	11/2004	11/2004	4/2009	11/2004	n/a
Nevada	10/2001	10/2001	n/a	10/2001	10/2001
New Hampshire	7/2013	n/a	n/a	7/2013	7/2013
New Jersey	10/2010	n/a	12/2012	10/2010	10/2010
New Mexico	7/2007	n/a	6/2009	n/a	7/2007
Oregon	12/1998	12/1998	11/2009	12/1998	1/2007
Rhode Island	1/2006	1/2006	4/2013	1/2006	1/2006
Vermont	7/2004	n/a	6/2013	7/2007	7/2004
Washington	11/1998	7/2011	4/2009	11/1998	n/a

Notes: Data source: Sabia and Nguyen (2016) and ProCon (http://medicalmarijuana.procon.org/view.resource.php?resourceID=000881; accessed August 2nd, 2017). We note that the following states passed MMLs after 2013: Arkansas (2016), Florida (2017), Illinois (2014), Maryland (2014), Minnesota (2014), New York (2014), North Dakota (2016), Ohio (2016), Pennsylvania (2016), and West Virgina (2017).

Table 2: Summary statistics: ASEC 1990 to 2013

Sample:	All states	MML states	Non-MML states
Outcome variables			
Any SSDI income	0.0274	0.0244	0.0292
Any WC income	0.0107	0.0122	0.00971
Control variables			
MML	0.162	0.427	0
Age	41.26	41.17	41.32
Male	0.480	0.483	0.479
Female	0.520	0.517	0.521
White	0.821	0.809	0.828
African American	0.106	0.0763	0.125
Other race	0.0729	0.115	0.0472
Hispanic	0.147	0.198	0.117
Non-Hispanic	0.853	0.802	0.883
Less than high school	0.155	0.159	0.153
High school	0.291	0.270	0.305
Some college	0.281	0.282	0.281
College graduate	0.272	0.289	0.262
Unemployment rate	0.0501	0.0552	0.0470
Hourly wage	20.72	22.07	19.89
Poverty rate	13.00	12.57	13.26
Minimum wage	7.275	7.691	7.020
EITC state-to-federal ratio	0.0482	0.0508	0.0465
TANF	658.7	818.3	560.9
PDMP	0.548	0.586	0.524
Democrat governor	0.453	0.487	0.432
Population	9,959,131	1,1382,219	9,086,621
Observations	2,243,528	852,719	1,390,809

Table 3: Characteristics of SSDI and WC benefit claimants

Sample by claimant status:	SSDI claim	WC claim	No claiming
Age	49.22	42.02	41.14
Male	0.492	0.618	0.478
Female	0.508	0.382	0.521
White	0.735	0.837	0.819
African American	0.194	0.103	0.105
Other race	0.071	0.060	0.075
Hispanic	0.107	0.151	0.151
Non-Hispanic	0.893	0.849	0.849
Less than high school	0.244	0.228	0.135
High school	0.393	0.365	0.298
Some college	0.265	0.306	0.274
College graduate	0.097	0.101	0.293
Weeks worked last year	3.77	31.01	40.09
Personal wage & salary income last year ($)	864.67	19,161.29	30,364.92
Work limiting disability	0.859	0.388	0.050
Observations	38,906	23,927	2,180,695

Table 4: Test of pre-implementation trends in SSDI and WC outcomes

Outcome:	Any SSDI	Any WC
Sample proportion:	0.0274	0.0107
$Treat_s * Trend_{st}$	0.0028	0.0000
	(0.0034)	(0.0021)
Observations	652,262	1,456,179

Notes: Sample proportions of the outcomes are based on the full sample. All models estimated with OLS and control for personal characteristics, state characteristics, state fixed effects, and time-to-event fixed effects. 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 an MML on SSDI and WC claiming

Outcome:	Any SSDI	Any WC
Sample proportion:	0.0274	0.0107
Any MML	0.0031**	0.0008
	(0.0016)	(0.0006)
Observations	1,421,399	2,243,528

Notes: All models estimated with a probit model (average marginal effects reported) and control for personal characteristics, state characteristics, state-specific linear time trends, state fixed effects, and year fixed effects. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 6: Effect of and MML on SSDI and WC claiming by age

Outcome:	Any SSDI	Any WC
Younger workers: 23-40 years		
Sample proportion:	0.0116	0.0113
Any MML	0.0032**	0.0019***
	(0.0016)	(0.0007)
Observations	656,952	1,095,590
Older workers: 41-62 years		
Sample proportion/mean:	0.0409	0.0113
Any MML	0.0027	-0.0000
	(0.0019)	(0.0007)
Observations	764,447	1,147,938

Notes: All models estimated with a probit model (average marginal effects reported) and control for personal characteristics, state characteristics, state-specific linear time trends, state fixed effects, and year fixed effects. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 7: Effect of an MML on SSDI and WC claiming by sex

Outcome:	Any SSDI	Any WC
Men		
Sample proportion:	0.0281	0.0137
Any MML	0.0036**	0.0014*
	(0.0018)	(0.0008)
Observations	681,356	1,077,479
Women		
Sample proportion:	0.0267	0.0078
Any MML	0.0025	0.0003
	(0.0018)	0.0007)
Observations	740,043	1,166,049

Notes: All models estimated with a probit model (average marginal effects reported) and control for personal characteristics, state characteristics, state-specific linear time trends, state fixed effects, and year fixed effects. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 8a: Effect of an MML on SSDI and WC claiming by law features

Outcome:	Any SSDI	Any WC
Sample proportion:	0.0274	0.0107
Cultivation:		
Cultivation	0.0035*	0.0004
	(0.0020)	(0.0006)
Observations	1,421,399	2,243,528
Dispensaries:		
Dispensaries	0.0031	0.0015*
	(0.0027)	(0.0009)
Observations	1,421,399	2,243,528
Non-specific pain:		
Non-specific pain	0.0040**	0.0009
	(0.0017)	(0.0006)
Observations	1,421,399	2,243,528
Registry:		
Registry	0.0016	0.0002
	(0.0011)	(0.0011)
Observations	1,421,399	2,243,528

Notes: All models estimated with a probit model (average marginal effects reported) and control for personal characteristics, state characteristics, state-specific linear time trends, state fixed effects, and year fixed effects. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 8b: Effect of an MML on SSDI and WC by law features: Control includes states that do not pass an MML

Outcome:	Any SSDI	Any WC
Cultivation:		
Sample proportion:	0.0265	0.0105
Cultivation	0.0031	0.00005
	(0.0020)	(0.00006)
Observations	1,195,999	1,889,718
Dispensaries:		
Sample proportion:	0.0268	0.0106
Dispensaries	0.0029	0.0017**
	(0.0026)	(0.0008)
Observations	1,377,746	2,180,887
Non-specific pain:		
Sample proportion:	0.0265	0.0107
Non-specific pain	0.0033**	0.0008
	(0.0016)	(0.0005)
Observations	1,335,321	2,102,737
Registry:		
Sample proportion:	0.0274	0.0101
Registry	0.0149	0.0004
	(0.0010)	(0.0011)
Observations	1,244,981	1,947,233

Notes: Alternative control group = states that have not passed any MML (see Table 1). Sample proportions vary across specifications as the sample changes across specifications. All models estimated with a probit model (average marginal effects reported) and control for personal characteristics, state characteristics, state-specific linear time trends, state fixed effects, and year fixed effects. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 9: Effect of an MML on SSDI and WC claiming using an event study model

Outcome:	Any SSDI	Any WC
Sample proportion:	0.0274	0.0107
-9	0.0013	0.0007
	(0.0017)	(0.0007)
-8	0.0024	0.0006
	(0.0023)	(0.0009)
-7	0.0025	-0.0005
	(0.0015)	((0.0005)
-6	0.0028	-0.0002
	(0.0020)	(0.0007)
-5	0.0014	-0.0005
	(0.0017)	(0.0005)
-4	0.0002	-0.0003
	(0.0013)	(0.0007)
-3	0.0002	-0.0015**
	(0.0014)	(0.0006)
-2	0.0006	-0.0008
	(0.0017)	(0.0006)
0	0.0023	-0.0001
	(0.0015)	(0.0007)
+1	0.0032**	-0.0003
	(0.0013)	(0.0008)
+2	0.0017	0.0006
	(0.0016)	(0.0010)
+3	0.0017	0.0002
	(0.0011)	(0.0008)
+4	0.0013	0.0007
	(0.0014)	(0.0008)
+5	0.0012	-0.0001
	(0.0012)	(0.0008)
+6	-0.0005	-0.0008
	(0.0013)	(0.0011)
+7	0.0016	0.0005
	(0.0020)	(0.0013)
+8	0.0006	-0.0006
	(0.0016)	(0.0011)
+9	-0.0004	-0.0001
	(0.0011)	(0.0006)
Observations	1,421,399	2,243,528

Notes: All models estimated with a probit model (average marginal effects reported) and control for personal characteristics, state characteristics, state fixed effects, and year fixed effects. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 10: Effect of an MML on alternative definitions of SSDI outcomes

Outcome:	No WLD	With WLD	First & second SS payments
Sample proportion/mean:	0.0391	0.0246	0.0266
Any MML	0.0014	0.0006	0.0026
	(0.0012)	(0.0009)	(0.0015)
Observations	2,243,528	2,243,528	1,421,399

Notes: WLD = work-limiting disability. SS = Social Security. See text for more details on the alternative definitions of SSDI. All models estimated with a probit model (average marginal effects reported) and control for personal characteristics, state characteristics, state-specific linear time trends, state fixed effects, and year fixed effects. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 11: Effect of an MML on WC claiming: 2001-2013

Outcome:	Any WC
Sample proportion:	0.0083
Any MML	-0.0008
	(0.0010)
Observations	1,421,399

Notes: All models estimated with a probit model (average marginal effects reported) and control for personal characteristics, state characteristics, state-specific linear time trends, state fixed effects, and year fixed effects. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 12: Effect of an MML on SSDI and WC claiming using alternative sets of controls for between state heterogeneity

Outcome:	Any SSDI	Any WC
Sample proportion:	0.0274	0.0107
State FE and year FE	0.0011	0.0003
	(0.0016)	(0.0006)
State FE, year FE, and state quadratic trends	0.0040**	0.0013
	(0.0016)	(0.0008)
State FE, year FE, and region-by-year FE	0.0018	-0.0000
	(0.0013)	(0.0005)
Additional state level controls	0.0026	0.0249
	(0.0016)	(0.0157)
Observations	1,421,399	2,243,528

Notes: FE = fixed effects. All models estimated with a probit model (average marginal effects reported) and control for personal characteristics, state characteristics, state fixed effects, and year fixed effects. Additional state level controls include beer tax, cigarette tax, indicator for marijuana decriminalization, and number of physicians. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 13: Effect of an MML on SSDI and WC claiming using alternative MML coding schemes

Outcome:	Any SSDI	Any WC
Sample proportion:	0.0274	0.0107
Any MML	0.0033**	0.0007
Pacula et al.	(0.0014)	(0.0007)
Any MML	0.0031**	0.0007
Wen et al.	(0.0016)	(0.0006)
Observations	1,421,399	2,243,528

Notes: All models estimated with a probit model (average marginal effects reported) and control for personal characteristics, state characteristics, state-specific linear time trends, state fixed effects, and year fixed effects. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 14: Effect of an MML on levels of benefit income

Outcome:	SSDI	WC
Sample mean:	330.8	87.09
Any MML	45.5773**	13.1943**
	(20.4280)	(6.4827)
Observations	1,421,399	2,243,528

Notes: All models estimated with a generalized linear model and control for personal characteristics, state characteristics, state-specific linear time trends, state fixed effects, and year fixed effects. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 15: Effect of an MML on SSDI and WC outcomes controlling for cross-state smuggling

Outcome:	Any SSDI	Any WC
Sample proportion:	0.0274	0.0107
Any MML	0.0031**	0.0008
	(0.0016)	(0.0006)
Observations	1,421,339	1,421,339

Notes: All models estimated with a probit model (average marginal effects reported) and control for personal characteristics, state characteristics, state-specific linear time trends, state fixed effects, and year fixed effects. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table 16: Effect of an MML on health outcomes

Outcome:	WLD	SAH
Sample proportion:	0.0758	0.5062
Any MML	0,0008	-0.0013
	(0.0009)	(0.0030)
Observations	2,243,528	1,765,135

Notes: WLD=work limiting disability. SAH= very good or excellent self-assessed health. All models estimated with a probit model (average marginal effects reported) and control for personal characteristics, state characteristics, state-specific linear time trends, state fixed effects, and year fixed effects. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

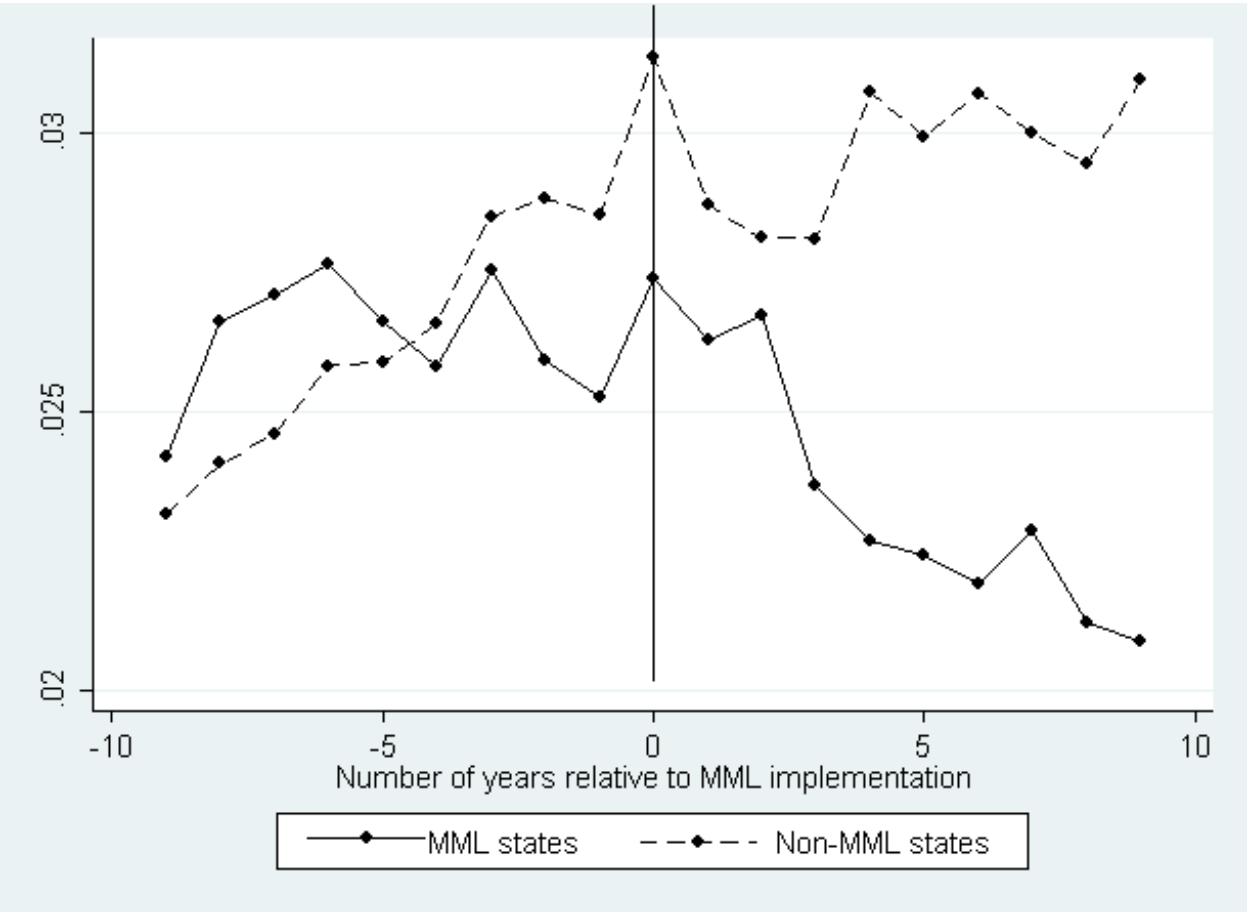

Figure 1: Trends in SSDI proportions

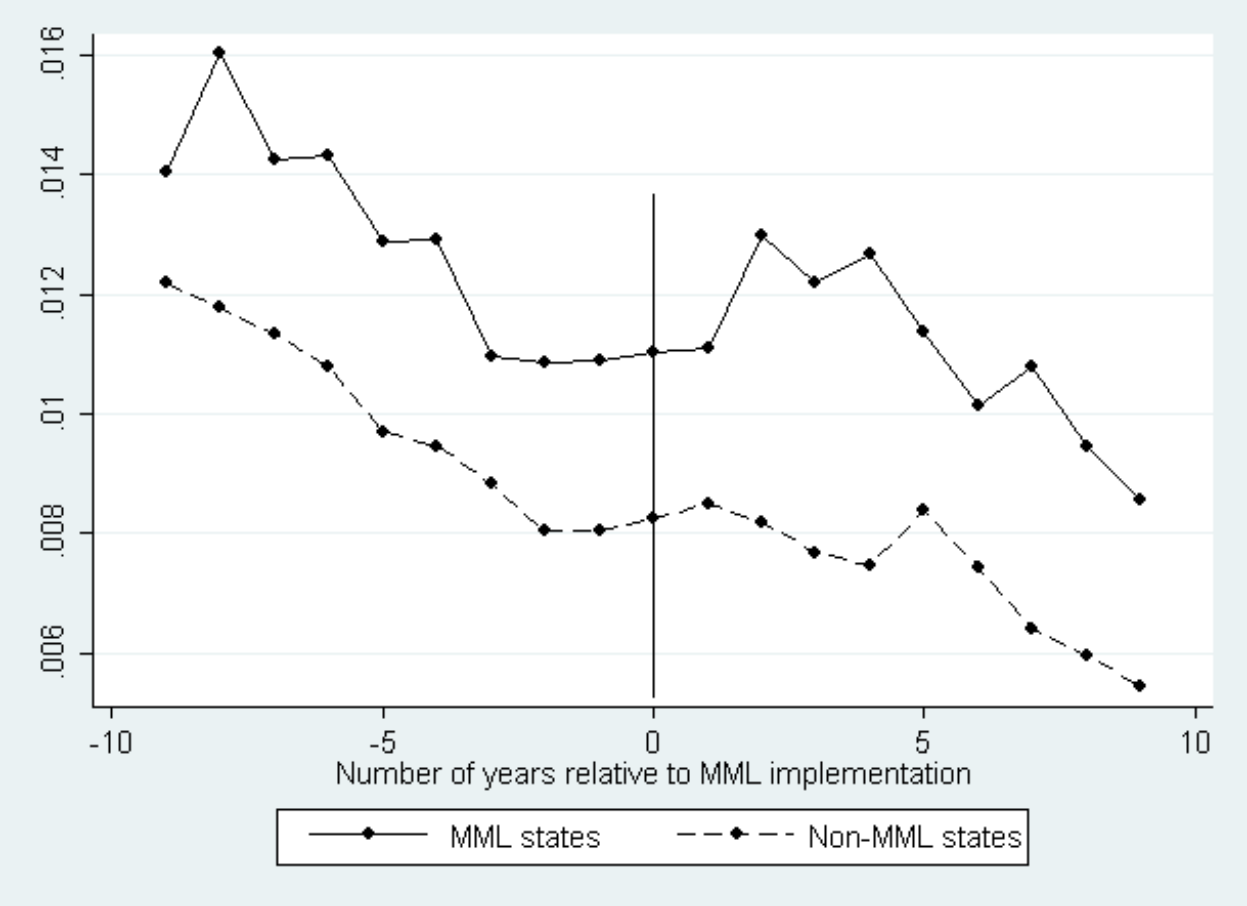

Figure 2: Trends in WC proportions

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