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Johanna Catherine Maclean
Keshar M. Ghimire
Lauren Hersch Nicholas

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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. We use data on benefit claiming drawn from the 1990 to 2013 Current Population Survey coupled with a differences-in-differences design. We find that passage of an MML increases SSDI, but not WC, claiming on both the intensive and extensive margins. Post-MML the propensity to claim SSDI increases by 0.27 percentage points (9.9%) and SSDI benefits increase by 2.6%. We identify heterogeneity by age and the manner in which states regulate medical marijuana. Our findings suggest an unintended consequence of MMLs: increased reliance on costly social insurance programs among working age adults.

Johanna Catherine Maclean
Department of Economics
Temple University
Ritter Annex 869
Philadelphia, PA 19122
and NBER
catherine.maclean@temple.edu

Keshar M. Ghimire
Business and Economics Department
University of Cincinnati - Blue Ash
9555 Plainfield Road
Blue Ash, OH 45236
USA
ghimirkr@ucmail.uc.edu

Lauren Hersch Nicholas
Johns Hopkins School of Public Health
624 N Broadway, Room 450
Baltimore, MD 21205
USA
lauren.nicholas@jhu.edu

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 disincentivize 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.[4]

Beginning with California in 1996, U.S. states have implemented laws that legalize the use of marijuana for medical purposes ('MMLs') among 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 2017, 28 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

[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.

[4] Previous economic research, outlined later in the manuscript, documents that state medical marijuana laws can have spillover effects to public health 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) and the effect of raising the retirement age on SSDI rolls (Duggan, Singleton, & Song, 2007).

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 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 (Hill, 2015; Whiting et al., 2015; Joy, Watson, & Benson, 1999; Lynch & Campbell, 2011). In survey settings, patients state that they use medical marijuana to manage symptoms related to health conditions (Nunberg, Kilmer, Pacula, & Burgdorf, 2011; Troutt & DiDonato, 2015). Further, the majority of medical marijuana 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).

Many of the health conditions that can qualify a patient for legal access to medical marijuana through state MMLs are also health conditions that could lead to an SSDI or WC claim. 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).

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 health, 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 on reported benefit claiming between 1990 and 2013 and estimate differences-in-differences regression models. We consider both the extensive (i.e., any claiming) and intensive (i.e., level of benefits) margins. We also explore heterogeneity across age as health policy scholars argue that younger individuals are more likely to use marijuana obtained through MMLs for recreational, and not medical, purposes than older individuals (Anderson, Hansen, & Rees, 2013; Pacula, Powell, Heaton, & Sevigny, 2015; Wen, Hockenberry, & Cummings, 2015; Sabia & Nguyen, 2016). Finally, we examine whether the manner in which a state chooses to regulate medical marijuana through its MML affects benefit claiming.

We find that passage of an MML increases SSDI claiming on both the intensive and extensive margins. In particular, post-MML the propensity to claim SSDI increases by 0.27 percentage points (9.9%) and SSDI benefits increase by 2.6% in the full sample. We find no statistically significant evidence that passage of an MML leads to changes in WC claiming in the full sample, although coefficient estimates are positive which is suggestive of MML passage leading to an increase in such claiming. We identify heterogeneity across younger (23 to 40 years) and older (41 to 62 years) adults: passage of an MML increases both SSDI and WC claiming among younger, but not older, adults. When we examine MML features, we find that laws permitting non-specific pain as a qualifying health condition and that allow dispensaries (venues at which patients can legally purchase medical marijuana) increase benefit claiming. Our results are broadly robust across a range of specifications.

These results contribute to a growing literature highlighting the relationship between specific medical treatments, and use of sick leave and disability insurance. Previous work documents 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 decreases when workers obtain access to anti-retroviral treatments (Habyarimana, Mbakile, & Pop-Eleches, 2010) and minimally invasive surgery (versus more aggressive elective procedures) (Epstein, Groeneveld, Harhay, Yang, & Polsky, 2013).

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

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, including both mental and physical disabilities. 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'),[5] (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; although this final requirement can be waived in some cases (Social Security Administration, 2016). 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. If an initial claim is denied, the applicant has the option to appeal the denial. The time period between the initial SSDI application and the final decision (either an approval or a denial) can extend from six months to several years depending on the number of appeals. In addition to receiving income, successful applicants are also eligible for Medicare Part 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. For instance, Autor and Duggan (2006) show that the share of working age adults (25 to 64 years) claiming SSDI increased from 2.2% in 1985 to 4.1% in 2005, representing an 87% increase. This increase in claiming occurred while there was no corresponding

[5]The amount of earnings considered to be SGA varies by disability. For example, according to the SSA, in 2017, the minimum monthly SGA earnings requirement for non-blind workers is $1,170 and $1,950 for blind workers (these earnings are net of impairment-related work expenses).

increase in self-reported disability within the working age adult population (Duggan & Imberman, 2009). Explaining this apparent paradox, rising SSDI rolls without a corresponding rise in self-reported disability, has received substantial attention from economists. Autor and Duggan (2006) document several factors that drove these substantial increases in SSDI rolls: (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, 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. Employers can make accomodations for such workers and such workers may be eligible for permanent partial disability benefits.

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

[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.

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, longshore 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 has 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 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 these benefits regardless of who was at fault in the accident leading to the injury or illness. In return for these guaranteed benefits, workers who receive WC 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] Of course 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.[13] 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.

As of August 2017, 28 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). As noted earlier in the manuscript, many of these conditions overlap with conditions that qualify a worker for SSDI and WC benefits, offering premise for our study.[14] We list the effective date for each state MML through 2013 in column 1 of Table 1. We leverage law effective dates collected by Sabia and Nguyen

[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] Indeed, the breadth of the list of health conditions was of concern to policymakers: AIDS, anorexia, arthritis, cancer, chronic pain, glaucoma, migraines, spasticity, or any other illness for which marijuana provides relief. For example, Senator Diane Feinstein was quoted as stating in regards to the Act broadly 'You'll be able to drive a truckload of marijuana through the holes in it. While it seems simple, the devil is in the details or, in this particular bill, the lack of details.' (http://www.latimes.com/health/la-oew-gutwillig-imler6-2009mar06-story.html; accessed August 15th, 2017.)

[14] We note that, while not the focus of our study, as of Septmeber 2017 seven states (AK, CA, CO, ME, MA, NV, OR, and WA) and DC have passed legislation to legalize recreational use of marijuana.

(2016). In our main analysis we focus on the passage of any MML, regardless of the law specifics. However, in extensions we consider heterogeneity across laws.

2.3 Economic evidence on the effects of state MMLs

The economic MML literature is substantial. 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 among adults.

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). The authors note, however, that passage of an MML that allows for dispensaries increases marijuana-related admissions to substance abuse treatment while passage of an MML that requires patients to register with the state reduces such admissions. 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.

In addition to influencing use of marijuana, MMLs also influence use of other drugs and alcohol. Anderson et al. (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 on the use of cocaine and heroin, and finds that drug possession arrests and admissions to substance

[15] We note that economists have also studied MMLs in the context of youth and young adults (Anderson, Hansen, & Rees, 2015; Pacula et al., 2015; Wen et al., 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. Thus, the literature that seeks to estimate the causal effects of MMLs on marijuana use using the above-noted methods faces this barrier.

abuse treatment related to these substances fall post-MML (although the coefficient estimates are often close to zero). 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%. In a complimentary study 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 adults, reported pain declines and general health status increases following passage of an MML. However, depressive symptoms increase post-MML among older men, suggesting a complex relationship between MMLs and older adult health. 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). Finally, in a recent study, heart attack-attributable deaths have been shown to increase, post-MML, in states that pass an MML relative to states that do not (Abouk & Adams, 2017).

Economic evidence further suggests that patients are using marijuana medically to treat symptoms associated with a wide range of health conditions, some 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 and adults suffering from a small set of serious illnesses (e.g., end-stage renal disease). For example, following passage of an MML, prescriptions for anxiety medications decline by 5% (Bradford & Bradford, 2016).[17]

Three studies investigate the impact of MML implementation on labor market outcomes. (i) Using data drawn from the Current Population Survey (CPS) Sabia and Nguyen (2016) conclude that passage of an 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

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

outcomes examined by the authors.[18] (ii) Ullman (2016) uses the CPS and shows that passage of an MML reduces work absences. (iii) Nicholas and Maclean (2016) focus on older workers, 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 probability of working among older adults, however.

Overall, the available economic literature suggests that passage of an MML can influence marijuana use and the use of other substances, health, 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.

We argue that it is important to consider SSDI and WC claiming separately from more standard labor market outcomes previously considered in the literature for at least three reasons. (i) As we document later in the manuscript, individuals who do and do not claim SSDI and/or WC are very different in terms of their labor market outcomes, accumulated human capital, and health stock. Thus, these groups may receive different benefits from medical marijuana use and face different costs in terms of labor market returns from recreational use of marijuana and/or the adverse effects of medical marijuana use. (ii) To allow economics to better inform public policy broadly we must understand how different policies interact. (iii) Given the high costs of SSDI and WC for governments and employers, understanding factors (policies or otherwise) that influence the number of claimants is important for estimating program costs and devising approaches to curtail non-legitimate claims. This information (without strong and likely untenable assumptions) cannot be gleaned from the available literature that has focused on standard measures of labor market participation and returns. 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.[19]

MMLs, by increasing access to medical marijuana, could affect claiming by influencing symptoms associated with health conditions that qualify workers for SSDI and WC. Indeed,

[18] Any work and conditional hours worked per week.

[19] We note that it is not possible to fully separate mechanisms for the two types of marijuana uses, however.

there is 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 (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 concurrent 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. Nicholas and Maclean (2016) provide a detailed discussion on the various ways in which substitution away from conventional healthcare and toward medical marijuana may influence symptom burden among patients. We briefly summarize this discussion here. 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 substantial heterogeneity in the effectiveness of any 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 benefit need.[20] 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.[21] We cannot rule out the possibility that MML-attributable substance abuse problems may exacerbate other health conditions which may lead to an SSDI claim (e.g., mental health conditions). Moreover,

[20] We note the possibility, based on the available literature, that marijuana may be a substitute for other substances. If true, then MML passage may reduce other substance use which could improve health and reduce claiming.

[21] 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.

if workers are intoxicated by marijuana used 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, amplifying the effects of MML passage on claiming. Finally, if MMLs lead to reduced productivity, and in turn lower wages (Sabia & Nguyen, 2016), then some marginal 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. Our objective in this study is to estimate the net effect of MMLs on 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).[22] The ASEC interviews approximately 150,000 U.S. residents age 15 years and older each year on a wide range of labor market, income, and health insurance outcomes in the month of March. The ASEC is well-suited to our research question as it offers detailed information on both SSDI and WC claiming. Indeed, 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 focus on individuals ages 23 to 62 years.

The ASEC does not include information on marijuana use, either for recreational or medical purposes. 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 of the effect of MML passage on claiming. Instead, our estimates are intent-to-treat (ITT). We return to the ITT nature of our estimates later in the manuscript.

[22] 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 subset of the income questions.

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. Using this information, we construct a variable coded one in state/year pairs with an MML in place and coded zero otherwise. ASEC respondents are interviewed in March, but income information (including the information on benefit claiming that we leverage in our study) pertains to the previous calendar year ('income year'). We match state MMLs to the *income* year, which is one year prior to the *survey* year.[23] Thus, our study examines SSDI and WC claiming over the period 1989 to 2012. Given that the SSDI application process can take several years, in unreported analyses we have lagged the MML variable one year and two years. Results using the lagged MMLs, which are available on request, are not appreciably different from those reported here.

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. 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 four past year claiming measures: (i) any SSDI claiming, (ii) the level of SSDI benefits (unconditional), (iii) any WC claiming, and (iv) the level of WC benefits (unconditional).[24] We convert the level of benefit variables to 2013 dollars using the Consumer Price Index. We take the logarithm of SSDI and WC benefits. This transformation implies that regression coefficient estimates have the interpretation of an approximation to the percent change in our benefit level regressions.[25]

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

[24]We include all forms of WC in our analysis of this benefit. We do not report the conditional benefit levels. If MMLs influence the probability of claiming, then examination of the conditional sample of claimants can lead to conditional-on-positive bias.

[25]We add one to all observations. In unreported analyses, we examined histograms and skewness of both the logged and unlogged benefit variables. The unlogged variables, in particular WC benefits, were highly left skewed and not suitable for LS regressions. Thus, we chose to use the logged benefit variables in our analysis. However, results are qualitatively the same if we instead use the unlogged variables, although estimates are somewhat more precise. See Table A2. Moreover, in unreported analyses that are available on request, we have explored the sensitivity of our results to adding alternative small values (i.e., 0.1 and 0.5) to each observation before taking the log transformation. Results do not appear to be sensitive to these alternative approaches to addressing zero values in our benefit level variables.

Our SSDI benefit claiming variables have limitations. Specifically, the SSDI variables are derived from an overall survey item on Social Security benefits receipt for the respondent herself or as combined benefits received by the respondent and family members. Such benefits may include SSDI and other forms of Social Security benefits (e.g., Old-Age and Survivors Insurance). We wish to study SSDI benefits for the respondent only. To isolate respondent-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 benefits. One of the possible reasons for receiving these benefits is a respondent's own disability. In our main analyses we use data from 2001 to 2013 and include only those adults who report disability as their first or second source of Social Security benefits in our classification of SSDI claiming.[26]

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 benefits and (ii) requiring that the respondent report both Social Security benefits (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 benefits (Burkhauser et al., 2014). Finally, we use data from 2001 to 2013 and construct similar 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 benefits.

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 (African American and other race with white as the omitted category), Hispanic ethnicity, and educational attainment (high school, some college, and a college degree with less than high school as the omitted category).[27] 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

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

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

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)[28] and the poverty rate (University of Kentucky Center for Poverty Research, 2016). 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 monthly 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 (2017) 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 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 following differences-in-differences (DD) regression model:

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

B_{jst} is a benefit outcome for individual j in state s in year t. The MML_{st} is an indicator 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 linear probability models (LPM) for binary outcomes[29] and least squares for continuous outcomes. However, results are robust to alternative functional forms: probit models for binary outcomes and Poisson models for continuous outcomes (see Tables A1 and A2). We cluster the standard errors around the state (Bertrand, Duflo, & Mullainathan, 2004). All results are unweighted. Results generated in models weighted by ASEC survey

[28]Source: CEPR Uniform Data Extracts (`http://ceprdata.org/`; accessed June 1st 2017).

[29]We choose to use LPMs as non-linear models such as probits and logits are vulnerable to the incidental parameters problem in the presence of fixed effects (Greene, 2004). Moreover, there are difficulties in comparing parameter estimates across non-linear regression models with different sets of covariates (Norton, 2012) and in robustness checking we use different controls for between-state differences.

weights are similar (see Table A3).

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 SSDI with an average (unconditional) benefit of $331. In terms of WC, 1.1% of the sample reports receiving income from this source with an average (unconditional) benefit of $87. 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% of the sample has a state MML in place. The individual-level and state-level characteristics are in line with a working age U.S. sample. 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 states that pass an MML. We observe the same pattern when we examine the unconditional level of income derived from these sources across MML passing and not-passing states.

Before proceeding to the main regression analysis, we report demographic characteristics of individuals who report receiving SSDI and WC benefits in Table 3. On average, 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, less educated, and less likely to be married 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 from wages and salary ($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.[30] In terms of claiming, SSDI claimants receive $12,023 in SSDI benefits and WC claimants recieve $8,166 in WC benefits.

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, less likely to

[30]Given the requirements for SSDI and WC eligiblity, these differences in terms of labor market and health outcomes are perhaps not surprising.

be a racial minority, more highly educated, have higher labor force attachment, and have better health than claimants.

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 four outcome variables in the pre-MML period. To do so, we center the data around the MML effective year. While there is an obvious effective year for states that passed an MML, this is not the case for states that did not pass an MML. 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 before the event in the pre-nine year bin and all years more than nine years after the event in the post-nine year bin. We plot unadjusted trends in any SSDI (Figure 1), the logarithm of SSDI benefits (Figure 2), any WC (Figure 3), and the logarithm of WC benefits (Figure 4).

While the two series do not move entirely in parallel in the pre-treatment period the trends for our outcomes appear to move broadly in the same direction. Moreover, 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 differences in trends.

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:

$$B_{jst} = \alpha_0 + \alpha_1 Treat_s * Trend_{st} + X'_{jst}\alpha_2 + \rho'_{st}\beta_3 + \delta_s + \eta_t + \epsilon_{jst} \tag{2}$$

We interact an indicator variable for states that pass an MML by 2012[31] ($Treat_s$) with a linear time-to-event trend ($Trend_{st}$; this variable differs across states depending on their 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 ($Treat_s * Trend_{st}$). We estimate Equation 2 for each of our four claiming outcomes. 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 A4. None 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. However, we note that some of the standard error estimates are somewhat large and prevent us from ruling out non-trivial differences in pre-treatment trends. However, Equation 1 controls for state-specific linear time trends which allow for linear differences in pre-treatment trends.

4.3 Regression analysis of benefit claiming

Table 4 reports selected results generated in our DD regression models for SSDI and WC benefit outcomes. Passage of an MML leads to a 0.27 percentage point (9.9%) increase in the probability of SSDI claiming and a 2.6% increase in SSDI benefits. We find no statistically significant evidence that passage of an MML leads to changes in WC claiming outcomes we consider. The coefficient estimates, while imprecise, suggest that passage of an MML leads to a 6.5% increase in the probability of claiming WC and a 0.7% increase in WC benefits.

In terms of our extensive margin outcomes (any SSDI and any WC claiming), while the estimated absolute effect sizes (i.e., percentage point) are modest some of the estimated relative effect sizes (i.e., %) are arguably non-trivial. We suspect that the low prevalence of our claiming outcomes (i.e., 2.7% of the sample claims SSDI and 1.1% of the sample claims WC) leads to the non-trivial relative effect size estimates.

While it is somewhat surprising that MML effects are more precisely estimated for our SSDI outcomes than our WC outcomes, we suspect that the divergence is potentially par-

[31] Recall that 2012 is the last income year in the ASEC as income variables pertain to the last calendar year.

tially driven by differences in power to reject the null hypothesis: our estimates are much smaller in magnitude for WC than for SSDI. Another possibility is that – for myriad reasons including differences in particular health conditions, costs and benefits of marijuana use in terms of labor market opportunities, and the propensity to use marijuana obtained through an MML for recreational vs. medical use – the populations that claim SSDI are more responsive to MMLs than the populations that claim WC. For example, WC claimants are much more attached to the labor market, have higher education, and have better health than SSDI claimants (Table 3). The consequences of impeded labor market productivity (which may occur with either medical or recreational marijuana use) are plausibly greater for WC claimants than SSDI claimants. Moreover, the health conditions from which these different populations are more likely to suffer may be more or less effectively treated by marijuana obtained through MMLs. For example, mental health problems are a common ailment among SSDI claimants (Social Security Administration, 2016).

5 Extensions and robustness checks

5.1 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 than younger adults (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. We next examine heterogeneity in MML effects by age: we estimate Equation 1 for respondents ages 23 to 40 years and 41 to 62 years. Table A5 reports results for SSDI and Table A6 reports results for WC.

We find no statistically significant evidence that passage of an MML leads to changes in benefit claiming among older adults. However, passage of such laws increases claiming among younger populations. Following implementation of an MML, the probability of claiming SSDI increases by 0.28 percentage points (24%) and the probability of claiming WC increases by 0.15 percentage points (15%) among workers age 23 to 40 years. We observe that the level of SSDI benefits increases by 2.4% and the level of WC benefits increases by 1.2% following an MML among younger workers, although the former coefficient estimate is not precise.[32]

[32]Because we idenitify the strongest evidence of MML effects on claiming among younger adults, we have re-estimated all robustness checking, reproted later in the mansucript, in the younger adult sample. The robustness checking results for this sub-sample are very similar to the full sample results; these results are

5.2 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 Tables A7 (SSDI) and A8 (WC). The coefficients estimated in the male and female samples are comparable in sign to the full sample, but the estimates are imprecise. While we acknowledge that the coefficient are imprecise in all sex-specific regressions, preventing us from drawing strong conclusions in regards to differential treatment effects by sex, we note that in all regressions the absolute and relative effect sizes are larger for men than for women. For example, post-MML the propensity to claim SSDI and WC increases by 10.3% and 9.5% among men and 9.0% and 1.3% among women in passing states relative to non-passing states.

5.3 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.[33] 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

available on request.

[33] Another MML feature that is potentially relevant to our analysis is explicit protection for workers who use marijuana medically from being fired by their employer for use of this medication. Several states have passed an MML that confers such protection to workers (Hollinshead, 2013). However, these laws were implemented very recently, 2012 or later, and thus we do not have sufficient post-law data to study the effects of this feature. We encourage future research on this topic.

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 Tables A9 (SSDI) and A10 (WC). Two law features appear to be particularly important for SSDI and WC claiming: non-specific pain included as a qualifying health condition and dispensaries. Passage of a law that includes non-specific pain as a qualifying health condition leads to an 12.8% increase in the probability of receiving SSDI, a 3.3% increase in SSDI benefits, and a 0.8% increase in WC benefits (the coefficient estimate in the any WC claiming regression is imprecise). Passage of an MML that allows dispensaries leads to a 1.3% increase in WC benefits. We find no statistically significant evidence that psasage of laws without these features lead to changes in claiming.

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, 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 2017 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 A11 (SSDI) and Table A12 (WC), are not appreciably different from the full sample results.

5.4 Event study

A general concern in analyses of public health policies, such as the state MMLs that we examine here, 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.[34] 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 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 Tables A13 (SSDI) and A14 (WC).

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 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.

5.5 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 benefits 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 benefits; this measure includes SSDI benefits, Old-Age and Survivors Insurance benefits, and such. (ii) We refine the measure defined in (i) by requiring that the respondent report Social Security benefits (regardless of source) and report a work-limiting disability at the time the respondent completed the ASEC. Requiring that the respondent have a work-limiting disability may allow us to better capture SSDI benefits (Burkhauser et al., 2014). We also use data 2001-2013 and include benefits when a respondent reports own disability as the first, but not second, reason for receiving Social Security benefits. We report results in Table A15. Overall the estimated coefficient estimates generated using these

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

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.

5.6 Alternative controls for between-state heterogeneity

In the analyses presented thus far we control for unobservable between-state time-varying heterogeneity through the use of state-specific linear time trends. While a standard approach in policy analyses (Sabia et al., 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 of treatment effects. (ii) If, on the other hand, 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,[35] 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).[36] Results generated in these alternative specifications are reported in Tables A16 (SSDI) and A17 (WC). The results are broadly robust to these different approaches to controlling for between-state differences. However the magnitude and the precision of the estimates varies across specification to some extent.

5.7 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).[37] We next re-estimate our regression models using coding schemes proposed

[35]We use the four U.S. regions: Northeast, Midwest, South, and West to construct the region-by-year fixed effects.

[36]We convert the nominal taxes to real terms using the CPI.

[37]Details of the law comparison are available on request from the corresponding author.

by Pacula et al. (2015) and Wen et al. (2015). Results are reported in Tables A18 (SSDI) and A19 (WC). The coefficient estimates are not appreciably different across the alternative approaches to coding MML.

5.8 Smuggling

Individuals living in a state 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 one if the state boarders a state with an MML and zero otherwise. Results are reported in Table A20. The point estimates generated in the models that control for cross-border smuggling are nearly identical to the main results although the standard error on the estimate in the any SSDI claiming regression increases slightly and the coefficient estimate is not statistically distinguishable from zero.

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 among adults ages 23 to 62 years using data from the 1990 to 2013 Current Population Survey. We find that passage of an MML increases SSDI claiming on both the intensive and extensive margins in the full sample. In particular, post-MML the propensity to claim SSDI increases by 0.27 percentage points (9.9%) and SSDI benefits increase by 2.6%. We find no statistically significant evidence that passage of an MML leads to changes in WC claiming in the full sample; athough the estiamted coefficients are positive. We identify heterogeneity by age: passage of an MML increases both SSDI and WC claiming among younger (23 to 40 years), but not older (41 to 62 years), adults. When we examine MML features, we find that laws permitting non-specific pain as a qualifying health condition and that allow dispensaries increase benefit claiming.

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. 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 MML passage leads to increases in SSDI and WC claiming among working age adults.

We hypothesize that our findings are plausibly driven by the work-impeding side effects of marijuana used medically and through recreational use of marijuana. Unfortunately, our dataset (the CPS) does not allow us to test these hypotheses. As datasets that allow researchers to separate 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 our effect sizes are reasonable. (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 0.27 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 (NSDUH) Azofeifa (2016) shows that 8.4% of U.S. residents aged 21 years and older reported any form of marijuana use in the past month in 2014. Rates are particularly high among younger workers. For instance, 19.6% among adults ages 18 to 26 years and 12.7% among adults ages 26 to 34 years. We can make similar comparisons for our estimates generated in models that include indicators for MMLs allowing non-specific pain as a qualifying health condition and MMLs that permit dispensaries. Previous economic studies note that these features are particularly important, and may lead to larger increases in use, for non-medical marijuana (Anderson et al., 2013; Pacula et al., 2015; Wen et al., 2015). 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.

Overall, the literature on the labor market effects of MMLs presents a quandary for policymakers. On the one hand, Ullman (2016) finds that passage of an MML reduces sick absences and 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-incentive 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; Bradford & Bradford, 2016, 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). 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

Sample:	All states	MML states	Non-MML states
Outcome variables			
Any SSDI income	0.0274	0.0244	0.0292
SSDI income	330.8	298.0	351.2
Any WC income	0.0107	0.0122	0.00971
WC income	87.09	105.1	76.07
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

Notes: Data source is the 1990-2013 ASEC.

Table 3: Characteristics of SSDI and WC benefit claimants, and non-claimants

Sample by claimant status:	SSDI	WC	No claiming
Age	49.22	42.02	41.14
Male	0.492	0.618	0.478
Female	0.508	0.382	0.521
Hispanic	0.107	0.151	0.151
Non-Hispanic	0.893	0.849	0.849
White	0.735	0.837	0.819
African American	0.194	0.103	0.105
Other race	0.071	0.060	0.075
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 ($)	864.67	19,161.29	30,364.92
Work limiting disability	0.859	0.388	0.050
Conditional benefit level ($)	12,023.18	8,166.23	-
Observations	38,906	23,927	2,180,695

Notes: Data source is the 1990-2013 ASEC.

Table 4: Effect of MML on SSDI and WC outcomes

Outcome:	Any benefit	Log(benefit income +1)
SSDI outcomes		
Sample proportion/mean:	0.0274	330.8
Any MML	0.0027*	0.0257*
	(0.0016)	(0.0150)
Observations	1,421,399	1,421,399
WC outcomes		
Sample proportion/mean:	0.0107	87.09
Any MML	0.0007	0.0065
	(0.0007)	(0.0054)
Observations	2,243,528	2,243,528

Notes: Data source is the 1990-2013 ASEC. All models control for personal characteristics, state characteristics, state-specific linear time trends, state fixed effects, and year fixed effects. LPM applied when the outcome is binary and LS applied when the outcome is continuous. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table A1: Effect of MML on any benefit outcomes using a probit model

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

Notes: Data source is the 1990-2013 ASEC. All models control for personal characteristics, state characteristics, state-specific linear time trends, state fixed effects, and year fixed effects. Probit models applied. Average marginal effects reported. 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 MML on level of benefit income using alternative specifications

Outcome:	SSDI	WC
Sample mean:	330.8	87.09
Poisson model	45.5774**	13.1943*
	(20.7026)	(6.7717)
Unlogged outcomes using LS	45.3803**	15.8172**
	(21.3648)	(7.8387)
Observations	1,421,399	2,243,528

Notes: Data source is the 1990-2013 ASEC. All models 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 A3: Effect of MML on SSDI and WC outcomes using sample weights

Outcome:	Any benefit	Log(benefit income+1)
SSDI outcomes		
Sample proportion/mean (weighted):	0.0299	0.276
Any MML	0.0039	0.0381
	(0.0033)	(0.0303)
Observations	1,421,399	1,421,399
WC outcomes		
Sample Proportion/mean:	0.0106	0.0866
Any MML	0.0010	0.0097
	(0.0007)	(0.0061)
Observations	2,243,502	2,243,502

Notes: Data source is the 1990-2013 ASEC. All models are weighted by the ASEC sample weights and control for personal characteristics, state characteristics, state-specific linear time trends, state fixed effects, and year fixed effects. LPM applied when the outcome is binary and LS applied when the outcome is continuous. 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: Test of pre-implementation trends in SSDI and WC outcomes

Outcome:	Any benefit	Log(benefit income+1)
SSDI outcomes		
Sample proportion/mean:	0.0262	310.94
$Treat_s * Trend_{st}$	0.0004	0.0018
	(0.0039)	(0.0361)
Observations	652,262	652,262
WC outcomes		
Sample proportion/mean:	0.0118	92.84
$Treat_s * Trend_{st}$	0.0000	0.0005
	(0.0001)	(0.0006)
Observations	1,456,179	1,456,179

Notes: Data source is the 1990-2013 ASEC. All models control for personal characteristics, state characteristics, state fixed effects, and time-to-event fixed effects. LPM applied when the outcome is binary and LS applied when the outcome is continuous. 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 MML on SSDI outcomes by age group

Outcome:	Any benefit	Log(benefit income+1)
Younger workers: 23-40 years		
Sample proportion/mean:	0.0116	120.1
Any MML	0.0028*	0.0244
	(0.0016)	(0.0146)
Observations	656,952	656,952
Older workers: 41-62 years		
Sample proportion/mean:	0.0409	512.0
Any MML	0.0027	0.0269
	(0.0019)	(0.0177)
Observations	764,447	764,447

Notes: Data source is the 1990-2013 ASEC. All models control for personal characteristics, state characteristics, state-specific linear time trends, state fixed effects, and year fixed effects. LPM applied when the outcome is binary and LS applied when the outcome is continuous. 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: Effect of MML on WC outcomes by age group

Outcome:	Any benefit	Log(benefit income+1)
Younger workers: 23-40 years		
Sample proportion/mean:	0.0100	65.73
Any MML	0.0015**	0.0122**
	(0.0007)	(0.0051)
Observations	1,095,590	1,095,590
Older workers: 41-62 years		
Sample proportion/mean:	0.0113	107.5
Any MML	-0.0001	0.0018
	(0.0008)	(0.0070)
Observations	1,147,938	1,147,938

Notes: Data source is the 1990-2013 ASEC. All models control for personal characteristics, state characteristics, state-specific linear time trends, state fixed effects, and year fixed effects. LPM applied when the outcome is binary and LS applied when the outcome is continuous. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table A7: Effect of MML on SSDI outcomes by sex

Outcome:	Any benefit	Log(benefit income+1)
Men		
Sample proportion/mean:	0.0281	374.8
Any MML	0.0029	0.0278
	(0.0018)	(0.0173)
Observations	681,356	681,356
Women		
Sample proportion/mean:	0.0267	290.3
Any MML	0.0024	0.0228
	(0.0018)	(0.0165)
Observations	740,043	740,043

Notes: Data source is the 1990-2013 ASEC. All models control for personal characteristics, state characteristics, state-specific linear time trends, state fixed effects, and year fixed effects. LPM applied when the outcome is binary and LS applied when the outcome is continuous. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table A8: Effect of MML on WC outcomes by sex

Outcome:	Any benefit	Log(benefit income+1)
Men		
Sample proportion/mean:	0.0137	124.1
Any MML	0.0013	0.0113
	(0.0009)	(0.0070)
Observations	1,077,479	1,077,479
Women		
Sample proportion/mean:	0.0078	52.9
Any MML	0.0001	0.0020
	(0.0007)	(0.0061)
Observations	1,166,049	1,166,049

Notes: Data source is the 1990-2013 ASEC. All models control for personal characteristics, state characteristics, state-specific linear time trends, state fixed effects, and year fixed effects. LPM applied when the outcome is binary and LS applied when the outcome is continuous. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table A9: Effect of MML on SSDI outcomes by law features

Outcome:	Any benefit	Log(benefit income+1)
Sample proportion/mean:	0.0274	330.8
Cultivation:		
Law prevalence rate: 11.01%		
Cultivation	0.0033	0.0309
	(0.0022)	(0.0210)
Observations	1,421,399	1,421,399
Dispensaries:		
Law prevalence rate: 8.45%		
Dispensaries	0.0038	0.0364
	(0.0031)	(0.0288)
Observations	1,421,399	1,421,399
Non-specific pain:		
Law prevalence rate: 14.62%		
Non-specific pain	0.0035*	0.0325*
	(0.0018)	(0.0169)
Observations	1,421,399	1,421,399
Registry:		
Law prevalence rate: 6.33%		
Registry	0.0013	0.0111
	(0.0010)	(0.0098)
Observations	1,421,399	1,421,399

Notes: Data source is the 1990-2013 ASEC. All models control for personal characteristics, state characteristics, state-specific linear time trends, state fixed effects, and year fixed effects. LPM applied when the outcome is binary and LS applied when the outcome is continuous. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table A10: Effect of MML on WC outcomes by law features

Outcome:	Any benefit	Log(benefit income+1)
Sample proportion/mean:	0.0107	87.09
Cultivation:		
Law prevalence rate: 11.01%		
Cultivation	0.0003	0.0034
	(0.0007)	(0.0051)
Observations	2,243,528	2,243,528
Dispensaries:		
Law prevalence rate: 8.45%		
Dispensaries	0.0015	0.0128*
	(0.0009)	(0.0074)
Observations	2,243,528	2,243,528
Non-specific pain:		
Law prevalence rate: 14.62%		
Non-specific pain	0.0009	0.0084*
	(0.0006)	(0.0049)
Observations	2,243,528	2,243,528
Registry:		
Law prevalence rate: 6.33%		
Registry	0.0003	0.0014
	(0.0011)	(0.0086)
Observations	2,243,528	2,243,528

Notes: Data source is the 1990-2013 ASEC. All models control for personal characteristics, state characteristics, state-specific linear time trends, state fixed effects, and year fixed effects. LPM applied when the outcome is binary and LS applied when the outcome is continuous. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table A11: Effect of MML on SSDI outcomes by law features: Untreated control group

Outcome:	Any benefit	Log(benefit income+1)
Cultivation:		
Law prevalence rate: 16.77%		
Sample proportion/mean:	0.0265	319.99
Cultivation	0.0031	0.0298
	(0.0023)	(0.0217)
Observations	1,195,999	1,195,999
Dispensaries:		
Law prevalence rate: 12.26%		
Sample proportion/mean:	0.0268	322.87
Dispensaries	0.0035	0.0339
	(0.0029)	(0.0276)
Observations	1,377,746	1,377,746
Non-specific pain:		
Law prevalence rate: 22.83%		
Sample proportion/mean:	0.0265	320.29
Non-specific pain	0.0028*	0.0269*
	(0.0016)	(0.0158)
Observations	1,335,321	1,335,321
Registry:		
Law prevalence rate: 11.34%		
Sample proportion/mean:	0.0274	328.82
Registry	0.0011	0.0102
	(0.0009)	(0.0095)
Observations	1,244,981	1,244,981

Notes: Data source is the 1990-2013 ASEC. Untreated control group = states that have not passed any MML. All models control for personal characteristics, state characteristics, state-specific linear time trends, state fixed effects, and year fixed effects. LPM applied when the outcome is binary and LS applied when the outcome is continuous. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table A12: Effect of MML on WC outcomes by law features: Untreated control group

Outcome:	Any benefit	Log(benefit income+1)
Cultivation:		
Law prevalence rate: 11.75%		
Sample proportion/mean:	0.0105	85.50
Cultivation	0.0004	0.0047
	(0.0007)	(0.0054)
Observations	1,889,718	1,889,718
Dispensaries:		
Law prevalence rate: 8.69%		
Sample proportion/mean:	0.0106	86.43
Dispensaries	0.0016*	0.0141*
	(0.0009)	(0.0073)
Observations	2,180,887	2,180,887
Non-specific pain:		
Law prevalence rate: 15.60%		
Sample proportion/mean:	0.0107	86.81
Non-specific pain	0.0008	0.0074
	(0.0006)	(0.0047)
Observations	2,102,737	2,102,737
Registry:		
Law prevalence rate: 7.29%		
Sample proportion/mean:	0.0101	81.48
Registry	0.0001	-0.0002
	(0.0011)	(0.0091)
Observations	1,947,233	1,947,233

Notes: Data source is the 1990-2013 ASEC. Untreated control group = states that have not passed any MML. All models control for personal characteristics, state characteristics, state-specific linear time trends, state fixed effects, and year fixed effects. LPM applied when the outcome is binary and LS applied when the outcome is continuous. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table A13: Effect of MML on SSDI outcomes: Event study model

Outcome:	Any benefit	Log(benefit income+1)
Sample proportion/mean:	0.0274	330.8
-9 years	0.0006	0.0062
	(0.0017)	(0.0153)
-8 years	0.0020	0.0154
	(0.0022)	(0.0197)
-7 years	0.0021	0.0188
	(0.0017)	(0.0143)
-6 years	0.0026	0.0254
	(0.0021)	(0.0192)
-5 years	0.0009	0.0094
	(0.0017)	(0.0155)
-4 years	0.0001	0.0007
	(0.0013)	(0.0121)
-3 years	-0.0001	-0.0012
	(0.0014)	(0.0126)
-2 years	0.0009	0.0078
	(0.0017)	(0.0161)
Event year	0.0021	0.0190
	(0.0017)	(0.0156)
+1 years	0.0033**	0.0312**
	(0.0013)	(0.0126)
+2 years	0.0021	0.0192
	(0.0018)	(0.0166)
+3 years	0.0019*	0.0178*
	(0.0011)	(0.0095)
+4 years	0.0015	0.0137
	(0.0014)	(0.0135)
+5 years	0.0011	0.0099
	(0.0012)	(0.0111)
+6 years	-0.0005	-0.0044
	(0.0013)	(0.0122)
+7 years	0.0010	0.0094
	(0.0019)	(0.0172)
+8 years	0.0000	-0.0004
	(0.0016)	(0.0154)
+9 years	-0.0009	-0.0080
	(0.0010)	(0.0095)
Observations	1,421,399	1,421,399

Notes: Data source is the 1990-2013 ASEC. All models control for personal characteristics, state characteristics, state fixed effects, and year fixed effects. LPM applied when the outcome is binary and LS applied when the outcome is continuous. The omitted category is the year prior to MML effective year. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table A14: Effect of MML on WC outcomes: Event study model

Outcome:	Any benefit	Log(benefit income+1)
Sample proportion/mean:	0.0107	87.09
-9 years	0.0013	0.0123
	(0.0009)	(0.0078)
-8 years	0.0015	0.0107
	(0.0011)	(0.0094)
-7 years	0.0001	-0.0014
	(0.0006)	(0.0052)
-6 years	0.0006	0.0029
	(0.0010)	(0.0082)
-5 years	0.0001	0.0009
	(0.0005)	(0.0045)
-4 years	0.0004	0.0017
	(0.0008)	(0.0070)
-3 years	-0.0011*	-0.0088*
	(0.0006)	(0.0050)
-2 years	-0.0003	-0.0029
	(0.0007)	(0.0055)
Event year	0.0002	0.0009
	(0.0009)	(0.0078)
+1 years	0.0001	-0.0008
	(0.0008)	(0.0065)
+2 years	0.0011	0.0094
	(0.0012)	(0.0101)
+3 years	0.0005	0.0033
	(0.0011)	(0.0094)
+4 years	0.0011	0.0114*
	(0.0008)	(0.0063)
+5 years	-0.0002	-0.0023
	(0.0010)	(0.0090)
+6 years	-0.0010	-0.0072
	(0.0014)	(0.0128)
+7 years	0.0002	0.0022
	(0.0016)	(0.0141)
+8 years	-0.0009	-0.0060
	(0.0011)	(0.0098)
+9 years	-0.0006	-0.0038
	(0.0006)	(0.0052)
Observations	2,243,528	2,243,528

Notes: Data source is the 1990-2013 ASEC. All models control for personal characteristics, state characteristics, state fixed effects, and year fixed effects. LPM applied when the outcome is binary and LS applied when the outcome is continuous. The omitted category is the year prior to MML effective year. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table A15: Effect of MML on SSDI outcomes: Alternative definitions of SSDI

Outcome:	Any benefit	Log(benefit income+1)
SSDI long with no WLD restriction		
Sample proportion/mean:	0.0391	435.0
Any MML	0.0018	0.0165
	(0.0012)	(0.0116)
Observations	2,243,528	2,243,528
SSDI long with WLD restriction		
Sample proportion/mean:	0.0246	287.2
Any MML	0.0007	0.0070
	(0.0010)	(0.0092)
Observations	2,243,528	2,243,528
First Social Security payment source only		
Sample proportion/mean:	0.0266	320.1
Any MML	0.0022	0.0214
	(0.0016)	(0.0147)
Observations	1,421,399	1,421,399

Notes: Data source is the 1990-2013 ASEC. WLD = work-limiting disability. See text for more details on the alternative definitions of SSDI. All models control for personal characteristics, state characteristics, state-specific linear time trends, state fixed effects, and year fixed effects. LPM applied when the outcome is binary and LS applied when the outcome is continuous. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table A16: Effect of MML on SSDI outcomes: Alternative sets of controls for between state differences

Outcome:	Any benefit	Log(benefit income+1)
Sample proportion/mean:	0.0274	330.8.1
State FE and year FE	0.0010	0.0085
	(0.0017)	(0.0160)
State FE, year FE, and state quadratic trends	0.0038**	0.0356**
	(0.0017)	(0.0161)
State FE, year FE, and region-by-year FE	0.0018	0.0166
	(0.0015)	(0.0141)
Additional state level controls	0.0026	0.0249
	(0.0016)	(0.0157)
Observations	1,421,399	1,421,399

Notes: Data source is the 1990-2013 ASEC. FE = fixed effects. All models 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. LPM applied when the outcome is binary and LS applied when the outcome is continuous. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table A17: Effect of MML on WC outcomes: Alternative sets of controls for between state differences

Outcome:	Any benefit	Log(benefit income+1)
Sample proportion/mean:	0.0107	87.09
State FE and year FE	-0.0008	-0.0062
	(0.0006)	(0.0053)
State FE, year FE, and state quadratic trends	0.0015*	0.0130*
	(0.0009)	(0.0075)
State FE, year FE, and region-by-year FE	-0.0006	-0.0053
	(0.0005)	(0.0044)
Additional state level controls	0.0006	0.0063
	(0.0006)	(0.0054)
Observations	2,243,528	2,243,528

Notes: Data source is the 1990-2013 ASEC. FE = fixed effects. All models 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. LPM applied when the outcome is binary and LS applied when the outcome is continuous. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table A18: Effect of MML on SSDI outcomes: Alternative MML coding schemes

Outcome:	Any benefit	Log(benefit income+1)
Sample proportion/mean:	0.0266	320.1
Pacula et al.	0.0028**	0.0276**
	(0.0014)	(0.0129)
Wen et al.	0.0027*	0.0257*
	(0.0016)	(0.0150)
Observations	1,421,399	1,421,399

Notes: Data source is the 1990-2013 ASEC. All models control for personal characteristics, state characteristics, state-specific linear time trends, state fixed effects, and year fixed effects. LPM applied when the outcome is binary and LS applied when the outcome is continuous. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table A19: Effect of MML on WC outcomes: Alternative MML coding schemes

Outcome:	Any benefit	Log(benefit income+1)
Sample proportion/mean:	0.0107	87.09
Pacula et al.	0.0006	0.0056
	(0.0007)	(0.0054)
Wen et al.	0.0006	0.0058
	(0.0006)	(0.0051)
Observations	2,243,528	2,243,528

Notes: Data source is the 1990-2013 ASEC. All models control for personal characteristics, state characteristics, state-specific linear time trends, state fixed effects, and year fixed effects. LPM applied when the outcome is binary and LS applied when the outcome is continuous. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

Table A20: Effect of MML on SSDI and WC outcomes: Controlling for cross-boarder smuggling

Outcome:	Any benefit	Log(benefit income+1)
SSDI outcomes		
Sample proportion/mean:	0.0274	330.8
Any MML	0.0027	0.0256*
	(0.0016)	(0.0150)
Observations	1,421,339	1,421,339
WC outcomes		
Sample proportion/mean:	0.0107	87.09
Any MML	0.0007	0.0067
	(0.0007)	(0.0053)
Observations	2,243,528	2,243,528

Notes: Data source is the 1990-2013 ASEC. All models control for an indicator for a state with an MML in place (proxy for ability to smuggle), personal characteristics, state characteristics, state-specific linear time trends, state fixed effects, and year fixed effects. LPM applied when the outcome is binary and LS applied when the outcome is continuous. Standard errors are clustered at the state level and are reported in parentheses. ***,**,* = statistically different from zero at the 1%,5%,10% level.

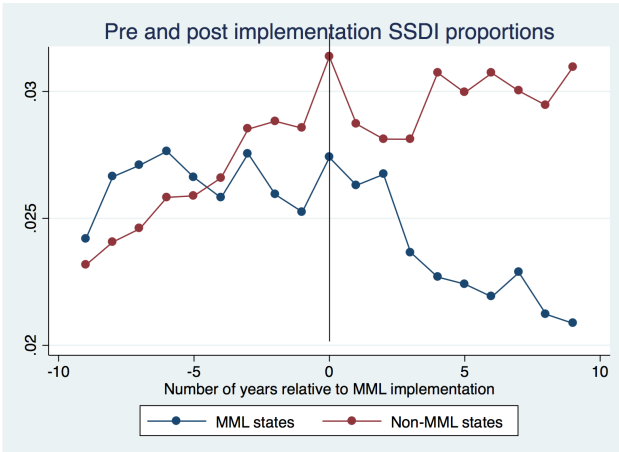

Figure 1: Trends in any SSDI claiming

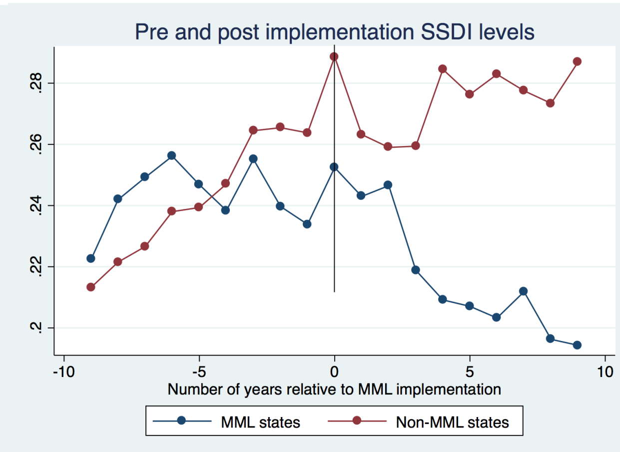

Figure 2: Trends in SSDI benefits(logged)

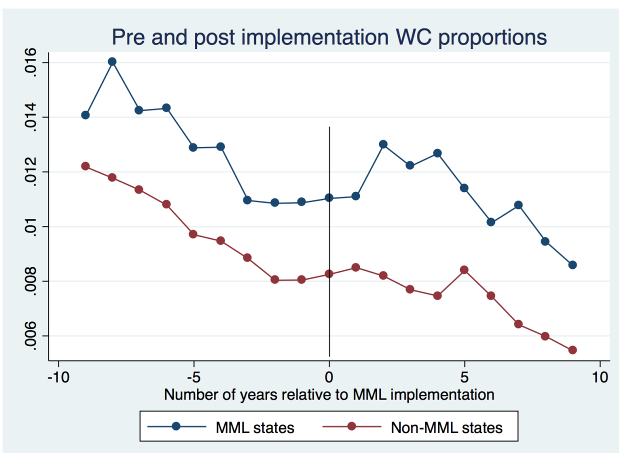

Figure 3: Trends in any WC claiming

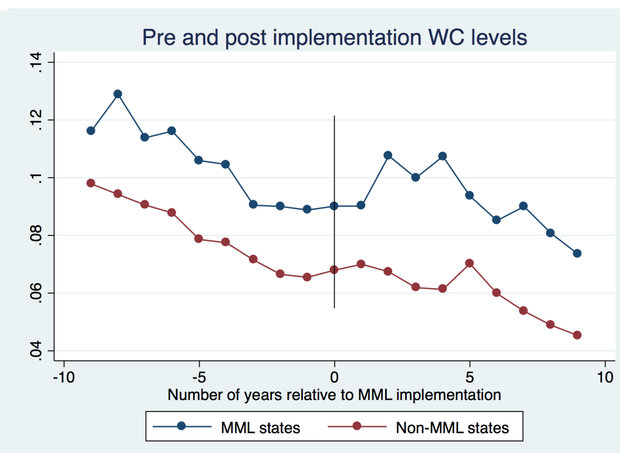

Figure 4: Trends in WC benefits (logged)

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