12-11-2018

Essays on Medical Marijuana Laws, Health Insurance and Health Care Utilization

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ABSTRACT

ESSAYS ON MEDICAL MARIJUANA LAWS, HEALTH INSURANCE AND HEALTH CARE UTILIZATION

BY

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AUGUST 2018

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National Health Expenditures Accounts estimates that U.S health care spending grew 4.3 percent from the previous year to reach $3.3 trillion, or $10,338 per person in 2016. The overall share of gross domestic product (GDP) related to health care spending was 17.9 percent in 2016, up from 17.7 percent in 2015. Moreover, increased use of opioid prescriptions led to excessive use and abuse of these drugs, resulting in nationwide "opioid epidemic". This dissertation examines how different policy interventions contributed to the rise in health care utilization and prescribed opioids in U.S.

The first chapter examines how medical marijuana laws changed utilization of prescription drugs with a special emphasis on prescribed opioids. More than half of the US population lives in a state that has adopted medical marijuana laws (MMLs). Studies show that most medical marijuana patients use marijuana for managing their pain with the overwhelming majority of them preferring it to opioids. Despite ongoing pro-marijuana policies and the growing trend of public acceptance, the evidence on how people change their prescription use due to the availability of marijuana as an alternative treatment is limited. Using the variations across state MMLs between 1996 and 2014 of Medical Expenditure Panel Survey (MEPS) this paper estimates the effects of MMLs on prescription drug utilization, with a

focus on opioids. I find that MMLs lead to a $2.47 decrease in per person prescribed opioid spending among young adults (ages 18-39) over a year. Most of this decrease results from the intensive margin of use and MML states that allow home cultivation experience even larger decreases. Furthermore, the decreasing effects are persistent over time and they get stronger following the years of implementation. MMLs also decrease the number of opioid pill use among young adults. I do not find any discernible impact on older populations' opioid utilization. I then investigate the effects on other prescriptions for which marijuana can be a potential substitute and find the allowance of dispensaries is generally associated with decreases, although the effects depend on the type of MML, the margin of use and age.

The second chapter examines how universal insurance coverage affects health care utilization drawing evidence from the health reform of Massachusetts in 2006. This law reformed insurance markets, mandated that all residents in the state would be required to take up health insurance, and provided subsidies for lower-income individuals to purchase it. Using data from MEPS between 2000 and 2015, I provide evidence that the Massachusetts health care reform increased counts of hospital and office-based medical provider visits significantly. The results were robust to using alternative control groups and different functional form assumptions. I find the reform's effects grew over time, reaching its maximum after 2010. The reform also increased health care expenditures and probability of health care service use significantly. Finally, I use the reform to instrument for health insurance and estimate large impacts of insurance on health care utilization.

The third chapter examines the impact of the Affordable Care Act on health care utilization. The Affordable Care Act (ACA) aimed to achieve nearly universal health insurance coverage in the United States through a combination of regulations, mandates, subsidies, exchanges, and Medicaid expansions. We use data from the Medical Expenditure Panel Survey (MEPS) to investigate the impacts of the ACA on the health care utilization and expenditures of non-elderly adults. A difference-in-difference-in-differences strategy separately

identifies the effects of the ACA's expansions of private and Medicaid coverage by leveraging variation in states' Medicaid expansion decisions and individuals' pre-ACA insurance status. Intuitively, impacts of the ACA's insurance expansions should be concentrated among those who lacked insurance prior to the law, and such individuals are more likely to be affected in states that participated in the Medicaid expansion. Similar methods have been used to study the ACA's effects on outcomes such as health insurance coverage, access to care, risky health behaviors, and self-assessed health. However, they have not been previously used to investigate impacts on health care spending. Theoretically, the net effect on spending is ambiguous. On one hand, insurance lowers the effective price of care, which should increase utilization across-the-board. On the other hand, insurance improves access to primary and preventive care, which could potentially reduce use of expensive emergency services. The results suggest that the ACA increased health care utilization in some dimensions – including counts of inpatient hospital visits, medical-provider office visits and total counts of prescription fills, inpatient, outpatient, medical-provider office and ER visits combined on its first year. However, these increases in health care utilization in counts were not observed in ACA's second year. We also found that the ACA increased coverage and led to significant gains in both expansion and non-expansion states consistent with what has been found by prior studies. This significant gain in insurance was not limited to ACA's first year but it carried to second year.

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BY

PELIN ÖZLÜK

A Dissertation Submitted in Partial Fulfillment

of the Requirements for the Degree

of

Doctor of Philosophy

in the

Andrew Young School of Policy Studies of

Georgia State University

GEORGIA STATE UNIVERSITY

2018

ACCEPTANCE

This dissertation was prepared under the direction of Pelin Özlük's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

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DEDICATION

This dissertation is dedicated to my grandmother, Ayşe Dinç.

ACKNOWLEDGMENTS

I would like to express my deepest appreciation to my advisor, Chuck Courtemanche for guiding and supporting me over the years. Without his guidance this dissertation would not have been possible. He has set an excellent example of a mentor, and role model for me. I would also express my gratitude to my dissertation committee members Jim Marton, Shif Gurmu and Jason Hockenberry for all of their guidance through this process; their discussion, ideas, and feedback have been invaluable. I would also like to thank Tom Mroz for always being available to discuss and guide my research, and Garth Heutel for his support.

I am extremely grateful to my mom, Vicdan Dinç for her endless love, support and protection. I would not have been who I am without her by my side. I am deeply grateful to my sister, Ekin Özlük for being my best friend and my guide in life. I am incredibly lucky to have her as my sister. I would also extend my deepest gratitude to my brother in law Jason Neymeyer who has been an excellent role model and my support system for me over the years. I also owe many thanks to my father Kenan Özlük for always being on my side.

I would like to extend my thanks to my best friends in life, Lale Seçginli and Aylin Kertik for their endless love and friendship. I am very grateful to have met you earlier in life so I could grow with you. I am deeply thankful to all the other great friends I have made in the program. They have been incredibly supportive and made my experience at Georgia State fun. I want to specifically thank Rhita Simorangkir for her genuine friendship.

Last but not least, I would like to express my gratitude to Kuzu, Koko, Buck and Teddy for filling my life with love and fun.

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Chapter 1

The Effects of Medical Marijuana Laws on Utilization of Prescribed Opioids and Other Prescription Drugs

1.1 Introduction

Between 1996 and 2017, 29 states and the District of Columbia enacted laws that legalized the medical use of marijuana. Eight states and D.C. legalized recreational use and 19 states and D.C. have operating dispensaries. The total estimated value of legal marijuana sales in the United States was $5.7 billion in 2015 and $7.1 billion in 2016 (Arcview, 2017). The market is projected to grow as more than half of the U.S population now lives in a state where marijuana is legalized either medically or recreationally. Understanding the consequences of legalizing marijuana as a medicine is important as more states are discussing new medical marijuana laws (MMLs) in the near future. However, all these ongoing pro-marijuana policies are founded on limited scientific evidence on marijuana's effects on health due to the federal government's classification of marijuana as a Schedule 1 substance, which imposes significant barriers to conducting randomized controlled trials with human subjects to study marijuana's effects.

Despite the limitations, there is some evidence suggesting that marijuana can improve several health conditions and symptoms like nausea and vomiting, loss of appetite, depression, anxiety, chronic pain, and muscle spasms, as well as regulate sleep.[1] Prior studies generally find that the most reported reason for using medical marijuana among medical marijuana patients is the relief of pain, and most of those who use it for pain relief use it together with their opioid-based prescriptions.[2] According to a recent survey from a database of medical marijuana patients conducted by Reiman et al. (2017), 63% of participants reported using marijuana for pain-related conditions. 30% reported using an opioid-based drug and of those 61% reported using it with marijuana. In addition, more than 97% of their sample agreed they were able to decrease the amount of opioids they consume when they also used marijuana. 53% of their participants were between 20 and 39 years old.

Allowing marijuana as an option to treat pain and other symptoms can have two opposing effects on people's prescription opioid and other drug utilization. First, it can reduce utilization by inducing people to substitute away from prescriptions to marijuana. Second, MMLs can act like direct-to-consumer prescription drug advertising, inducing people to seek medical help for their conditions, which in turn increases demand for prescriptions.

This paper examines if MMLs influence prescription drug utilization with a particular focus on opioids; a category of powerful pain-reducing medicines with severe risks of addiction, abuse, overdose and death.[3] Using Medical Expenditure Panel Survey (MEPS) household and prescribed medicine files, I estimate the effects of MML implementation and

[1] Whiting et al. (2015); Borgelt et al. (2013); Jensen et al. (2015); Institute of Medicine (1999), Amar (2006), National Academies, (2017).

[2] Reinerman et al. (2011), Reiman et al. (2017).

[3] According to Centers for Disease Control (CDC), half of all U.S. opioid deaths involve a prescription opioid and 91 Americans die from opioid overdose every day. Deaths from prescription opioids and the sales of these prescriptions drugs have quadrupled from 1999. National Institute on Drug Abuse reports young adults (age 18 to 25) are the biggest abusers of prescription opioid pain relievers and in 2014 more than 1,700 died from prescription drug (mainly opioid) overdoses-more than died from overdoses of any other drug, including heroin and cocaine combined. https://www.drugabuse.gov/related-topics/trends-statistics/infographics/abuse-prescription-rx-drugs-affects-young-adults-most.

its provisions on utilization of prescribed opioids by exploiting the variations in MMLs across states over time. For my main analysis, I show results from two-part models, jointly estimating the extensive and intensive margins of prescribed opioid expenditures and their effects on each part of the model separately. I then examine MMLs' effects on utilization of other categories of drugs for which medical marijuana is a plausible substitute. Studying the effects on these other prescriptions is also important because they make up a large portion of overall health care expenditures.

My main results indicate that MMLs significantly decrease expenditures on opioids among young adults (ages 18-39) by $2.47 per person over a year. This decreasing effect results from the significant decrease on the intensive margin, implying that rather than quitting opioids altogether, young adults continue to use them with marijuana. States allowing home cultivation of marijuana experience even larger decreases in opioid expenditures. Furthermore, these decreasing effects of MMLs on opioid expenditures are persistent over time and they get stronger following the years of MML implementation. The results are similar when we consider the effects on the total amount of prescribed opioid pills. Namely, implementation of a MML decreases the total amount of prescription opioid pills by 2.16 pills per person over a year among young adults. I find no discernible effect of MMLs on the opioid utilization of older populations.

I then estimate MML's effects on utilization of other prescription drugs and find that MML states which allow retail dispensaries generally experience decreases on spending for the drugs which marijuana can substitute among young adults. MML is also associated with significant decreases in sedatives among elderly population (ages 65+). The results from other prescription drugs mostly depend on age and the level of access MMLs provide to marijuana.

Based on my findings MMLs can potentially alleviate the problems associated with opioid misuse in younger adults, the biggest abusers of prescription opioids. MMLs with

looser restrictions, especially those that allow greater access by legalizing dispensaries and allowing home cultivation, can reduce excess medical costs associated with adverse drug events[4], which cause more than 1 million emergency department visits and cost $3.5 billion each year (Aspden et al. 2007). The third reason why MMLs can be useful is because it can reduce the costs on the insurance pool. Medical marijuana is not covered by insurance like prescription drugs. If people switch to marijuana they pay it out of pocket. If MMLs turn a public health care cost into a private cost this can be welfare increasing by internalizing an externality.

The paper proceeds as follows. Section 2 summarizes the existing literature and provides background information on prevalence of marijuana on health and the evidence on its substitutability with opioids. Section 3 outlines the theoretical framework by laying out a simple patient-physician interaction in an MML state and gives some testable implications. Section 4 describes the data, variable measurement, and identification strategy. Section 5 shows primary results of the effects of MML on opioids and Section 6 presents sensitivity analyses and examines effects on other prescriptions. I conclude with a summary of my findings and implications for future medical marijuana policy design in Section 7.

1.2 Background

1.2.1 Medical uses of marijuana and substitutability with opioids

The National Sciences report of 2017 systemically reviewed the most recently published studies since 2011 that were "fair-and-good quality" in reaching conclusions on the

[4]An adverse drug event (ADE) is an injury resulting from medical intervention related to a drug. This includes medication errors, adverse drug reactions, allergic reactions, and overdoses. (Office of Disease Prevention and Health Promotion). https://health.gov/hcq/ade.asp

health effects of cannabis.[5] The report finds: 1) conclusive evidence that cannabis is effective in reducing chronic pain in adults, cancer-induced nausea and vomiting and patient-reported spasticity symptoms, 2) moderate evidence that cannabis is effective in improving short-term sleep outcomes, and 3) limited evidence that cannabis is effective in improving symptoms of anxiety and post-traumatic stress disorder.

Whiting et al. (2015) did a meta-analysis from a total of 79 trials (6462 participants) and reported the following findings: 1) moderate-quality evidence to suggest cannabis was beneficial for the treatment of chronic neuropathic or cancer pain and spasticity due to multiple sclerosis, 2) low-quality evidence to suggest cannabis was associated with improvements in cancer-induced nausea and vomiting, weight gain in HIV and sleep disorders, and 3) very low-quality evidence to suggest cannabis was associated with improvement in anxiety.

Given the risks and problems associated with opioid use and the growing acceptance of using marijuana as a medicine it is natural to ask two questions: 1) Can marijuana be a substitute for opioid-based medicines, and if so 2) do people really substitute away from opioids to marijuana? The literature from clinical studies and with selected samples from medical marijuana patients suggests that medical marijuana patient may substitute opioids for marijuana.

Abrams et al. (2011) study the cannabis-opioid interaction drawing evidence from 21 patients with chronic pain. They conclude that cannabis augments the pain relieving effects of opioids and their combination may allow for opioid treatment at lower doses with fewer side effects. Drawing evidence from an open-label clinical research trial, Haroutounian et al. (2016) found treatment of chronic pain with medicinal cannabis resulted in improved pain outcomes and a significant reduction in opioid use.

[5]Scientific literature refers to marijuana as cannabis. I use the terms "marijuana" and "cannabis" interchangeably in this paper.

In addition to the clinical results above, there is suggestive evidence that medical marijuana patients change their opioid use in response to medical marijuana use. Studies involving surveys of medical marijuana patients report that the most common reason patients cited for using medical marijuana was the relief of pain (Reinerman et al. 2011; Reiman et al. 2017). Reinerman et al. (2011) find 79.3% of the medical marijuana patients reported having tried other medicines presented by their physicians and almost half of them were opioids. Reiman et al. (2017) find 30% of their sample reported using an opioid-based medication currently or in the past six months and out of those 61% reported using it with cannabis. More strikingly, they report that 92% of the sample "strongly agreed/agreed" that they prefer cannabis to opioids and 93% "strongly agreed/agreed" that they would be more likely to choose cannabis for opioids to treat their condition. Boehnke et al. (2017) find medical cannabis use was associated with a 64% decrease in opioid use among medical marijuana patients with chronic pain between 2013 and 2015 in Michigan.

Although there is some evidence that availability of marijuana decreases the use of opioids, it is hard to extrapolate these results from the above studies to wider populations since their conclusions are based on small and selected samples that rely on self-reported outcomes.

1.2.2 Effects of MMLs and contribution of this study

Although the literature on MMLs is rich, the effects studied are mostly focused on their unintended consequences, such as recreational marijuana use, alcohol consumption, initiation by youth, drunk driving, cigarettes and other substance use. Lynne-Landsman et al. (2013) show no effects of MMLs on adolescent marijuana use in the first few years after their enactment using the National Youth Risk Behavioral Surveys (YRBS). Anderson et al. (2015) revisit the relationship using data from the national and state YRBS, Treatment

Episode dataset, and National Longitudinal Survey of Youth 1997. They find MMLs were not associated with an increase in marijuana use among teenagers. Anderson et al. (2013) found a significant and negative relationship between MML and traffic fatalities, especially for those involving alcohol. Pacula et al. (2015) re-examine the effects of MMLs on recreational marijuana use by adult and youth populations and they also examine different provisions of MMLs. They report that treating MMLs as one dichotomous variable hide the effects of different provisions of MMLs. They show that not all MMLs are the same and the provisions of the law matter. In particular, they find that the MMLs that legally protect dispensaries can increase recreational marijuana use and abuse among adults and youth compared to MMLs that do not protect this supply source. Wen et al. (2015) show estimates from the National Survey on Drug Use and Health (NSDUH) and report that MMLs increase marijuana use and abuse among people who are 21 and older and initiation in younger populations. They also find MMLs increase binge drinking for 21 and above but have no effect on psychoactive substance use in either age group.

The MML literature on problematic opioid use is less comprehensive. Bauchhuber et al. (2014) examined state-level death certificates in the U.S. between 1999 and 2010 and found that states with MMLs had lower mean annual opioid overdose mortality rates compared with states without them. Powell et al. (2015) studied the effects of MML on problematic opioid use and found that broader access to marijuana reduced the abuse of highly addictive painkillers. Smart (2015) finds that growth in the supply of medical marijuana decreases opioid poisonings for adults between 45 and 64 by 12-16%. Yuan (2017) finds MMLs were associated with 23% and 13% reductions in hospitalization related to opioid abuse and overdose respectively. These studies all involve outcomes of people on the margins of abusive and possibly non-medical use. In this paper, I will show the effects on outcomes involving prescribed opioid use from a sample which represents the U.S population more broadly, and not from a population of opioid abusers.

The literature examining the effects on prescription drug use more broadly is very limited. Bradford and Bradford (2016) examined data on all prescription drugs filled by physicians for the Medicare Part D enrollees from 2010 to 2013. They find that MML implementation led to significant reductions in daily doses filled per physician in seven drug categories for which marijuana can serve as an alternative. These conditions include anxiety, depression, nausea, pain, psychosis, seizures and sleep disorders. In another paper Bradford and Bradford (2017) find significant negative associations between the presence of MML and quarterly logged average prescription units filled for the aforementioned drug categories among the Medicaid population from 2007 to 2014.

I extend the studies from the Bradford and Bradford articles in several ways. First, my analyses span 1996 to 2014, giving me a richer source of policy variation. During those 19 years, 23 states and D.C implemented MMLs and this relatively longer time horizon also enables me to estimate the long run effects of MMLs. Second, my observations are representative of the U.S population instead of consisting of patients on Medicaid and Medicare with positive spending. I will show the effects of MMLs on the extensive and intensive margins separately. It is plausible that MMLs affect prescription use differently on these two margins since the decisions on the probability of use and amount of use are decided by different agents. Third, this paper will investigate isolated effects of different MML provisions. Prior research suggests heterogeneity in MMLs lead to different effects which indicates that the design of these laws is essential in analyzing the costs and benefits of MMLs. Lastly, I focus explicitly on the utilization of opioids, defining utilization in terms of expenditures and pills both. Knowing how MMLs change the utilization of prescribed opioids and other prescription drugs is not only important for the analysis of MMLs but also important within the context of the growing trend of prescription drug costs and the costs associated with their misuse, such as the recent epidemic of opioid abuse.

1.3 Theoretical Framework

There are many mechanisms through which MMLs and their provisions can affect the demand of prescription drugs for which marijuana can be a substitute. The first and most obvious effect would be that patients with these conditions will seek their physicians' recommendation to substitute their prescriptions with marijuana. However, having a MML in place may also encourage a fraction of people who also had the conditions/symptoms but for some reason did not visit a physician before a MML was enacted. Enactment of a MML may serve to inform these people about their existing conditions and to seek medical help just like how direct-to-consumer advertising of prescription drugs would. Due to information asymmetry, the physician is the agent of the patient and she will make the decision whether to and if so, how much to prescribe/recommend an FDA-approved prescription or medical marijuana. Given marijuana's classification as a Schedule 1 drug, and the resulting absence of scientific evidence and incentives that the physician would have if she prescribed prescriptions supplied by the pharmaceutical firm (low cost of information due to heavy advertising/detailing/scientific evidence/habit formation, less risk), some physicians will be reluctant to substitute it.

Following Brekke et al. (2006), I assume there is a continuum of patients with a condition in a therapeutic drug market which marijuana can have a potential to treat on the line segment [0, 1]. The location of the patient x $\in$ [0,1] is associated with his condition and personal characteristics. They all need either a prescription drug (Rx=0) or medical marijuana (m=1). Rx and m are located at the either ends of a unit interval [0, 1] and are indexed as i. This classification of 0 and 1 only reflect their chemical compounds and the treatment effects. I assume the patient's utility takes the following linear form when he takes the treatment i:

$$U^{patient}(x, i) = \upsilon - \tau|x - i| - C_i \tag{1.1}$$

where the parameter υ represents the effectiveness of drug i. I assume that both treatments Rx and m have the same effectiveness but they differ in their treatment effects to a given x. τ represents the weight given to the utility loss that is realized due to the mismatch between the condition x and the treatment choice (the distance between the condition and the treatment choice). These can be thought of as side effects. I assume that υ and τ are both positive. C_i represents the out-of-pocket cost for the treatment.

Consider a population of people who have a condition and let $z \in [0,1]$ be the fraction of patients who already saw a doctor related with their condition and (1-z) the fraction of patients who have the condition but did not see a doctor yet (potential patients). When states adopt MMLs this can serve as a marijuana advertisement inducing some of these potential patients to be aware of their conditions and encourage them to go to the doctor's office. Let $\phi \in [0,1]$ be the fraction of patients who receive information about the legalization of medical marijuana in their state. I assume all patients need a treatment, whether medical marijuana or a prescription drug. Only potential patients who have not heard about MMLs will not go to a doctor's office. The fraction of patients who go to the doctor's office for treatment is then N=z+(1-z)ϕ.

I assume all physicians face the same distribution of patients. Once the patient goes to the physician, the physician asks questions to determine the patient's type; his location $x \in [0,1]$. After observing the patient's type the physicians can either recommend medical marijuana or prescribe a drug. I assume there are two type of physicians. The first physicians who will not recommend medical marijuana no matter how much the patient insists; I call them "Type 1 physician" and denote their share as θ. The second is physicians who are willing to recommend marijuana if the patient insists. I call them "Type 2 physicians" and their share is (1-θ).

Consider a type 1 physician who will not recommend marijuana in any case. I assume her utility function takes the linear form below;

$$U^{physician}(x, Rx) = b_{Rx} + \gamma U^{patient} \tag{1.2}$$

where b_{Rx} denotes the private benefit she receives from prescribing the prescription drug and γ denotes the weight she puts on her patient's utility. Plugging the patient's utility given in equation (1) type 1 physician will prescribe Rx to the patient x only if the following is true:

$$U^{physician}(x, Rx) \geq 0 \iff b_{Rx} + \gamma\upsilon - \gamma\tau x - \gamma C_{Rx} \geq 0 \tag{1.3}$$

If $U^{physician}(.)<0$, then the physician will recommend a different treatment or no treatment at all. Consider a type 1 physician who is indifferent between prescribing and not prescribing. Solving (3) we get

$$\tilde{x} = \frac{b_{Rx} + \gamma\upsilon - \gamma C_{Rx}}{\gamma\tau} \tag{1.4}$$

She will prescribe the drug if the patient x is on the interval $[0,\tilde{x}]$ and not prescribe if the patient is between $[\tilde{x},1]$.

Now consider a type 2 physician who considers marijuana as an alternative to Rx. She will recommend marijuana (m) instead of Rx only if the following condition holds:

$$\begin{aligned} U^{physician}(x, m) \geq U^{physician}(x, Rx) \iff b_m + \gamma(\upsilon - \tau(1-x) - C_m) \\ \geq b_{Rx} + \gamma(\upsilon - \tau x - C_{Rx}) \end{aligned} \tag{1.5}$$

where b_m denotes the private benefit (or cost – e.g., her time cost of searching for information about marijuana or the cost of writing a recommendation letter) the physician gets from

recommending medical marijuana and C_m denotes the financial cost of medical marijuana to the patient. Let $\tilde{x}$ denote the patient for whom the physician is indifferent in recommending m vs. prescribing Rx. By solving (4) we get;

$$\hat{x} = \frac{1}{2} - (\frac{\gamma(C_{Rx} - C_m) + b_m - b_{Rx}}{2\gamma\tau})$$

This means the physician will recommend marijuana if the patient x is located on $[\hat{x},1]$ and prescribe Rx if he is on $[0,\hat{x}]$. Since the physician will not recommend m or prescribe Rx if her utility is not positive the condition

$$b_m + \gamma(\upsilon - \tau(1 - \hat{x}) - C_m) = b_{Rx} + \gamma(\upsilon - \tau\hat{x} - C_{Rx}) \geq 0$$

must hold. This is satisfied when $\tilde{x} \geq \hat{x}$.

Proposition 1 *Entrance of medical marijuana as another treatment option will decrease the 'mismatch' between a given therapeutic condition and the prescription drug substituting marijuana with prescription drugs.*

Proposition 2 *Substitution effect; $\tilde{x}$-$\hat{x} \geq 0$ will be higher for more expensive drugs and/or for drugs which treat conditions that are not a good match with the prescription drug (or drugs with more severe side effects).*

Proposition 3 *In states where the patient's cost of obtaining marijuana is lower (small C_m) and physician's benefit of recommending it is higher (or lower cost of recommending, high b_m), more prescription drugs will be substituted.*

From the physician's choices above we can derive the shares of patients who get Rx and m respectively,

$$M_{Rx} = [z + (1 - z)\phi] * [\theta\tilde{x} + (1 - \theta)\hat{x}] \; and,$$
$$M_m = [z + (1 - z)\phi] * [(1 - \theta)(1 - \hat{x})].$$

If a MML was not enacted the share of the patients who would be on Rx would simply be $z\tilde{x}$. The difference between the share of prescription drugs after and before the MML then would be $\tilde{x}[\theta\phi(1\text{-}z)+z(\theta\text{-}1)]+\hat{x}(1\text{-}\theta)[z+(1\text{-}z)\phi]$. A high enough $\theta\phi$ (the fraction of new patients who visit the type 1 physician) could increase the prescription drug shares after the MML.

Proposition 4 *If the share of new patients that visit the type 1 physician ($\theta\phi$) is high enough prescription drug utilization can increase after the MML.*

Proposition 5 *For prescription drugs which are already a good match with a given condition (less severe side effects), utilization can increase after the MML.*

1.4 Estimation

To determine the effects of MMLs on prescription drug spending I use prescribed medicine event-level data linked to person level data from the Medical Expenditure Panel Survey (MEPS) spanning 1996 to 2014. Starting from 1996, MEPS collects detailed information for each person in selected households. This information includes demographic characteristics, health insurance coverage and income. MEPS Prescribed Medicine Files contain pharmacy-provided information on names of prescribed medicines obtained, their therapeutic class and sub-class, total amount paid for the prescribed medicines and source of their payments for each time a prescription drug was purchased.

The MEPS is a nationally representative panel survey and it has an overlapping panel design. A new panel of sample households is selected each year and they are surveyed for two years. I acquired the unrestricted version of MEPS with state identifiers and merged the state-and year-level MML variables. As seen in Table 1.1, 23 states and D.C implemented MMLs during the study period.

Since the literature suggests that there is relatively stronger evidence of marijuana as a painkiller and the fact that the majority of medical marijuana patients use it for their pain, specifically preferring it to opioid-based painkillers, I choose the main outcome variable as the total amount of dollars spent on opioid-based medicines. Focusing on opioids is also important from a policy perspective considering the costs associated with opioid misuse.

The key independent variables are indicators for MML implementation (effective dates) in a given state and year and its individual components. As noted by Pacula et al. (2015), MML states differ highly in how they allow medical marijuana and ignoring the heterogeneities in these policy dimensions that exist both across time and states can mask their heterogeneous effects and the mechanisms through which MMLs affect utilization. Following Pacula et al. (2015) and Wen et al. (2015), I analyze the effects of four key components that can lead to heterogeneity in prescription drug utilization: i) a "retail dispensary" provision, an indicator of whether the state's MML explicitly allows/protects dispensaries to dispense marijuana to medical marijuana patients, ii) a "home cultivation" provision, an indicator of whether a state's MML allows the medical marijuana patient to cultivate a certain amount of marijuana, iii) a "non-specific pain" provision, an indicator of whether the state's MML lists any chronic pain or intractable pain in the eligible conditions for medical marijuana instead of specifically listing the conditions associated with the pain, and iv) a "patient registry" provision, an indicator for whether a state's MML requires the patient registry. These provisions can directly determine both the monetary and search

costs of obtaining medical marijuana of the patient as well as marijuana's perceived risk and appropriateness for recommendation from the physician's view.

I control for individual and state level factors that are correlated with prescription drug spending and with state decisions about MMLs. Individual-level covariates include a rich set of sociodemographic and economic characteristics. State-level covariates include four time-varying measures reflecting the variations in state economic conditions between 1996 and 2014: i) state unemployment rate, ii) state median household income, iii) state average personal income, and iv) state uninsured rate. I include two policy variations during the study period that can affect prescription drug spending and MML implementation. These state-level policy variables include indicators for operational prescription drug monitoring programs (PDMPs) and the implementation of a marijuana decriminalization/depenalization in a state.

After pooling all the year, collapsing the prescribed opioid transactions at the year- and person-level and excluding people under the age of 18, I have a sample of 435,035 person level observations. Tables 1.2 and 1.3 show the summary statistics for dependent and independent variables.

1.4.1 Data characteristics and two-part model

Like other health care utilization data, prescription drug utilization distributions tend to be skewed because 1) there cannot be negative spending, 2) there is a mass at point zero for non-users, 3) patients with more severe conditions use substantially more on prescription drugs than those with less severe conditions, and 4) there can be a small number of patients with astronomical spending due to catastrophic health conditions. Health economists often use log-transformed models to deal with these types of skewed outcomes. Other approaches include more flexible methods of conditional density estimation or estimation with GLM.

Certain transformations such as logging are not appropriate, especially when there is a large mass of zeros. First, adding an arbitrary constant to observations is not recommended, and second, using one-part models implicitly assume that observations with zero outcomes are similarly affected by covariates as nonzero outcomes. These models are shown to behave poorly compared to multi-part models (Duan et al. 1983; Mihaylova et al. 2011).

Due to the presence of the zero mass of non-users in the data, I use a two-part model approach. The two-part model splits the prescription spending into two parts and applies the basic rule of probability in estimating the parameters in the conditional mean function $E(y|x)=Pr(y>0|x)\times E(y|y>0,x)$.

Figure 1.1 shows the nonlinearities in the distribution of opioid spending. There is a large mass of non-users (approximately 90%), and the spending from users is skewed to the right even after logging.

Since health care utilization data show heteroscedasticity, a re-transformation that assumes homoscedastic, normally distributed log-scale error terms will give biased results. Due to the complications that can arise with estimating the correct form of heteroscedasticity, I avoid using OLS on logged outcomes with heteroscedastic retransformation and use GLM for consistent estimation instead. The advantages of using GLM compared to models with transformations are more broadly discussed in Manning and Mullahy (2001) and Jones (2000).

GLM extends the classical linear models in two ways. First, it allows the dependent variable to be distributed with any exponential family. Second, it allows for any monotonic differentiable function of the dependent variable to vary linearly with the covariates (the link function), rather than requiring the dependent variable itself to respond linearly (McCullagh and Nelder 1989). Another advantage of using GLM is that it gives predictions on the raw scale since it does not transform data and it also allows for heteroscedasticity. Modeling

health care utilization and costs with GLM is a common approach in the literature (e.g. Goda et al. 2011; Chandra et al. 2014; Strumpf et al. 2017).

For the baseline model, I use probit estimation, shown below, to estimate the probability of being a prescription drug user:

$$Pr(Y_{iast} > 0 \mid X) = \Phi(X\beta)$$

where Y_{iast} is the binary variable equal to one if the consumption for a person i living in state s in year t for the drug category a is positive and zero otherwise. X is a vector of explanatory variables including all the control variables in Table 3, state and year fixed effects and state-specific linear time trends to capture the state-level factors that evolve over time at a constant rate.

For the intensive margin, I use GLM models with log-link and gamma family to estimate the amount of spending conditional on being a user as shown below:

$$E(Y_{iast} \mid Y_{iast} > 0, X) = \exp(X\gamma)$$

where Y_{iast} denotes the prescription drug spending for person i for drug category a in state s and year t, while X denotes the same vector of covariates as in the first part.

As suggested by Manning and Mullahy (2001), I used modified Park tests to determine the relationship between the conditional variance and the conditional mean functions, namely the parameter δ in $Var[Y_{iast}|Y_{iast}>0,\mathrm{X}]=\alpha[E(Y_{iast} \mid Y_{iast} > 0, X)]^{\delta}$. In all drug cases, $\hat{\delta}$ was closest to 2 implying the gamma family.

Standard errors in all regressions are robust to heteroscedasticity and they are clustered at the state level to correct for serial correlation. The clustered standard errors allow

the errors to be correlated within states while allowing them to be independent across states (Bertrand et al. 2004).

As the main results, I report the combined marginal effects from both parts of the model[6]

$$E(Y_{iast} \mid X) = Pr(Y_{iast} \mid X > 0) \times E(Y_{iast} \mid Y_{iast} > 0, X)$$

This setup models the difference in utilization on the original scale of the dependent variable (dollar amount) yielding estimates that are readily interpretable. It also allows for heteroscedasticity where *Var(Expenditure|X)* depends on the mean level of conditional expenditures, *E(Expenditure|X).*

I also report the results from probability of use and amount of use separately. It is possible that MMLs (and their provisions) have opposite effects on each margin of use, especially if they act as an advertisement and encourage people to visit doctors who then prescribe them drugs, increasing the probability of utilization, while decreasing the amount of utilization by the users that are already on these drugs. If MMLs have opposite signs in different parts, then it would be possible for the marginal effect to be significant in isolated parts of the model along with the combined marginal effect being insignificant.

According to the CDC, prescription drug utilization is highest for people age 65 and older, and there are substantial differences in utilization based on age. I stratified the sample into three age groups because prescription drug utilization varies largely depending on age, and lumping everyone in the same sample obscures this heterogeneity (Kantor et al. 2015). The samples are ages 18-39 (N=186,144), 40-64 (N=180,723) and 65 and older (N=68,168). Because there are stricter barriers for minors to obtain medical marijuana and the fact that they are much less likely to have the conditions for which marijuana can be beneficial, I exclude people younger than 18 from the sample.

[6]I used STATA's twopm command developed by Belotti et al. 2015 to obtain the combined marginal effects and their standard errors.

1.5 Primary Results

Table 2 presents the means of the main outcome variable of opioid spending along with the drug categories that marijuana can potentially replace for the full sample. Both the probability of any spending and the amount of spending conditional on positive spending on opioids and other potentially marijuana substitutable prescriptions are lower in MML states compared to control states.

To determine whether these differences are driven by MMLs, I estimate two different models. First, I show results from the models that only include any MML, and in the second I report the results from the model which only include its provisions. I also report a model that simultaneously estimates all provisions and MML, but due to collinearity when the fixed effects are included, I do not report these results as main findings.[7]

For my analyses I show results from two-part models instead of OLS on the whole sample for three reasons. First, many people in these samples do not use these prescription drugs, and two-part models explicitly model this large mass of non-users. Second, the two-part model yields lower Akaike information criterion. Third, the two-part model gave better out-of-sample predictions compared to OLS. I also run joint significance tests where the null hypothesis is the coefficients from the four provisions of MMLs are jointly equal to zero and report their p-values. I perform these tests for the models that include indicators for all provisions and an indicator for existence of any MML. The motivation is to test whether these provisions jointly explain variations which are not captured by a generic MML indicator.

Tables 4 through 6 show the effects of a MML and its provisions on the different margins of opioid spending among different age groups. According to the results in Table 4, a MML has no discernible effect on the probability of using opioids in young adults (ages 18-39). Although the coefficient on "any MML" is positive, it is insignificant. Similarly,

[7] Also, the interpretation of "any MML" becomes difficult in this model. These results are available upon request.

none of the provisions show any discernible effects. However, there is a significant decrease in opioid spending on the intensive margin. Namely, among young adult users of opioids there is a decrease of $37.46 per person over a year associated with the passing of MML, which translates to a 53.7% decrease from the baseline mean of opioid expenditures. Looking at the model which includes its provisions we can see that "home cultivation" is the main driver of this decrease with an even larger and significantly negative effect. Although "retail dispensary" has negative effects, its coefficient is not precisely estimated. The last two columns in Table 4 report the combined effects of MML and its provisions on the overall population bringing the two parts together. Implementation of a MML significantly lowers opioid spending in the overall population of young adults by $2.47 per person over a year. Focusing on the effects of individual provisions in states where home cultivation is allowed, young adults use $4 less of opioids per person holding all other provisions constant. The "home cultivation" provision appears to be the main driver of the decreasing effect of MML on opioids among young adults and these effects result from the intensive margin of use. Tables 5 and 6 show there is not much evidence that a MML and its provisions significantly change opioid spending among middle age (ages 40-64) and elderly people (ages 65+). The only significant effect is found among middle age people. Namely, in states where the law allows retail dispensaries, there is a 1.4% point drop in the probability of using opioids among this group when we hold the other provisions fixed (a 14% decrease from the baseline mean). As pointed out by earlier literature, most medical marijuana patients are younger adults so it makes sense that we see a significant drop in opioid spending among younger populations and almost no effect among older people.

The above analyses show independent effects of the four provisions, but states have combinations of these provisions. Table 7 shows linear combinations of the marginal effects from various combinations of the four provisions for each age group on the overall spending of opioids consisting of both parts. First, I examine the linear combinations of the marginal

effects of "home cultivation," "non-specific pain" and "retail dispensary" provisions. California is a state with this type of MML. California's type of MML is effective in reducing opioid spending among young adults by $3.86 per person over a year and has no effect on older populations. Second, I examine the effects of "retail dispensary," "home cultivation," "non-specific pain" and "patient registry" provisions. Colorado is an example of a state with such a MML. Colorado's type of MML is effective in reducing opioid spending among young adults by $3.27 per person over a year. Next, I examine the combined effects of "retail dispensary," "non-specific pain" and "patient registry" provisions (New Jersey-type) and combined effects of "home cultivation," "non-specific pain" and "patient registry" (Alaska-type). Both New Jersey's and Alaska's types of MML are not associated with any significant decreases in reducing opioid spending.

A California-type MML which allows home cultivation, legalizes and protects dispensaries, and imposes no restrictions such as having a specific type of pain to be eligible or requiring a registry of the patient is one of the least strict types of MML.[8] It is also the type of MML that reduces opioid spending the most among young adults, as measured by the amount of dollar reduction in this study. Although not as loose as California's MML, Colorado's type of MML is also one of the loosest models and associated with decreases in opioid spending comparable to California's.

These results from the combined effects of provisions indicate the effects of MML are not uniform but depend on the different combinations of provisions, consistent with Pacula et al. (2015). The types of MMLs with the most generous provisions, which include the protection and allowance of dispensaries with home cultivation, seem to be the most effective types of MMLs in decreasing spending on opioid prescriptions.

[8]I also check whether the overall reductions in opioids were driven by California alone. The estimates from models excluding California show similar and even slightly larger estimates in magnitude. These results are available upon request.

1.6 Additional Analyses

Up to this point I have shown that the response of total prescription opioid expenditures to MMLs depends on the age of the users and the margin of use. The point estimates from combined marginal effects point decreases in spending on prescription opioids associated with MMLs among young adults. To further assess the validity of this finding, I perform two types of sensitivity and two other additional analyses by exploring (i) the timing of the policy implementation and policy endogeneity, (ii) the effects of MMLs on the number of total opioid pills acquired instead of expenditures, (iii) the effects of MMLs and their provisions on spending on other prescription drugs for which marijuana can potentially be used as a substitute and (iv) the effects of MMLs and their provisions on prescription drugs for which MMLs are not supposed to have any effect.

1.6.1 Event studies

Here I replicate my baseline specification with two-part models for expenditures on opioids adding lead and lag indicators. This flexible event study approach enables me to investigate whether there are any pre-existing trends in opioid expenditures which are endogenous to MML adoption. Furthermore, it shows if the law has differential effects over time after a MML is adopted. I exclude the indicator for the last year prior to MML adoption and set it equal to zero for normalization.

Figure 2 shows the estimated average marginal effects of the timing of the intervention within four or more years before and after for each age group. The results for young adults indicate there is a drop in prescription opioid expenditures after a year following the MML adoption (relative to the year prior to adoption). The decreasing effect of a MML becomes statistically significant after two years following the year it takes effect and continues to be significantly negative even after four years or more, with its magnitude reaching its

maximum after three years. The decreasing impact of MML on opioids among young adults is persistent over time with the long run difference being even larger than its instantenous effect. There is not evidence of pre-existing trends: prior to intervention the effect of a MML is indistinguishable from zero.

Turning to middle age and elderly populations there is not much evidence supporting the hypothesis that a MML changes prescribed opioid expenditures over time. Among the elderly population, a MML increases the opioid utilization after the first year of its adoption (relative to the year prior to adoption), but this estimate is barely significant and it dissipates the following years. There is not evidence of pre-trends before MML implementation in either of these age groups.

These results from event study analyses support the main findings that MML implementation decreases the spending of prescription opioids among young adults but does not have any discernible effect on older populations.

1.6.2 Effects on total number of opioid pills

So far, all the analyses were concerned with the expenditure outcomes for prescription opioids. Although total expenditure is an important outcome from a government budget spending perspective, it is not the only or the most complete measure of utilization. To investigate whether the spending decreases in opioids associated with MMLs are attributed to use rather than heterogeneous prescription drug prices, I perform analyses on total number of prescribed opioid pills purchased using MEPS prescribed medicine files. Despite being an imperfect measure of utilization, total number of prescribed opioid pills obtained can provide some insights for the mechanisms of the effects found in main results.

Table 8 shows the average marginal effects of MMLs from two-part models on total opioid pills for the same age groups. Turning to results for the young adults on Table 8, the

decreasing effect of "any MML" on opioid utilization remains. Namely, the mere adoption of MML decreases the number of prescription opioid pills in the young population by 2.16 pills per person over a year, which is a 27% decrease from the baseline mean. We can see that decreases from "any MML" on opioids among young adults mainly result from the effects from "home cultivation" and "retail dispensary" provisions. The effects of MML and its provisions are null among the older populations when the outcome variable is number of pills instead of total expenditures.

Comparing these results we see that the effects of MMLs found on opioid pills support the primary results found on the opioid expenditures: implementation of MML decreases opioid utilization among young adults.

1.6.3 Effects on the utilization of other prescription drugs

Although the majority of medical marijuana patients report using marijuana for pain, there exists suggestive evidence on marijuana's effects on other health conditions. Furthermore, Reinerman et al. (2011) report the other common reasons patients cite for using medical marijuana were muscle spasms, headache and anxiety. Reiman et al. (2017) report mental health conditions were the second most common reason for using medical marijuana after pain. In the light of these findings I study the effects of MMLs on other prescription drugs for which marijuana can be a potential substitute.

The non-opioid prescription drugs I examine fall under four major groups: non-opioid painkillers, antidepressants, anticonvulsants and sedatives. These categories of drugs are commonly prescribed and they treat the conditions medical marijuana states render eligible. They are also examined by earlier studies (Bradford and Bradford 2016 and 2017). If MMLs are causing people to switch from their prescriptions to medical marijuana, utilization of these drugs must show the biggest change. However, Bradford and Bradford's 2016 and 2017

analyses only include Medicaid and Medicare recipients who incurred positive expenditures of prescriptions. Here, l extend the analyses to a broader population.

Tables 9 through 11 show the combined marginal effects of "any MML" and MMLs' four main provisions on expenditures for other marijuana-substitutable prescriptions for each age group. The mere implementation of a MML has no impact on other drugs, except a barely significant spending decrease in sedatives among young adults by $1.47 per person over a year and a significant decrease in sedatives among the elderly by $6.75 per person over a year.

Focusing on the effects of the four main provisions of MMLs, "retail dispensary" and "home cultivation" provisions are generally associated with significant decreases on antidepressant and anticonvulsant expenditures among young adults and the elderly. Having a "non-specific pain" provision in a state's MML is associated with a significant increase in sedative spending among young adults. This increase in sedative spending results from the increase in the extensive margin: having a "non-specific pain" provision increases the probability of sedative use significantly. This could be attributed to "non-specific pain" provision's creation of ambiguities in eligibility criteria and extension of the patient base to people with relatively milder pain (or no pain), who later end up being prescribed other prescriptions upon seeing the physician. In fact, a "non-specific pain" provision also significantly increases the probability of using antidepressants and anticonvulsants among young adults.[9]

Having a "patient registry" provision offsets the increasing effects of a "non-specific pain" provision in young adults' sedative spending by decreasing it by $4.51 per person over a year. It also decreases the elderly's sedative spending by $10.18 per person over a year. The decreasing effect of a "patient registry" provision seems odd at first, but it could be due to three reasons. First, requiring the registration of the patient could make the recommendation of marijuana less risky from the physician's viewpoint, decreasing her cost

[9]These results from extensive margin of use from other prescriptions are in Appendix Table 1A.

of recommending it. Similarly, being registered by the state and having a medical marijuana patient identification card can decrease the patient's risk of arrest from carrying marijuana. Looking at tables 9 and 11, it is natural to ask why sedatives are the drug category that is most sensitive to these provisions in young adults and elderly. As pointed out in Proposition 5 MMLs' effects depend on the "mismatch" (or side effects) associated with a prescription drug class and the health condition it treats. Sedatives along with opioids are reported to be a class of drugs with the most severe side effects.[10] Furthermore, mental health conditions and anxiety are found to be the second most commonly reported reason for using medical marijuana among medical marijuana patients (Reinerman et al. 2011 and Reiman et al. 2017). Therefore, a MML and its provisions can decrease sedative utilization more relative to other categories of drugs with less severe side effects for which marijuana can substitute.

1.6.4 Placebo tests

Here, I check the effects of MMLs on drug classes for which marijuana has no potential to substitute. I perform these analyses to demonstrate that negative effects of MML only exist for the drug classes for which marijuana can be a substitute and not for the other drugs. Tables 12 through 14 show results for some of the other commonly prescribed drugs on which MMLs should not have any negative effect. The commonly prescribed placebo drugs include hormones, hypertension drugs, cardiovascular agents and acid reducers. The results generally support the hypothesis that MMLs and their provisions do not decrease expenditures on other drugs, although there are some statistically significant increases, especially with middle age and elderly people. "Patient registry" is linked with decreasing spending in one of the tests, although it is only marginally significant at the 10% significance level.

[10] According to the CDC, sedatives were involved in 31.7% of drug-poisoning ER visits between 2008 and 2011. Hampton et al. 2014 find sedatives made up most of the adverse drug event related ER visits between 2009 and 2011 compared to all other psychiatric medications.

1.7 Conclusion

This paper shows that implementation of a MML by itself decreases opioid utilization among young adults significantly, whether utilization is defined as spending or the number of pills. Most of these reductions result from the intensive margin of utilization. The decreasing effects of MMLs on opioids among young adults are persistent over time. They continue to decrease opioid spending among young adults even four or more years after the year of their implementation. The decreasing effects of MMLs are only observed among young adults except for the allowance of retail dispensaries which decreases the probability of use among middle age adults. MMLs also decrease sedative spending among the elderly. Given that opioids and sedatives are the drug classes associated with the most severe cases of addiction and adverse drug events, MMLs can be useful in alleviating the problematic use of these prescriptions. Consistent with the prior literature, ignoring the heterogeneity in MMLs can mask important effects of their individual provisions. States with the loosest MMLs experience the biggest reductions in opioid utilization.

Despite growing trends of pro-marijuana policies, there remains a lack of scientific evidence and consensus as to what extent marijuana affects health in the short and long terms. Unlike prescription drugs, there are almost no guidelines on how to use marijuana for medicinal purposes regarding its dosage, type, frequency and the method of its consumption. Although states have been experimenting with different MMLs since 1996, conducting randomized controlled experiments on marijuana with human subjects remains challenging given its Schedule 1 categorization by the federal government.

There are several policy implications from this study. First, non-MML states with high rates of opioid abuse and adverse drug events especially stemming from young adults should look more carefully into adopting MMLs. Second, MML states should consider the consequences of having different provisions since MMLs with restrictive supply channels are

less likely to experience utilization reductions in prescribed opioids or other prescription drugs, while less restrictive supply policies increase recreational use and abuse as found by Pacula et al. (2015). This implies states should weigh the pros and cons of different provisions when they design their MMLs according to their needs. Lastly, more research is needed to inform policy makers on identifying the characteristics of medical marijuana patients and why and how they use and substitute it. More randomized clinical trials are also needed to assess the effects of marijuana on health so that physicians and patients are more clear on how to use it effectively.

Chapter 2

The Impact of Universal Coverage on Health Care Utilization: Evidence from Massachusetts

2.1 Introduction

The price elasticity of health care utilization is a great concern for economists and policymakers. Although there are a few randomized controlled trials investigating the impact of cost sharing on health care utilization like the RAND and Oregon health insurance experiments, these studies are outdated or limited to certain populations. Moreover, recent reforms in health care markets raise questions about the impact of these insurance expansions on utilization and health in the long term. The most recent insurance expansion is the Affordable Care Act (ACA) which was enacted nationwide in 2014. However, years before the ACA a very similar health insurance expansion reform took place in Massachusetts in 2006. The goal of the Massachusetts health care reform of 2006 was to achieve universal health insurance by "incremental universalism", which meant it would reduce the uninsurance rate by filling the gaps in the existing system rather than ripping up the system and starting over (Gruber, 2008). According to Gruber, incremental universalism was composed

of three strategies called the "three-legged stool" which had to be taken together to achieve universal coverage (Gruber, 2011).

The first leg of the "three-legged stool" strategy was to regulate insurance markets to extend insurance to people who were previously priced out of the market due to their health. This meant the insurers were no longer allowed to deny or drop coverage based on pre-existing conditions or charge differential premiums based on health conditions, except smoking status and age (community rating) (Kirk, 2000; McDonough et al., 2006, Courtemanche and Zapata, 2014). Massachusetts' health insurance exchange, The Commonwealth Health Insurance Connector Authority, offered health insurance to those without access to group markets. The Connector began to enroll people with incomes less than 100% of the federal poverty line (FPL) in October 2006, then extended enrollment to those with incomes less than 300% FPL in January 2007, and finally extended it to everyone in May 2007 (Courtemanche and Zapata, 2014). Also, the reform mandated that family policies must maintain dependent children for two years past loss of dependency or to age 25 (McDonough et al., 2006).

The first leg alone would result in adverse selection and lead to market failure with very high premiums which would drive the healthy people out of the market (the death spiral). Therefore, the second leg of the stool mandated that everyone had to have health insurance and employers had to offer health insurance to their employees. Individuals without appropriate health insurance would face penalties that cost half of the cheapest health insurance offered through Connector, and employers with more than 10 employees would face penalties costing up to $295 per full-time employee per year unless they offered health insurance. These mandates took effect in 2007 (Massachusetts Health Insurance Connector Authority, 2008; Courtemanche and Zapata, 2014).

Because these mandates would be very costly to middle and low income individuals, the third leg of the stool provided subsidies and expanded Medicaid. Health insurance was made free for people with incomes below 150% FPL and premiums were partially subsidized for

people with incomes between 150% FLP and 300% FPL with no deductibles. The reform also mandated that Medicaid would cover children below 300% FPL (McDonough et al., 2006; Courtemanche and Zapata, 2014). While some of the details were different, both the ACA and the Massachusetts reform were based on the three-legged stool strategy. However, Massachusetts had a lower pre-reform uninsured rate compared to other states. [1]

There is a rich literature studying how the reform impacted health insurance take-up, utilization of specific health care service outcomes such as ER visits, and self-assessed health status. This paper contributes to the existing literature in several ways. First, I estimate the impact of the reform on less-studied utilization outcomes such as counts of prescription drug purchases, inpatient, outpatient and office visits in addition to ER visits. Second, I examine the effect of the reform on health care expenditures on the external and internal margins. Third, I investigate the long run effects of the reform using a dataset with longer pre and post treatment periods. I find that the reform increased hospital and office visit counts and expenditures significantly. Namely, I find the reform increased total counts of health care service events by 1.2 and $942 per person over a year. The reform decreased the counts of prescription drug purchases, but increased their expenditures. I also find the reform's effects were not only long-lasting but they got stronger over time. In the long run, the reform increased counts of all health care service events significantly including prescription drug purchases.

The paper proceeds as follows. Section 2 summarizes the literature on Massachusetts' health care reform. Section 3 describes the data, and provides a motivation for the identification strategy. Section 4 further explains the identification strategy and model selection. Section 5 discusses the primary results. Section 6 presents several robustness checks to test the sensitivity of the main results, validate the assumptions of the identification strategy,

[1] Courtemanche and Zapata (2014) lays out a systematic review comparing the two reforms.

and examine the differential effects of the reform over time. Section 7 briefly summarizes the findings and discusses a direction for future research based on the findings.

2.2 Literature Review

Many papers have studied the effects of the Massachusetts health care reform on various outcomes. Long et al. (2009) found the reform decreased the uninsured rate by 6.6 percentage points in non-elderly adults. Long and Stockley (2011) found the Massachusetts reform decreased unmet needs because of cost and delays in obtaining care for the low-income population. It also increased use of health care for this population. According to Yelowitz and Cannon (2010), while the reform expanded the coverage overall, it reduced the private insurance coverage for the population that the reform targeted for government programs and had little effect on self- assessed health. Cogan et al. (2010) found that after the reform employer-sponsored insurance premiums increased by 6% compared to other states. Kolstad and Kowalski (2012) examine the effects of the reform on many outcomes. They show that the reform decreased the uninsured rate, length of stay in the hospital (especially for long stays) and emergency room admissions (especially among the poor population). The reform also decreased visits for preventable conditions. Miller (2012a) uses variation both across counties and states and finds that counties with a relatively more uninsured rate pre-reform experienced a larger increase in the insured rate. She also found that it reduced ER visits for the conditions that can be treated in a physician's office and the conditions that can be prevented with routine care. Miller (2012b) examined the effects on children's outcomes and found that the reform increased probability a child had any insurance, decreased the probability a child had an ER visit, increased the probability a child had an office visit and check-up and increased the probability that child was in excellent health. Courtemanche and Zapata (2014) examined a dataset with longer pre-and-post reform periods and found that

the reform increased both overall self-assessed health status and less subjective determinants of health status.

To sum up, most of the earlier literature focused on the outcomes of health, insurance rate, and utilization of specific services such as ER visits. To my knowledge, only Long et al. (2008) separately examined the effects on other health care utilization outcomes such as out-of-pocket spending for prescription drugs. In their paper, they compared the outcomes for a cross-sectional sample of a non-elderly population just prior to the reform to another cross-sectional sample of the same population right after the reform. They reported a significant decrease in out-of-pocket spending for health care and this decrease was largely driven by the decrease in prescription drug costs. This reduction was most prominent among low-income individuals.

Understanding changes in out-of-pocket payments due to the reform is key to understand its costs and benefits due to consumption smoothing. The RAND health insurance experiment in the 1970's found that more generous insurance plans increased spending on pharmaceuticals (Leibowitz et al., 1985). Finkelstein et al. (2012) used lottery selection in a randomized Oregon Medicaid expansion as an instrument for insurance and found that insurance coverage increased the probability of taking any prescription drugs. In addition, both the RAND and Oregon experiments showed that cost sharing increased health care utilization. The results from RAND showed that cost sharing increased the number of medical contacts (Manning et al. 1987). The Oregon experiment showed that having insurance was associated with an increase in the probability of any hospital admissions that do not originate from ER and increases in hospital days, total list charges and number of procedures performed (Finkelstein et al. 2102). The Massachusetts reform provides another way to examine the relationship between insurance and health care utilization as well as show the longer-term impacts of having health insurance on health care utilization.

The purpose of this essay is to find whether Massachusetts' health care reform changed total use of and spending on health care services. This paper will extend the existing literature by examining the long-term impacts of universal coverage on prescription drug utilization as well as other health care utilization. The results can shed light on the mechanisms through which the reform led to better health, a consistent finding in this literature.

2.3 Data

I estimate the effect of the reform on health care utilization using the unrestricted version of the Medical Expenditure Panel Survey (MEPS), a set of large-scale surveys of families and individuals and their medical providers in the United States. The MEPS is suitable for my analysis because it collects data on demographic characteristics, income, and insurance coverage as well as health care services including prescription medicines, inpatient, outpatient, medical provider office visits and ER visits each time they are utilized and their related expenditures. More detailed information including the type, payment and source of payment for each utilization event is also collected by their medical providers. I use MEPS waves from 2000 to 2015 which enables me to uncover the long-term effect of the reform on health care utilization as well as pre-reform trends. I acquired state-identifiers of MEPS from the Agency of Health Care Research and Quality (AHRQ) to add state controls.

I construct the sample by pooling all years between 2000-2015 from the MEPS consolidated household files which are merged to event-level files from their health care use events. The treatment group consists of individuals living in Massachusetts and the control group is the rest of the country. Because the reform prevented people with Medicare from purchasing insurance through the Connector (Blue Cross Blue Shield of Massachusetts, 2006) it did not change health insurance coverage for the elderly. Following prior literature (e.g. Long, 2008;

Miller, 2011a; Yelowitz and Cannon 2010; Courtemanche and Zapata, 2014) I only include the non-elderly population ages 18 to 64.

The main health care utilization outcomes include total counts of and total expenditures on prescription drugs and, inpatient, outpatient, office and ER visits utilized by an individual over a year. I will also aggregate all these events both at the count and expenditure level and estimate the effect of the reform on total utilization. It is important to estimate both the count and the expenditure of the health care utilization outcomes because estimating only the effect on expenditures alone would not tell us whether the reform affected utilization or prices. Therefore, the main outcome variables for utilization must be captured by the number of counts of utilization events. Estimating the expenditures after knowing the effects of the reform on the counts of utilization can then give an idea whether the law changed prices and whether it resulted in increases or decreases in overall health care expenditures.

Tables 2.1 and 2.2 compare means for the other 50 control states (49 states and District of Columbia) in the pretreatment years between 2000 to 2005 for the independent and dependent variables, respectively. Before the reform, residents in other states purchased more prescriptions drugs and spent more on them than Massachusetts residents. However, Massachusetts residents on average had significantly more counts of outpatient and office visits. They were also more likely to use any health care service before the reform. Also, Massachusetts residents were on average more educated, less likely to be unemployed, more likely to be a student, and more likely to have high incomes compared to residents living in other states. I will use a difference-in-difference estimator to account for these pre-reform differences in utilization outcomes while conditioning on a rich set of controls.

Figures 2.1 and 2.2 plot the average values of the total health care service counts and dollar spending in Massachusetts and the control states every year from 2000 to 2015, along with their 95% confidence intervals, respectively. These healthcare measures are the totals of prescription drug purchases, inpatient, outpatient, medical-provider office and ER visits.

According to Figures 2.1 and 2.2, both the total counts of health care service and spending were very similar in Massachusetts compared to rest of the country before the reform. After 2006, there is a gap between Massachusetts and other states where Massachusetts experienced an increase in health care service spending and use. This increase is more apparent in total count measure and the trend seems to be upward, whereas in other states the trend is generally stable over time. There is an increasing trend in Massachusetts which is steeper than rest of the country and the resulting gap reaches its maximum in 2012 for both count and spending outcomes.

Next, I conduct regression analyses to more formally investigate whether the health care use in Massachusetts relative to the control states was a causal response to health care reform to support the preliminary findings from Figures 2.1 and 2.2.

2.4 Regression Analysis

I start with estimating the effect of the reform on health care service use counts with negative binomial models employing a difference-in-difference strategy. For my main analysis, I use the counts of utilization in prescription drugs (each time a prescription drug is purchased), inpatient hospital visits, outpatient hospital visits, medical-provider office visits, ER visits and total counts of all these utilizations. For the primary results, I report average marginal effects from a negative binomial model specification due to the health service use count data being discrete and extremely skewed to the right. The reason I prefer the marginal effects from the negative binomial model as the primary results is because it has fewer restrictions on the mean-variance relationship compared to Poisson.[2] The conditional

[2]Marginal effects from Poisson are also reported to compare with the main results in Appendix Table A2.1.

mean expectation for the negative binomial is given by;

$$E[count_{iast}|\mu_{iast}, \alpha] = \mu_{iast} \tag{2.1}$$

where i, a, s and t are indices for individual, health care service type, state and year respectively, *count* is the count of use/purchase for health care service a, α is the overdispersion coefficient and

$$\mu_{iast} = exp(\delta_0 + \delta_1 MA_s + \delta_2 Post_t + \delta_3 MA_s * Post_t + \delta_4 X'_{ist} + \theta_s + \rho_t + \theta_s \rho_t) \tag{2.2}$$

where MA_s is a dummy variable which indicates the person lives in Massachusetts. $Post_t$ is a dummy that turns to one on and after 2006. X'_{ist} consists of all the person-level control variables that are listed in Table 2.1. θ_s and ρ_t are state and year fixed effects and $\theta_s \rho_t$ denotes the state-specific linear time trends which control for unobserved state-level factors that evolve linearly with time. I use the above difference-in-difference identification strategy for health care outcomes with the appropriate models for count and expenditure measures. This DD strategy assumes that in the absence of the reform changes over the health care service counts and expenditures would have been the same in Massachusetts and the control states, conditional on the control variables. Figures 2.1 and 2.2 provide preliminary support for this assumption. Therefore, I use all other states as the control group in main analyses. I will later consider alternative control states in the robustness checks section.

Next, I estimate how the reform affected health care counts and expenditures using two-part models (or hurdle models for count outcomes). The reasons for using two-part models are both due to the shape of the health care utilization distribution and the nature of the decision-making processes that go into health care utilization which typically involve different agents at different margins of utilization. There is a large portion of the sample with no

health care use and the two-part model does not assume the covariates affect the decisions to use any health care and the amount of use or spending (conditional on any use/spending) similarly. I first report the average marginal effects from the probit model shown below which shows how the reform affected the probability of health care use for each health care service category conditional on controls.

$$\Pr(y_{iast} > 0|X) = \Phi(\gamma_0 + \gamma_1 MA_s + \gamma_2 Post_t + \gamma_3 MA_s * Post_t + \gamma_4 X'_{ist} + \omega_s + \upsilon_t + \omega_s \upsilon_t) \tag{2.3}$$

Later, I report the second part of the model to show how the reform changed health care spending for those who use health care services. In the second part, I use GLM with log link and negative binomial family for the count outcomes and log link and gamma family for the spending outcomes given below to deal with the high skewness that is present in the health care expenditure distribution. The conditional mean expectation of the second part of the model is given below;

$$E[y_{iast}|y_{iast} > 0|X) = exp(\alpha_0 + \gamma_1 MA_s + \alpha_2 Post_t + \alpha_3 MA_s * Post_t + \alpha_4 X'_{ist} + \lambda_s + \tau_t + \lambda_s \tau_t) \tag{2.4}$$

Finally, I combine the effect sizes from both parts of the two-part model and report the combined average marginal effect of the reform.

$$E[y_{iast}|X] = Pr(y_{iast}|X > 0) \times E[y_{iast} > 0, X] \tag{2.5}$$

It is important to mention that I report average marginal effects of the interactions of treatment and time ($MA_s * Post_t$). Puhani 2012 shows that when the treatment effect is the parameter of interest it is right to focus on the marginal effect of the interaction term.

The standard errors in all regressions are heteroscedasticity robust and clustered by state unless otherwise specified. I will more formally investigate whether the standard errors are underestimated in main analyses in the robustness checks section.

2.5 Results

Table 2.3 presents results from the preferred model. These negative binomial regression estimates of the effect of the reform on health care service counts are all significant. The reform increased all types of health care service counts of inpatient, outpatient, office and, ER visits significantly by 0.03, 0.3, 0.8 and 0.05 counts per person over a year respectively. The reform decreased the counts of prescription purchases significantly by 0.6 counts per person over a year. When I aggregate all types of utilization counts at the person-year level, I find that the reform is associated with a significant increase in total health care counts of utilization by 1.2 counts per person over a year. Table 2.4 repeats the analyses using hurdle models which separately estimate the extensive and intensive parts of utilization due to the large mass of zero utilization counts present in the data and the nature of the decision process where the decisions at these two margins are likely to be made by different agents (self vs. the physician). The combined average marginal effects from the hurdle models are very similar to the preferred model estimates from the negative binomial, pointing that the reform increased counts of health care service use for all types, but decreased prescription drug purchase counts significantly.

Next, I estimate the effects of the reform on health care expenditures using the two-part model. Table 2.5 shows combined average marginal effects for the full sample on expenditures for all the types of health care services. I find the reform increased the expenditures on prescription drug purchases, inpatient, outpatient, office and ER visits by \$204, \$386, \$204, \$229 and \$85 for a Massachusetts resident over a year. It is also associated with a \$942

increase in total health care expenditures for a person over a year. Note that these are the combined marginal effects encompassing both the external and internal margins of spending. It is possible that the reform's effects on these two different margins are different in direction and magnitude. Therefore, I report the reform's isolated effects on both of those margins separately in Tables 2.7 and 2.8. According to results on the extensive margin of spending (or use) in Table 2.7 the reform increased the probabilities of using prescriptions and attending inpatient, outpatient, office and ER visits by 3.6, 1.5, 8.8, 6.8 and 4.5 percentage points, respectively. Additionally, the reform increased the likelihood of having any of these health services by 5.7 percentage points.

I then turn to the intensive margin of spending in Table 2.7. The reform is again found to be associated with significant increases along this margin. Specifically, the reform led to the following increases per person over a year: $277 increase in prescription drug expenditures among prescription users, $1855 increase in inpatient visit expenditures among people with any inpatient visits, $243 increase in outpatient visit expenditures among people with any outpatient visits, $271 increase in office visit expenditures among people with any office visits, and $241 increase in ER visit expenditures among those with any ER visits. The reform increased total health care spending by $1113 per person over a year among people who used any of these health care services. Note that these expenditures represent total expenditures and include out-of-pocket payments and any amount paid by insurance regardless of the insurance type. According to the results from the extensive and intensive margins, we can see that the combined effects of the reform on health care spending are a result of both of these margins. The significance and the magnitudes of the combined effects are driven by the increases in both parts of the model. The effects found from the two-part model on expenditures may be attributed to increases in use as well as increases in prices (or both). Although I find the reform decreased the counts of prescription purchases, it increased both the probability of using prescriptions and prescription spending conditional

on any prescription spending. This can be due to several reasons. First, it could be that as a result of the reform people may have switched from less expensive prescriptions to more expensive prescriptions while buying less of them. It could also be the simple result of people purchasing prescriptions in bulk amounts resulting in fewer counts of prescription purchase events. Another explanation could be that the reform might have increased prescription drug prices.

2.6 Robustness Checks

This section investigates the validity of the assumption that the rest of the nation is a valid counterfactual to Massachusetts by trying different sets of control states. All these estimations are done for count outcomes using negative binomial regressions. The first control group consists of 10 states that have the most similar pretreatment utilization counts for each type of health care service. The second control group consists of 10 states with the most similar pretreatment trends to Massachusetts. These control states are found by regressing each health care utilization count on year for each state between 2000 and 2005 and picking the 10 states with the most similar slopes to Massachusetts' slope. Third, I use a control group which consists of 10 states with the most similar insurance coverage to Massachusetts before the reform. The fourth control group consists of other New England states due to their geographical proximity to Massachusetts. Finally, I use a synthetic control method developed by Abadie et al. (2010) by aggregating data to the state-year level and letting the data assign weights to each state (after including the control variables) so that when they are used together they will give the closest match to Massachusetts before the reform. After obtaining these weights, I multiplied them by the MEPS sampling weights for individuals. Tables 2.8 to 2.12 show results of these robustness checks for each type of

health care service count utilized. The standard errors in these models are clustered by the state-year level since the samples cover fewer states (11 or less in most cases).

The robustness checks using alternative control groups from Tables 2.8 through 2.12 give significant results for $MA_s * Post_t$ for the most part, and the ranges of the estimates are close to the main result from Table 2.3. For prescription drug events, except the group which only consists of New England states, the coefficients are all negative, with estimates ranging from -1.27 to -0.09. The main model estimate is within this range. However, compared to residents of other New England states, Massachusetts residents increased the counts of their prescription purchases by 2.2 after the reform. For inpatient visit outcomes, different control groups give estimates ranging from 0.0926 to 0.3 which is consistent with the result from the main specification, although the main result is on the conservative side of this range. For outpatient visit counts, all specifications give significant and positive estimates ranging from 0.83 to 0.149, with the main estimate falling on the conservative side (0.328). Office visit estimates range from 3.4 to 0.96 so the main estimate of 0.84 is closer to the lower end. ER visit estimates range from 0.0382 to 0.0683 with the main estimate being 0.05 falling in the estimates range. The estimates for the total counts of utilization events range from 6.4 to 1.6 although not always significant. The main estimate is 1.183 which is less than any of these estimates but significant.

2.6.1 Event Study

In this section, I replicate my baseline specification adding more interaction terms. Following Courtemanche and Zapata (2014) I split the years before and after 2006 into sections. I create indicators of five time periods: "early before" defined as the years between 2000-2002, "late before" defined as the years between 2003-2005, "during" defined as years of 2006 and 2007, "early after" defined as the years between 2008-2010 and "late after" defined as

the years after 2010. I interact these indicators with MA_s indicator except for "early before" which serves as the reference period. This flexible event study type approach investigates whether there are any pre-existing trends that are so unique to Massachusetts that no other state or combination of states can accurately be used as a counterfactual. Also, it shows if the reform had differential effects on utilization count outcomes depending on time. It is plausible to expect an immediate increase in utilization right after the passage of the reform especially if there was a pent-up demand, or it could also be the case that the full effects of the reform come into play later in time as the spillovers (i.e. knowledge spillovers, diffusion of competition in health care markets) of the reform may take time to materialize.

Table 2.13 displays the average marginal effect estimates for the interaction terms. The coefficient of "MA*early after" and "MA*late after" are all significant and positive. "MA*during" is also significant and positive for all outcomes except prescription drugs. The coefficient of "MA*late before" is significant and positive in inpatient and ER visit counts although these coefficients are very small. The average marginal effects of "MA*late before" on office visit counts and total utilization counts are significant and negative implying that there was already a different trend in Massachusetts just a few years before the reform took effect. Despite these significant coefficients on "MA*late before", the reform's increasing effects grew over time reaching the maximum after 2010. The marginal effects of "MA*late after" are approximately twice the size of the marginal effects of "MA*early after" and "MA*during". This implies the increases in utilization in response to the reform were fully realized four years after its implementation.

2.6.2 Tests Related to Inference

In this section, I investigate whether the DD estimator understates the standard errors. The results found in this paper can be driven by understated standard errors especially

given the treatment variable does not change within a state over time. This can cause several problems in inference as pointed out by Bertrand et al. (2004). To test whether the increases in utilization are still significant after aggregation of the data I collapse all data into a state-level panel with three time periods – before, during (2006-2007) and after, interact them with MA and regress the state average utilization on MA*during and MA*after with state and period fixed effects. Second, I collapse data into two cross-sectional units of Massachusetts and other states, define the same period dummies as the previous inference test, then regress averages of health care service counts on MA*during, MA*after, MA dummy and year fixed effects. Tables A2.2 and A2.3 present the results from these specifications. MA*After remains statistically significant and positive in both regressions despite the small sample size except inpatient visit counts.

2.6.3 Instrumental Variables

Following Courtemanche and Zapata (2014), I conduct an instrumental variables estimation by using $MA_s * Post_t$ as an instrument to estimate the impact of insurance on health care service use counts. This approach assumes that the reform affects health care utilization only by granting insurance on the extensive margin of providing insurance conditional on controls. As pointed out by Courtemanche and Zapata (2014) this assumption rules out the effect of switching across insurance plans on the intensive margin on health care utilization. This assumption would also not hold if the reform caused other changes in the health care system or spillovers which would then affect health care utilization. Despite these concerns, instrumenting with $MA_s * Post_t$ for insurance gives estimates on the effects of the reform through the insurance path, if it was the only channel through which the reform affected health care utilization.

For the first stage of the instrumental variables estimation, I use a linear probability model as shown below to estimate the effect of the reform on insurance coverage;

$$ins_{ist} = \beta_0 + \beta_1 MA_s + \beta_2 Post_t + \beta_3 MA_s * Post_t + \pi_4 X'_{ist} + \sigma_s + \psi_t + \sigma_s \psi_t + \varepsilon_{ist} \quad (2.6)$$

where ins_{ist} is a binary variable if the person i living in state s in year t had insurance coverage. Because I am using negative binomial models I use the two-stage residual inclusion (2SRI) approach, which includes the residuals from the first stage as a regressor in the second stage. Terza et al. (2008) show that when the second stage of the instrumental variables estimation is a nonlinear function this approach gives consistent estimates. The second stage is given by;

$$E[count_{iast}|\mu_{ist}, \alpha] = exp(\pi_0 + \pi_1 ins_{ist} + \pi_2 \hat{u}_{ist} + \beta_3 X'_{ist} + \theta_s + \rho_t + \theta_s \rho_t) \quad (2.7)$$

where $\hat{u}_{ist}$ denotes the residuals from the first stage regression. I calculated the asymptotic standard errors with the formula given in Terza (2011).

The estimated coefficient $\hat{\pi}_1$ gives the local average treatment effect of insurance for those who gained insurance as a result of the reform. This does not represent the average treatment effect of insurance since people did not randomly gain coverage due to the reform. Table 2.14 reports the coefficient estimates from the first and second stages of instrumental variables estimation. According to the estimate on the first stage, the reform is associated with a 5.3 percentage point increase in coverage. Turning to the second stage results, obtaining insurance leads to statistically significant increases in all but one health care service outcome. Specifically, having insurance is estimated to increase counts of prescription purchases, inpatient visits, outpatient visits, ER visits and total health care use counts by 8.4,

0.08, 0.45, 0.06 and 12.6 per person over a year respectively. Having insurance increases outpatient visit counts but the coefficient is not statistically significant.

2.7 Conclusion

In this paper, I examined the effect of health care reform in Massachusetts on use of health care services using MEPS data. A negative binomial difference-in-difference analysis showed that the reform increased the counts of inpatient, outpatient, office, and ER visits significantly. I find that the reform was associated with decreases in counts of prescription drug purchases. Overall, it significantly increased total utilization of health care services measured in counts of health care events. These results were robust to using different functional forms and alternative control groups. I also examined the effect of the reform on the probability of use and amount of dollar spending. I found that the reform increased all of these outcomes significantly. Although I find the reform decreased prescription drug purchase counts, the reform was associated with increased probability of using prescriptions on the extensive margin and more prescription spending on the intensive margin. This can mean either the reform changed the prescription drug prices, or people switched their prescriptions to more expensive from less expensive while using them less, or people simply purchased them in bulk amounts, decreasing their counts of purchases. I also found evidence that the increasing effects of the reform grew over time, especially after 2010, doubling its initial impact. Finally, I used the reform as an instrument for health insurance coverage and estimated large and positive impacts of coverage on utilization.

To sum up, I show that the reform was mostly associated with increased use of hospital and office visits. However, I do not examine which types of health care service visits were impacted the most, or outcomes of length of stay, or the prescription drug types. The results found in this paper should be taken with the results from prior literature. Kolstad and

Kowalski (2012) showed that the reform decreased the number of inpatient visits originated from ER as well as hospitalizations for preventible conditions. They also found the reform increased the utilization of preventive services. Miller (2012) found a decrease in nonurgent ER visits. The findings from this paper can provide evidence that the gains in self-assessed health documented by earlier literature (Courtemanche and Zapata 2014) are likely real, rather than due to the "warm-glow" effect documented in the Oregon experiment literature. Future research should focus on what specific conditions were responsible for the increase in health care services as well as examining whether the effects differed across different populations.

Chapter 3

The Impact of the Affordable Care Act on Health Care Utilization

3.1 Introduction

The Patient Protection and Affordable Care Act (ACA) is a United States federal statute enacted on March 23, 2010. The ACA aims to attain nearly universal health insurance coverage. This goal would be achieved through a combination of mandates, regulations on insurers, expanding Medicaid subsidies and health insurance exchanges. Most of these major provisions of the ACA were implemented in 2014. Studies show that the ACA led to increased health insurance coverage (Frean et al. 2016, Courtemanche et al. 2016, 2017 and 2018). The goal of this paper is to investigate whether the ACA led to any changes in health care utilization through increasing insurance coverage.

In order to reach and sustain universal coverage, the ACA imposed three major components which simultaneously supported each other known as "the three-legged stool". The first component regulated and reformed the non-group insurance markets so that consumers without access to employer-provided or public coverage would have access to coverage. These reforms included going from experience rating to community rating, guaranteed issue, and

minimum coverage requirements as well as setting up marketplaces to foster competition among insurance plans.

Because these reforms would incentivize high-risk consumers to enroll and drive up the premiums leading to an adverse selection death spiral the second leg of the three-legged stool was implemented. The second leg mandated that individuals without coverage would be penalized. The penalty was also imposed on employees with more than 100 employers unless they covered at least 95% of their full-time employees and their dependents (Courtemanche et al. 2017).

Mandating individuals and employees to pay for health insurance would bring the affordability problem. The third leg of the stool aimed to solve this problem by making insurance more affordable through providing subsidies for individuals who do not qualify for Medicaid or other sources of affordable insurance and expanding Medicaid in states which opted to expand via the ACA.

The main purpose of this paper is to estimate the effect of the ACA both with and without the Medicaid expansion on health care utilization. The health care utilization outcomes include prescription drug, inpatient, outpatient, office-based medical provider, and emergency room (ER) utilization.

We separately estimate the effects of the ACA's non-Medicaid and Medicaid portions by using a difference-in-difference-in-difference (DDD) strategy, with the differences coming from time, state Medicaid expansion status, and person's treatment status where the treatment is defined as the insurance status of the person in 2013. In other words, the treatment variable is a binary indicator that turns to 1 if the person was uninsured in 2013 and 0 otherwise. Due to the two-year panel structure of MEPS, we are only able to capture the changes between two years using the person's treatment status. However, not all insurance take-up happened in 2014, but there were continued coverage gains in 2015 (Frean et al. 2017, Diamond et al. 2018). We also estimate the effects of the ACA using the same strat-

egy for the years of 2014 and 2015 by re-defining our treatment variable as the insurance status of the person in 2014. This will give us the effects of the Medicaid and non-Medicaid portions of the ACA between 2014 and 2015.

We find that the ACA resulted in significant increases in total health care utilization counts in its first year between 2013 and 2014. Specifically, we find that the ACA increased use of inpatient and office-based medical provider visits significantly in the 2013-2014 sample. The increase in total counts of health care use was barely significant between 2014-2015 although we find that the full ACA increased health insurance coverage in this period significantly with a magnitude very similar to its effect in its first year.

3.2 Literature Review

3.2.1 Effects of Health Insurance on Health Care Utilization

The effect of health insurance on health care utilization has been an important topic in the literature. Causal evidence comes from a variety of observational and randomized controlled studies. The first of those was the RAND Health Insurance Experiment (HIE) of the 1970's and 1980's which randomly assigned people to health insurance programs with varying levels of cost-sharing. The study found that "a catastrophic insurance plan reduces expenditures 31% relative to zero out-of-pocket price" (Manning et al. 1987). They found that cost sharing increased the number of medical contacts, rather than the intensity of each contact. In 2008 Oregon's Medicaid granted a lottery in which randomly selected uninsured low-income adults in Oregon could enroll in Medicaid. Finkelstein et al. (2012) found the treatment group which was selected by the lottery experienced a 25 percentage point increase in the probability of being insured compared to the control group that was not selected. The authors found that in its first year the experiment led to significantly higher

health care utilization and lower out-of-pocket expenditures. Namely, the Oregon health experiment found that having insurance was associated with an increase in the probability of any hospital admissions that do not originate from ER and increases in hospital days, total list charges and numbers of procedures performed. Another study by Taubman et al. (2014) studied Oregon health experiment and found that the Medicaid coverage increased ER visits.

There is also evidence from non-experimental settings. These results mainly come from studies involving Medicaid and Medicare. Dafny and Gruber (2005) explore the impact of the Medicaid expansion that occurred between 1983-1996 on child hospitalizations and find that the Medicaid expansions increased hospitalizations significantly among children. Lichtenberg (2002) and Card et al. (2008) find that Medicare eligibility increased health care utilization.

More recent evidence on the effect of insurance of health care utilization comes from the effects of ACA's dependent coverage provision which took effect in 2010 and mandated insurers to cover dependents up to 26 years old. Papers that studied health care utilization mostly find increases associated with the mandate. Sommers et al. (2012) found evidence of an increase in access to care, and reductions in the number of young adults who delayed getting care and for those who did not get care because of costs. Akosa Antwi et al. (2015) report that the mandate was associated with an increase in inpatient visits, especially for mental health conditions. Barbaresco et al. (2015) found that the mandate was associated with increases in the probabilities of having a primary care doctor, and reporting excellent health and a reduction in BMI. They also report ACA increased risky-drinking but did not find any evidence it affected preventive care utilization. Chua and Sommers (2014) examined the drug utilization change due to the provision and did not find any significant changes.

3.2.2 Effects of the 2014 Affordable Care Act

In order to understand the effect of having health insurance on health care utilization with evidence from the ACA we first need to know whether there is evidence that the ACA leads to increases in health insurance coverage. The existing literature using different data and methods all point to the ACA indeed increasing health insurance coverage. The earlier evidence comes from basic pre-post comparisons at the national level. These studies find the ACA resulted in a 2.8 to 6.9 percentage point increase in coverage depending on the data, the time span and the population (Long et al. 2014, Smith and Medalia 2015, Courtemanche et al. 2016, Obama 2016, Barnett and Vornovitsky 2016, McMorrow et al. 2016). Using a difference-in-difference-in-difference model which exploits state-level variation in the Medicaid expansion portion of the ACA and the pre-treatment uninsured rates in local areas, Courtemanche et al. (2017) find that the ACA with the Medicaid expansion increased health insurance coverage by 5.9 percentage points. They find that the ACA without the Medicaid expansion increased health insurance coverage by 2.8 percentage points. Frean et al. (2017) provide evidence on the effects of various components of the ACA by exploiting variations in income, geography and time. Their model examines public and private coverage expansions as well as the individual mandate. They report that their model explains 60% of the coverage gains in 2014 and 2015.

Recent literature on the effects of the ACA is not limited to health insurance coverage. Kaestner et al. (2018) find that the ACA with the Medicaid expansion had important financial impacts by reducing the number of unpaid bills and the amount of debt sent to third-party agencies. Wherry and Miller (2016) find that in the second half of 2014 the Medicaid portion of the ACA increased health insurance by 7.4 percentage points among adults. They report that Medicaid expansions resulted in significant increases in physician visits in general practice, overnight hospital stays, rates of diabetes and high cholesterol

diagnoses. Shartzer et al. (2015), Polsky et al. (2015), Kirby and Vistnes (2016), Sommers et al. (2015), Sommers and Blendon (2015), and Courtemanche et al. (2018) find the ACA improved access to care. Simon et al. (2017) find that the Medicaid expansions of the ACA increased insurance coverage and access to care among the targeted population of low-income childless adults and improved self-assessed health. Courtemanche et al. (2018) find that the ACA improved self-assessed health among older non-elderly adults. Goldman et al. (2018) found that the ACA was associated with reduced out-of-pocket spending for medical care, particularly among lower-income people.

A few recent studies have analyzed changes in health care utilization and spending in 2014, though none of them estimate the causal impact of the full ACA. Nikpay et al. (2016) and Selden et al. (2016) conduct pre-post analyses. Simon et al. (2016) estimate the causal impact of the Medicaid expansion alone. In contrast, this paper estimates the causal impacts of both the public and private components of the ACA on utilization and spending. The previous studies examined a relatively narrow range of utilization outcomes: uninsured hospitalizations (Nikpay et al., 2016), office visits and ER visits (Selden et al. 2016), and certain forms of preventive care (Simon et al. 2017). Sommers et al. (2017) showed evidence from low-income populations of Kentucky, Arkansas and Texas that the ACA significantly increased preventive health visits, and among people with chronic conditions it increased regular care for these conditions and medication adherence. We examine additional categories, such as prescription drugs and outpatient visits, as well as total spending. Finally, we will expand on these studies by including the second year of post-treatment data.

There are many channels through which the ACA can affect health care utilization. By increasing insurance coverage, the ACA can directly improve access to health care leading to an increase in demand. On the other hand, having health insurance can decrease utilization in the long run by causing improvements in healthy behaviors and increasing preventive care.

Having health insurance can also lead to risky health behaviors via moral hazard and income effects which can in turn lead to increase in health care utilization in the long term.

The primary purpose of this paper is to estimate the impact of the ACA's 2014 provisions on a variety of outcomes related to health care utilization. In addition, this paper examines differential effects resulting from differences in Medicaid expansion decisions of the states. The secondary contribution of this paper is to estimate the effects of ACA's different portions by assigning the treatment variable on the individual level. This identification strategy has advantages compared to other strategies used in the literature before. First, unlike the studies which used the difference-in-difference strategy, it does not make the assumption that the states which expanded Medicaid and those that did not must have the same counterfactuals and trends. This assumption is strong given the political nature of the Medicaid expansion decisions of the states. It is likely that the unobserved determinants of these state decisions are correlated with the determinants of our outcome variables such as insurance coverage and health care utilization. The identification strategy used in this paper also has advantages over the DDD strategy employed by Courtemanche et al. (2017, 2018) which used the pre-ACA local area uninsured rates as the measure of treatment exposure of the individual. Our treatment status comes from the person's observed pre-ACA insurance status as opposed to the pre-ACA insurance status of the local area in which he lives. In other words, it does not make the assumption that the treatment status of the individual is correlated with his location's exposure to the treatment, where this exposure is assumed to be proportional to its pre-ACA uninsured rate. The identification strategy in this paper readily takes the individual's own insurance status pre-ACA as the treatment.

We use AHRQ's Medical Expenditure Panel Survey (MEPS) with state-identifiers between 2011-2015, with the sample restricted to individuals between 19 and 64 years of age. We use the MEPS because it includes detailed information on a wide range of health care utilization outcomes for a sample that is representative of U.S population.

Our results suggest that the implementation of the full ACA including the Medicaid expansion increased total counts of health care use significantly only in its first year. We find that the full ACA specifically increased inpatient and medical-provider office visit use significantly in its first year. Between 2014 and 2015, the full ACA is associated with increases in total counts of health care use and total counts of prescription purchases but these results are only significant at the 10% level.

3.3 Data

Our primary data source is the 2011-2015 waves of the Medical Expenditure Panel Survey (MEPS). The MEPS consists of a nationally representative subsample of U.S households that participated in the previous year's National Health Interview Survey. MEPS has an overlapping panel design, with each household participating in 5 rounds of interviews that take place over a 2.5-year period. After completing the survey, MEPS obtains permission from the respondent to contact their medical providers, resulting in detailed information on dates, charges, and sources of payments that enables the computation of health care utilization variables for each person in each year.

We consider a wide range of outcomes. Categories of utilization will include counts of physician and non-physician office visits; inpatient, outpatient, and emergency room visits; and prescription drugs. For each category, we measure both frequencies of use (e.g. number of visits, number of prescriptions filled) and expenditures in the past twelve months. Our primary utilization outcome is the count of health care service utilized for these outcomes.

Before we examine the effects of the ACA on health care utilization we will turn to estimate its effects on health insurance coverage since this is the primary channel through which the ACA should influence utilization. Later, we will examine the ACA's effects on total counts on five main health care types for outcomes: office visits, ER visits, inpatient

visits, outpatient visits and prescription drugs. We also look at the effects on total numbers of these utilizations as the total counts of these events. Finally, we will examine the effects on expenditure outcomes for these same dimensions of care. It is reasonable to expect that gaining health insurance can immediately increase health care utilization especially if there is a pent-up demand specifically for the newly insured individuals. However, Finkelstein et al. (2012) show one-year estimates of providing health insurance from the Oregon health insurance experiment and report that they do not find any evidence of a large immediate utilization effect of pent-up demand. O'Malley et al. (2016) look at the utilization outcomes for ER and primary care visits for enrollees in the Oregon health insurance experiment and find that primary care visit rates were higher initially (4-12 months) compared to the second year. They conclude that the effect of health insurance expansion on utilization depends on the timing and the prior health insurance coverage of the new enrollees. This paper can shed light on to whether this "pent-up" demand exists by comparing the effects of the ACA on both insurance take-up and utilization between the 2013-2014 and 2014-2015 panels of the MEPS.

The MEPS contains demographic information that will enable the inclusion of a set of control variables. Such controls will include indicators for race/ethnicity (white, black, Hispanic, or other), income (poor, near poor, low income, middle income, or high income), education (less than high school, high school, some college, 4-year degree or greater), gender, marital status, age, and number of children in the home. The MEPS also contains a series of event-level information for the surveyed individuals on prescribed medicines, inpatient visits, outpatient visits, ER visits and medical-provider office visits. These event-level observations represent a unique utilization event and they include detailed information on utilization and expenditures. We aggregate these events by year and individual level and match this aggregated event data with individual-level data by year and individual identifiers to construct our sample.

Our main sample consists of 19 to 64 years old people from the 2013-2014 waves of the MEPS. We exclude individuals older than 64 since the ACA was not intended to affect the health care coverage of seniors. Because we are using an individual-level treatment variable where the treatment is being uninsured in 2013 and MEPS panels include only two years, we are only able to follow the same individual pre-and post-ACA between the years of 2013 and 2014. We will also use another sample that includes the 2014 and 2015 waves to identify the effects of the ACA between 2014 and 2015. Following Courtemanche et al. (2017), we include interactions of the post-treatment dummy with indicators of whether states set up their own private exchanges and whether these exchanges experienced glitches. Having these controls will eliminate some of the bias that can be caused by the omitted variables that are hard to measure and correlated with the states' decisions to expand Medicaid, health care utilization and coverage.

In our main specifications which include only 2013 and 2014 as sample years, we classify the 27 states that expanded Medicaid by 2014 as Medicaid expansion states. January 2014 is the time when most of the expansions took place. For the specifications that use 2014 and 2015 year samples we classify 3 more states that expanded Medicaid in 2015 as Medicaid expansion states.

Table 3.1 provides 2013-2014 and 2014-2015 means and standard deviations of the dependent variables and Table 3.2 does the same for the controls. According to Table 3.1, counts of utilization are very similar across the 2013-2014 and 2014-2015 samples but slightly higher in the 2014-2015 sample. However the expenditure outcomes are considerably higher in 2014-2015 sample relative to the 2013-2014 sample.

Next we will show whether these differences are driven by the ACA by relying on the common counterfactual trends assumption in the outcomes on the bases of Medicaid expansion status and the uninsurance status in 2013.

3.4 Econometric Analyses

For each utilization outcome, our main goal is to estimate the effects of both the fully implemented ACA that includes Medicaid expansion and the ACA without the Medicaid expansion. It is important to isolate the effects of the ACA components from the year-to-year fluctuations that would have occurred even in the absence of the ACA. We adopt the DDD strategy which exploits the variations in being treated at the individual level, as being treated is defined as being uninsured in 2013 – just a year prior to the ACA. This DDD strategy makes one less assumption compared to other DDD strategies that were used in the literature which exploited local area pre-ACA uninsured rates as their treatment variable (Courtemanche et al. 2017 and 2018) and assumed that the treatment at the individual level was correlated with the treatment exposure at the local area level.

We first start with estimating the effect of components of the ACA on the probability of getting coverage. Assuming that the individual is treated only when he has no insurance coverage pre-ACA in 2013, we estimate the linear DDD model given below:

$$\begin{aligned} y_{ist} = {} & \beta_0 + \beta_1 POST_t + \beta_2 MEDICAID_s + \beta_3 T_i + \beta_4 (MEDICAID_s * POST_t) + \\ & \beta_5 (MEDICAID_s * T_i) + \beta_6 (POST_t * T_i) + \beta_7 (MEDICAID_s * POST_t * T_i) + \\ & \beta_8 X'_{ist} + \varepsilon_{ist} \end{aligned} \tag{3.1}$$

where y_{ist} is the insurance coverage indicator for individual i in state s in year t , $POST_t$ is an indicator whether period t is in 2014 or not, $MEDICAID_s$ is an indicator whether state s participated in the ACA's 2014 Medicaid expansion, T_i is the 2013 insurance status that turns to 1 when the individual is uninsured in 2013, X'_{ist} is a vector of control variables, and ε_{ist} is the error term.

In equation (3.1), β_6 captures the effect of the ACA without the Medicaid expansion. The identifying assumption for the impact of the non-Medicaid expansion components of the

ACA is that, in the absence of the ACA, any changes in outcomes that would have occurred to an individual in 2014 would not have varied differently by his insurance status in 2013 conditional on the controls. This is a weaker assumption than a simple DD model that only uses a pre-post comparison which would assume there would have been no changes at all in the outcomes without the ACA conditional on the controls.

Similarly, β_7 captures the effect of the Medicaid expansion. We assume that in the absence of the ACA, the differences in the outcomes for the insured and uninsured people in 2013 who live in Medicaid expansion states would have evolved similarly compared to the differentials in non-Medicaid expansion states. This is a weaker assumption than a simple DD model which assumes that there would have been no differential changes across expansion and non-expansion states conditional on the controls.

As our primary outcome variable, we use the counts of utilization in prescription drugs (each time a prescription drug is purchased), inpatient hospital visits, outpatient hospital visits, medical-provider office visits, ER visits and total counts of all these utilizations. For our primary results, we report estimates from the negative binomial model specification due to the nature of our data being discrete and extremely skewed to the right.

3.5 Sensitivity and Other Checks

There are a few challenges with modeling counts of health care utilization. Because the count data is discrete and non-negative, the count data generating process may be distorted by a linear model, leading to negative predictions (King 1988). There are a few alternative specifications that can give better predictions like the Poisson or the negative binomial depending on the level of dispersion. For our primary results we prefer the marginal effects from negative binomial model because it has less restrictions on the mean-variance

relationship compared to Poisson. In this section we will report marginal effects from Poisson to compare and check the sensitivity of our main findings.

Later, we replicate our analyses for the main samples of 2013-2014 and 2014-2015 years for the expenditure outcomes in dollar amounts using two-part models. The reason we use two-part models is because an overwhelming majority of our samples do not incur any health care utilization. Also, a two-part model does not make the assumption that the ACA and its components affect the external and internal margins of health care expenditures in the same direction and/or magnitude. We use a probit estimation for the first part to model how the ACA affected the probability of any use of health care on the extensive margin and GLM with log-link and gamma family for estimating the second part of the equation where the dependent variable becomes any positive health care spending conditional on any spending. The advantages of using GLM as opposed to logged-OLS and level-OLS when modeling health care expenditures are broadly discussed in health economics literature (Manning & Mullahy 2001, Jones 2000, Deb & Norton, 2018). The main reason to use GLM in favor of OLS is because our data is extremely skewed to the right and GLMs have more flexibility with dealing this extreme skewness. GLMs allow any function (the link function of the GLM) of the outcome variable to vary linearly with the covariates, as opposed to OLS which requires this function to be of linear nature only.

For our next set of checks we run the same regressions in our main model for the samples pre-ACA, namely the samples of MEPS from 2011-2012 and 2012-2013. In each of these models the treatment now is defined as the uninsurance rate in the previous year (i.e. the treatment would be the uninsurance status in 2011 in the 2011-2012 sample and it would be the uninsurance status in 2012 in 2012-2013 sample). Post is a dummy that turns to 1 in 2012 in the 2011-2012 sample, and it turns 1 in 2013 in the 2012-2013 sample. In essence, these regressions provide placebo tests since a hypothetical ACA that did not actually happen in those years should give us null results.

Because we are using non-linear models, we report average marginal effects of the interactions for our main coefficient of interest. Ai and Norton (2003) show that in non-linear models the marginal effect of the full interaction term is not equal to the cross-difference (or derivative in the continuous case). However, Puhani (2008) shows that the cross difference in Ai and Norton's paper is not equal to the treatment effect and he points out that when the treatment effect is the parameter of interest it is right to focus on the marginal effect of the interaction term in non-linear difference-in-difference models provided that the non-linear model has a strictly monotonic transformation function.

3.6 Results

Table 3.3 reports results from DDD regression from linear probability models for health insurance coverage for the 2013-2014 and 2014-2015 samples separately. Although the ACA was implemented in 2014 some insurance take-up continued between 2014 and 2015 as documented by literature.

The results suggest that between 2013 and 2014 private portion of the ACA increased the probability of having insurance by 40 percentage points for those who were uninsured in 2013. The Medicaid expansion led to statistically significant additional gains on insurance by 7 percentage points on those who were not insured in 2013. This means the fully implemented ACA increased coverage on previously uninsured people by 47 percentage points at a statistically significant level in its first year. The results from 2014-2015 sample indicate that the impact of the fully implemented ACA on insurance was a little less but comparable in its second year. The fully implemented ACA increased the probability of having insurance among previously uninsured individuals by 41 percentage points at a statistically significant level between 2014 and 2015. In its second year, Medicaid portion of the ACA increased the probability of having insurance by 8 percentage points - just a little more than the first year's

impact and it was statistically significant. The private portion of the ACA in its second year significantly increased the probability of having insurance by 33 percentage points.

After showing that the ACA increased health coverage significantly we turn to analyses on how the ACA affected health care utilization of counts. Tables 3.4 and 3.5 report average marginal effects on total counts of various health care services from negative binomial regressions for the first and second year of the ACA respectively.

According to Table 3.4, the ACA increased counts of inpatient hospital and medical-provider office visits significantly by 0.04 and 2.5 counts of visits per person in its first year. It also led to a significant increase in total counts of health care service use by 4 counts per person (including prescription fills, inpatient, outpatient, medical-provider office and ER visits). The only decreasing effect of the ACA is seen on ER visit counts but it is not statistically significant. Most of the increases in counts of visits are attributable to the private portion of the ACA rather than the Medicaid expansion except inpatient hospital visit counts. The private portion of the ACA increased outpatient and medical-provider office visit counts significantly by 0.2 and 1.5 counts respectively. Interestingly, the private and Medicaid portions of the ACA have opposite effects on inpatient hospital visits compared to office and outpatient visits. Although insignificant, Medicaid expansion increased inpatient hospital visits. The full effect of the ACA on inpatient hospital visits was a significant increase, stemming from the Medicaid portion.

Table 3.5 shows the effects of the ACA and its portions on 2014-2015 sample. The results suggest that the fully implemented ACA's impacts on health care service counts were all positive but insignificant (barely significant on prescription fill counts and total counts) despite its impact on health insurance coverage being comparable to the first year's. The private portion of the ACA increased inpatient hospital visits by 0.05 per person significantly and increased prescription fill counts by 1.7 fill per person although it was only significant at the 10% level.

We then replicate our main analyses running Poisson models to see whether our results are sensitive to model selection. Tables 3.6 and 3.7 show the average marginal effects from Poisson models on the counts of the same outcome variables. The Poisson model gives very similar results to the negative binomial, pointing out that the fully implemented ACA increased inpatient hospital visits, medical-provider office visits and total counts of utilization in its first year significantly, at very similar magnitudes compared to main results. The only difference is that Poisson model picks up a significant increase in total counts of health care use that results from the private portion of the ACA. Just like the negative binomial, Poisson results do not show any significant changes on health care use associated with the full ACA in its second year. Only significant increase in the second year is an increase in inpatient hospital visits associated only with the private portion of the ACA by 0.05 counts per person. Although it is a small increase in magnitude it is consistent and significant across both models.

Next, we conduct analyses of the ACA's first-year impact on health care spending and use. Table 3.8 shows combined average marginal effects from the two-part models consisting of both the external (probability of any use) and the internal margins of health care spending (expenditures conditional on any use). We find the full ACA significantly increased medical-provider office visit expenditures by $416 per person in its first year. The other expenditure outcomes are not meaningfully affected by the full ACA. Table 3.9 reports average marginal effects on the extensive margin of use. We find that the fully implemented ACA increased probabilities of having prescriptions, inpatient hospital visits and medical-provider office visits by 3.5, 3.4 and 10 percentage points respectively. It also resulted in an increase in the probability of having any health care services significantly by 8 percentage points among people who were uninsured in 2013. Results from the second part of the two-part model in Table 3.10 model indicate that the full ACA significantly decreased spending in inpatient hospital and ER visits by $12,632 and $1,096 per person in its first year among people who

use these services and who were not insured prior to the ACA. Given that we found the ACA increased the count and probability of inpatient hospital visits, this decrease in the intensive margin of inpatient spending could be attributed to either the ACA decreasing inpatient hospital prices/costs and/or ACA resulting in less expensive procedures in inpatient hospital settings.

Lastly, we repeat our main regressions on counts on pre-ACA years and report these results in Tables 3.11 and 3.12. In these placebo regressions, $POST_t$ is defined as 2012, and T_i is defined as being uninsured in 2011 in 2011-2012 sample. Similarly, $POST_t$ means 2013 and T_i means being uninsured in 2012 in 2012-2013 sample. We conduct these analyses to show that a placebo ACA does not meaningfully change utilization outcomes (or it does not affect utilization outcomes more than it would have due to chance). We find only two significant effects of the fully implemented placebo ACAs – an increase in prescription fills between 2011-2012 and an increase in ER visits between 2013-2014. Although the different portions of a hypothetical ACA do not show any significant effects between 2011 and 2012, there are some significant effects in 2012-2013 sample for two outcomes associated with a private portion of a placebo ACA. On the year prior to real ACA, the private portion of a hypothetical ACA is associated with significant increases in medical-provider office visits and total utilization counts (1.35 and 2.4 per person among those who were uninsured in 2012 respectively). The most likely explanation for these significant placebo results is the early Medicaid expansion in several states. Alternatively, the significant placebo results could also be just due to chance or indicate that this sample period which is just a year away from ACA is not a true placebo period for the ACA.

3.7 Conclusion

In this paper, we used data from Medical Expenditure Panel Survey to examine the effects of the 2014 ACA provisions on health care utilization. Using DDD strategy that exploits variation in time, pre-treatment uninsurance status, and state Medicaid expansion status, we separately estimated the effects in both Medicaid expansion and non-expansion states. The results suggest that the ACA increased health care utilization in some dimensions – including counts of inpatient hospital visits, medical-provider office visits, as well as total counts of health care service utilization including all of these services, outpatient and ER visits on its first year. However, these increases in health care utilization were not observed in ACA's second year. We also found that the ACA increased coverage and led to significant gains in both expansion and non-expansion states consistent with what has been found by prior studies. This significant gain in insurance was not limited to ACA's first year but it carried to the second year.

One explanation for the lack of significant results in ACA's second year despite its significant impact on increasing coverage could be attributed to differences among people who got insurance immediately following the ACA relative to those who were enrolled a year later. This can be explained by an existing pent-up demand stemming from people who needed health insurance immediately to satisfy their unmet health care needs. People who got coverage in ACA's second year might lack this pent-up demand relative to the people who got the insurance in the year of ACA's immediate implementation. This finding suggests that the timing and the prior health insurance coverage of the new enrollees matter in determining changes in health care use.

We also found that the overall effect of the ACA on the whole population in its first year was not significant in expenditures except an increase in office visits. However, ACA significantly increased the likelihood of having any health care use across many dimensions.

This paper leaves several questions for future research. First, we are only able to look at the effects of the ACA in its first and second years. Future studies should keep following ACA's effects in following years as its long-term effects may not have been realized yet. Second, more research is needed to show how the ACA altered health care service prices. For example, although we found the ACA increased inpatient hospital visits very moderately we see a decrease in inpatient hospital visit expenditures among users who were uninsured prior to the ACA. We also find the ACA resulted in decreases in expenditures of ER visits among users of ER. However, we do not know if these spending decreases on the intensive margin are due to price changes or people needing less expensive procedures.

Another limitation of this study is that it only focuses on individuals who lacked coverage pre-ACA. Therefore it is not able to explain how going from a less extensive coverage to a more extensive coverage affects health care use. More research is needed to answer this question in the future.

Figures and Tables

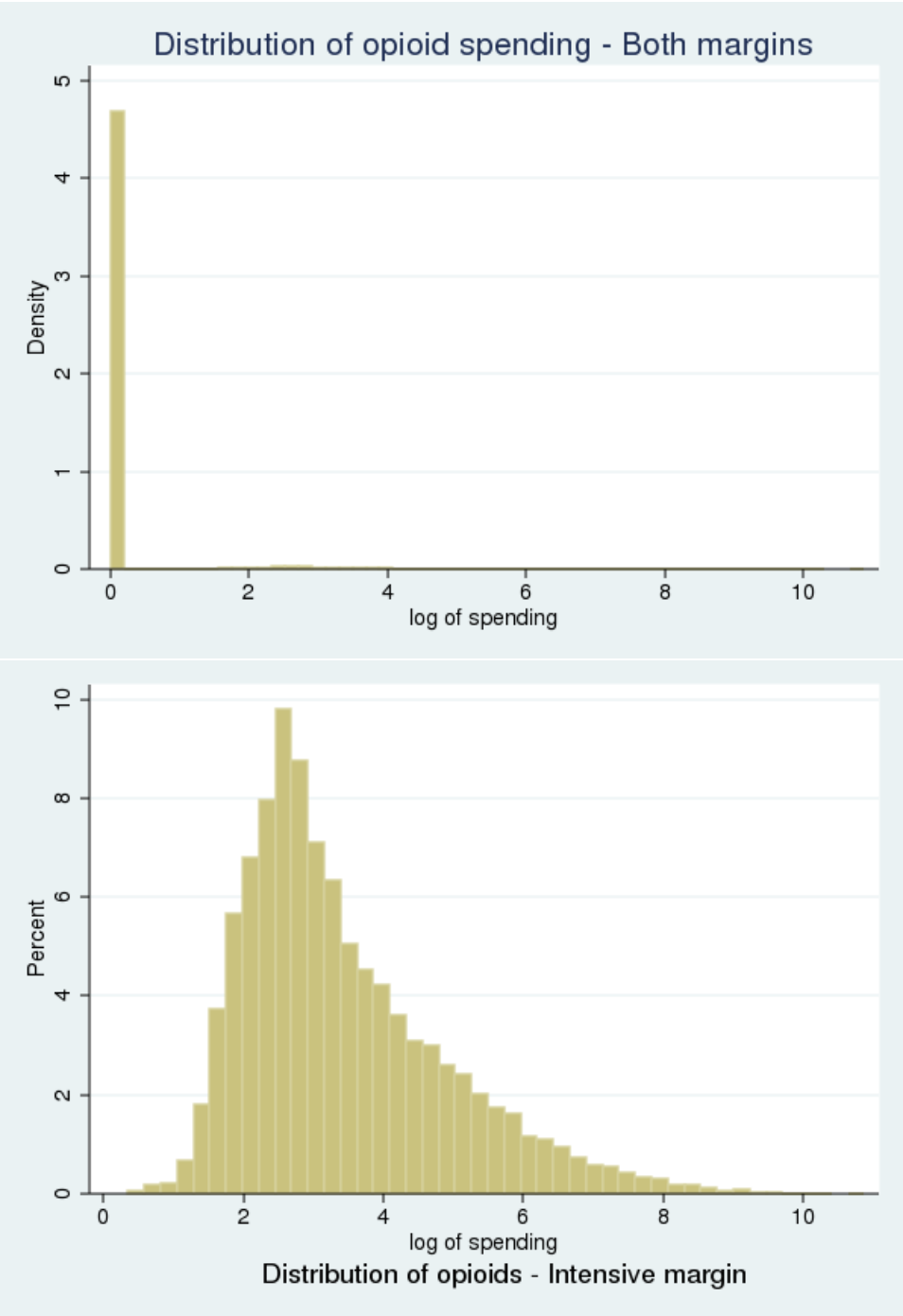

Figure 1.1: Distribution of opioid expenditures - Ages 18+

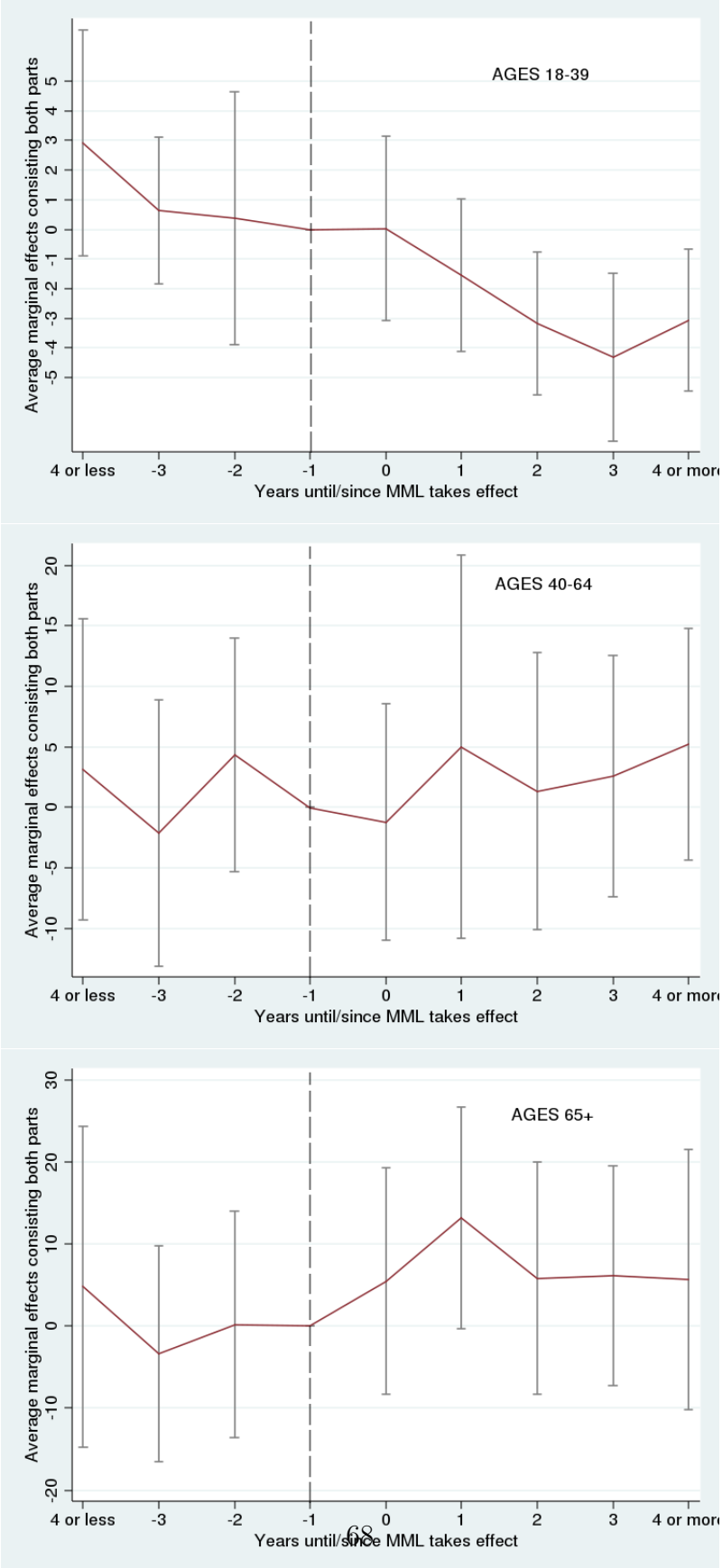

Figure 1.2: Results from event study analyses on opioid expenditures

(a) The year MML takes effect is represented by 0. The pre-adoption year is set to zero for normalization and excluded from the regression. The coefficients are estimates from the two-part models with probit in the first and GLM (with a log link and gamma family) in the second part.

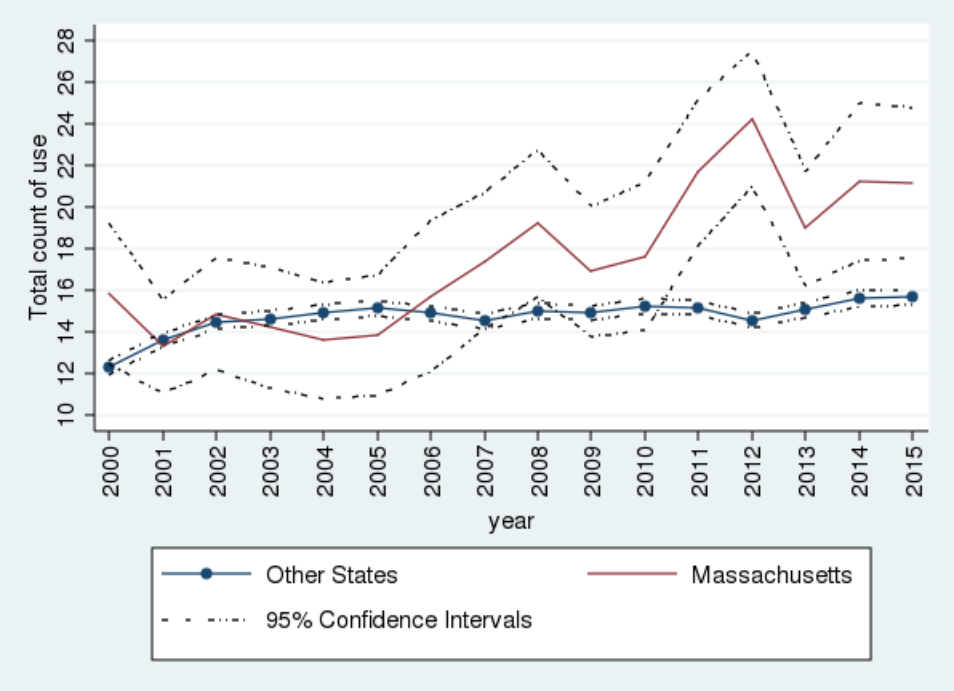

Figure 2.1: Changes in Health Care Service Use Counts 2000 to 2015

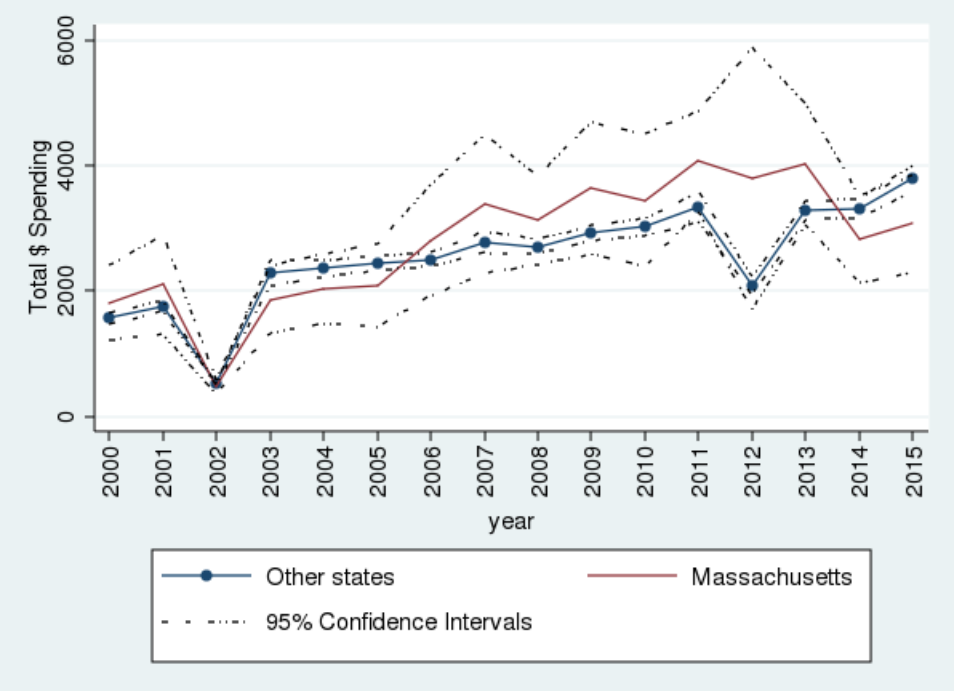

Figure 2.2: Changes in Health Care Service Use Spending 2000 to 2015

Table 1.1: Medical marijuana laws and provisions by state, 1996-2014

State	Effective date	Retail dispensary	Home cultivation	Non-specific pain	Patient registry
California	1996	1996	1996	1996	-
Colorado	2001	2001	2001	2001	2001
Montana	2004	-	2004	2004	-
Michigan	2008	-	2008	2008	-
Oregon	1998	-	1998	1998	2007
Washington	1998	-	-	1998	-
Alaska	1999	-	1999	1999	1999
Arizona	2011	2011	2011	2011	2011
Connecticut	2012	-	-	-	2012
District of Columbia	2010	2010	-	-	2010
Delaware	2011	2011	-	2011	2011
Hawaii	2000	-	2000	2000	2000
Iliinois	2014	2014	-	-	2014
Maine	1999	2009	1999	-	2009
Maryland	2014	2014	-	2014	2014
New Jersey	2010	2010	-	2010	2010
New Mexico	2007	2007	2007	-	2007
New York	2014	2016	-	2014	2014
Nevada	2001	-	2001	2001	2001
Rhode Island	2006	2009	2006	2006	2006
Vermont	2004	-	2004	2007	2004

(a) Sources: Effective dates of MMLs and provisions are taken from Wen et al. 2015 and http://medicalmarijuana.procon.org/view.resource.php?resourceID=000881

Table 1.2: Summary statistics for outcome variables

	Control states		**MML states**	
	mean	**s.d**	**mean**	**s.d**
Opioids				
Participation	0.0919	0.289	0.0838	0.277
Spending	14.73	210.1	12.28	266.2
Non-opioid painkillers				
Participation	0.202	0.416	0.173	0.378
Spending	40.54	229.77	30.91	339.57
Antidepressants				
Participation	0.101	0.301	0.0815	0.274
Spending	48.17	263.7	38.02	236.3
Anticonvulsants				
Participation	0.0591	0.236	0.0456	0.209
Spending	30.35	278.5	23.48	255.1
Sedatives				
Participation	0.0822	0.275	0.0608	0.239
Spending	17.86	153.0	12.02	114.4
Number of observations	233,010		202,025	

Table 1.3: Summary statistics for control variables

	Control states		MML states	
	mean	**s.d**	**mean**	**s.d**
Individual-level controls				
Demographic controls				
Age dummies: Ages 18-24 (ref.)				
Ages 25-29	0.0922	0.289	0.0973	0.296
Ages 30-34	0.0947	0.293	0.0988	0.298
Ages 35-39	0.0952	0.293	0.101	0.301
Ages 40-44	0.0952	0.293	0.101	0.301
Ages 45-49	0.0938	0.292	0.0953	0.294
Ages 50-54	0.0886	0.284	0.0890	0.285
Ages 55-59	0.0771	0.267	0.0720	0.258
Ages 60-64	0.0617	0.241	0.0573	0.232
Ages 65-69	0.0508	0.220	0.0453	0.208
Ages 70-74	0.0408	0.198	0.0358	0.186
Ages 75-79	0.0323	0.177	0.0290	0.168
Ages 80-84	0.0229	0.149	0.0204	0.141
Ages 85-90	0.0180	0.133	0.0170	0.129
Male	0.459	0.498	0.467	0.499
Race dummies: Other (ref.)				
White	0.564	0.496	0.477	0.499
Black	0.217	0.412	0.117	0.322
Hispanic	0.178	0.383	0.303	0.460
Married	0.534	0.499	0.529	0.499
Living in an MSA	0.758	0.428	0.903	0.297
Economic controls				
Education dummies: Less than high school (ref.)				
High school graduate	0.419	0.493	0.380	0.485
College graduate	0.264	0.441	0.304	0.460
Unemployed	0.398	0.498	0.392	0.488
Student	0.0538	0.226	0.0625	0.242
Family income as % of poverty line: Poor (ref.)				
Near poor	0.370	0.483	0.350	0.477
Low income	0.164	0.371	0.156	0.363
Middle income	0.311	0.463	0.296	0.456
High income	0.289	0.453	0.340	0.474
Health insurance dummies: Uninsured (ref.)				
Publicly insured	0.180	0.384	0.208	0.406
Privately insured	0.617	0.486	0.611	0.487

State-level controls				
% Unemployment rate	5.896	1.883	6.706	2.220
% Uninsured rate	15.70	4.967	14.46	4.023
$ Average personal income	33,840	6,997	39,448	8,539
$ Average household income	43,911	7,641	50,208	7,681
Decriminalization law	0.157	0.363	0.552	0.497
Prescription drug monitoring law	0.566	0.496	0.774	0.418

Table 1.4: Ages 18-39 - Average marginal effects on opioid spending

	Extensive margin		Intensive margin		Combined	
Any MML	0.00105		-37.46***		-2.473***	
	(0.00361)		(12.64)		(0.884)	
Retail dispensary		-0.00499		-13.23		-1.205
		(0.00430)		(15.23)		(1.067)
Home cultivation		-0.00363		-56.29**		-4.042**
		(0.00632)		(26.09)		(1.818)
Non-specific pain		0.00855		12.65		1.384
		(0.00620)		(25.02)		(1.741)
Patient registry		0.00255		6.415		0.592
		(0.00409)		(24.20)		(1.661)
N	186,144	186,144	12,894	12,894	186,144	186,144
Baseline means of outcomes	0.0693	0.0693	69.68	69.68	4.827	4.827

(a) Standard errors in parentheses are clustered at the state level. Controls include state and year fixed effects, state-specific linear time trends, and all the controls listed in Table 3. *** p<0.01, ** p<0.05, * p<0.1

Table 1.5: Ages 40-64 - Average marginal effects on opioid spending

	Extensive margin		Intensive margin		Combined	
Any MML	-0.000389		6.531		0.572	
	(0.00401)		(44.99)		(4.474)	
Retail dispensary		-0.014**		12.85		-0.646
		(0.00446)		(32.32)		(3.281)
Home cultivation		-0.00569		61.41		4.964
		(0.0105)		(73.70)		(7.508)
Non-specific pain		0.00885		-45.15		-2.803
		(0.0109)		(66.74)		(6.776)
Patient registry		0.00167		-15.18		-1.174
		(0.00756)		(57.06)		(5.771)
N	180,723	180,723	18,220	18,220	180,723	180,723
Baseline means of outcomes	0.101	0.101	207.1	207.1	20.88	20.88

(a) Standard errors in parentheses are clustered at the state level. Controls include state and year fixed effects, state-specific linear time trends, and all the controls listed in Table 3. *** p<0.01, ** p<0.05, * p<0.1

Table 1.6: Ages 65+ - Average marginal effects on opioid spending

	Extensive margin		Intensive margin		Combined	
Any MML	-0.000999		75.82		7.917	
	(0.00895)		(50.39)		(5.591)	
Retail dispensary		0.00539		2.214		1.153
		(0.0116)		(42.50)		(4.948)
Home cultivation		0.00190		145.9		15.88
		(0.0183)		(90.90)		(10.19)
Non-specific pain		0.00329		-75.92		-7.535
		(0.0169)		(99.42)		(10.98)
Patient registry		-0.00897		48.30		3.625
		(0.0138)		(94.74)		(10.37)
N	68,168	68,168	7,227	7,227	68,168	68,168
Baseline means of outcomes	0.106	0.106	171.8	171.8	18.22	18.22

(a) Standard errors in parentheses are clustered at the state level. Controls include state and year fixed effects, state-specific linear time trends, and all the controls listed in Table 3. *** p<0.01, ** p<0.05, * p<0.1

Table 1.7: Effects of different policy combinations on opioid expenditures

	18-39	40-64	65+
home+dispensary+nsp	-3.863***	1.514	9.500
(California)	(1.373)	(6.199)	(11.71)
home+dispensary+nsp+reg	-3.271**	0.341	13.12
(Colorado)	(1.332)	(7.982)	(8.190)
dispensary+nsp+reg	0.771	-4.623	-2.757
(New Jersey)	(2.140)	(7.621)	(11.50)
home+nsp+reg	-2.066	0.987	11.97
(Alaska)	(1.820)	(8.157)	(8.921)
N	184,144	180,723	68,168

(a) Standard errors in parantheses are clustered at the state level. Controls include state and year fixed effects, state-specific linear time trends, and all the controls listed in Table 3. *** p<0.01, ** p<0.05, * p<0.1. nsp and reg mean non-pain specification and patient registry respectively.

Table 1.8: Average marginal effects on opioid pills

	18-39		40-64		65+	
Any MML	-2.160**		1.310		0.979	
	(0.869)		(2.753)		(4.650)	
Retail dispensary		-1.440*		-0.153		2.722
		(0.847)		(3.941)		(6.773)
Home cultivation		-3.166		3.382		-2.562
		(2.088)		(4.680)		(13.60)
Non-specific pain		1.157		-6.588		-0.722
		(2.123)		(4.904)		(13.94)
Patient registry		0.0881		5.282		6.178
		(1.410)		(4.997)		(9.927)
N	184,144	186,144	180,723	180,723	68,168	68,168
Baseline means of outcomes	7.965	7.965	24.92	24.92	27.07	27.07

(a) Standard errors in parentheses are clustered at the state level. Controls include state and year fixed effects, state-specific linear time trends, and all the controls listed in Table 3. *** p<0.01, ** p<0.05, * p<0.1

Table 1.9: Other medicines Ages 18-39

	Non-opioid painkillers		Antidepressants		Anticonvulsants		Sedatives	
Any MML	3.096		-0.128		1.474		-1.468*	
	(2.215)		(3.768)		(4.310)		(0.356)	
Retail dispensary		-3.337		-4.910**		-6.062**		1.139
		(2.315)		(2.351)		(2.775)		(0.792)
Home cultivation		3.720		4.726		-0.648		-1.936
		(4.192)		(7.166)		(6.064)		(1.332)
Non-specific pain		0.778		-3.249		-5.301		2.516**
		(4.544)		(7.350)		(5.963)		(1.255)
Patient registry		3.620		1.328		10.09*		-4.514***
		(3.438)		(4.712)		(6.117)		(1.064)
N	186,144	186,144	186,144	186,144	186,144	186,144		
Baseline means of outcomes	13.13	13.13	22.57	22.57	15.31	15.31	6.119	6.119

(a) Standard errors in parantheses are clustered at the state level. Controls include state and year fixed effects, state-specific linear time trends, and all the controls listed in Table 3. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.10: Other medicines Ages 40-64

	Non-opioid painkillers		Antidepressants		Anticonvulsants		Sedatives	
Any MML	-3.501		5.781		-3.017		-2.848	
	(4.360)		(6.036)		(4.986)		(4.571)	
Retail dispensary		-5.356		3.033		-0.273		-3.702
		(6.839)		(4.272)		(8.870)		(3.068)
Home cultivation		-10.97		-4.213		-10.97*		7.176
		(7.130)		(12.47)		(6.334)		(5.834)
Non-specific pain		8.460		7.957		10.20*		-4.428
		(6.158)		(12.84)		(5.614)		(5.551)
Patient registry		-0.718		0.587		-4.547		-4.640
		(9.110)		(7.502)		(12.12)		(4.778)
N	180,723	180,723	180,723	180,723	180,723	180,723		
Baseline means of outcomes	48.10	48.10	61.0	61.0	36.7	36.7	21.74	21.74

(a) Standard errors in parantheses are clustered at the state level. Controls include state and year fixed effects, state-specific linear time trends, and all the controls listed in Table 3. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.11: Other medicines Ages 65+

	Non-opioid painkillers		Antidepressants		Anticonvulsants		Sedatives	
Any MML	-5.075		-1.094		-1.083		-6.747**	
	(9.742)		(9.286)		(10.63)		(3.205)	
Retail dispensary		4.217		-4.523		-18.86***		4.973*
		(7.993)		(8.185)		(6.963)		(2.712)
Home cultivation		-3.039		-33.42***		-18.55		-3.732
		(17.60)		(11.22)		(18.43)		(5.538)
Non-specific pain		-30.13*		15.93		10.70		-2.138
		(17.17)		(11.15)		(15.87)		(4.861)
Patient registry		15.89		15.67		11.51		-10.18**
		(13.16)		(11.61)		(11.80)		(4.147)
N	68,168	68,168	68,168	68,168	68,168	68,168		
Baseline means of outcomes	66.81	66.81	53.98	53.98	34.22	34.22	22.34	22.34

(a) Standard errors in parantheses are clustered at the state level. Controls include state and year fixed effects, state-specific linear time trends, and all the controls listed in Table 3. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.12: Placebo Test Ages 18-39

	Hormones		Hypertension drugs		Cardiovascular agents		Acid reducers	
Any MML	-3.286		-		-		0.271	
	(2.484)						(0.372)	
Retail dispensary		-2.030		-		-		0.842*
		(2.949)						(0.446)
Home cultivation		-6.508		-		-		0.409
		(5.089)						(1.274)
Non-specific pain		9.356*		-		-		-0.118
		(4.963)						(1.247)
Patient registry		-6.056*		-		-		-0.713
		(3.096)						(0.746)
N	186,144	186,144	186,144	186,144	186,144	186,144		

(a) Standard errors in parantheses are clustered at the state level. Controls include state and year fixed effects, state-specific linear time trends, and all the controls listed in Table 3. *** p<0.01, ** p<0.05, * p<0.1. There are not enough users of hypertension drugs and cardiovascular agents in this sample.

Table 1.13: Placebo Test Ages 40-64

	Hormones		Hypertension drugs		Cardiovascular agents		Acid reducers	
Any MML	-2.573		1.043		-0.164		-0.568	
	(3.995)		(2.944)		(1.275)		(2.319)	
Retail dispensary		4.297***		12.15***		7.013**		-2.738
		(2.070)		(4.502)		(3.520)		(1.676)
Home cultivation		-4.527		-7.535		-2.283		4.660
		(9.959)		(8.398)		(2.589)		(3.024)
Non-specific pain		-2.416		5.586		2.032		-2.280
		(9.775)		(7.745)		(2.238)		(2.659)
Patient registry		-1.570		-10.89		-7.095*		1.105
		(3.935)		(5.647)		(3.847)		(2.867)
N	180,723	180,723	180,723	180,723	180,723	180,723	180,723	180,723

(a) Standard errors in parantheses are clustered at the state level. Controls include state and year fixed effects, state-specific linear time trends, and all the controls listed in Table 3. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.14: Placebo Test Ages 65+

	Hormones		Hypertension drugs		Cardiovascular agents		Acid reducers	
Any MML	2.474		-4.601		-0.999		8.719**	
	(3.918)		(8.103)		(4.212)		(3.726)	
Retail dispensary		-0.0800		-13.30		-2.180		-3.721
		(2.930)		(8.464)		(3.903)		(3.077)
Home cultivation		11.80		10.38		-0.809		-6.720
		(12.11)		(16.35)		(6.702)		(7.435)
Non-specific pain		-6.614		3.028		-6.906		14.43**
		(11.92)		(14.66)		(5.452)		(6.952)
Patient registry		-0.714		-6.701		8.236		3.752
		(6.701)		(11.36)		(5.842)		(6.213)
N	68,186	68,186	68,186	68,186	68,186	68,186	68,186	68,186

(a) Standard errors in parantheses are clustered at the state level. Controls include state and year fixed effects, state-specific linear time trends, and all the controls listed in Table 3. *** p<0.01, ** p<0.05, * p<0.1.

Table 2.1: Means of control variables

	Other states N=296,661	**Massachusetts N=4,707**
Individual-level controls		
Age dummies: Ages 18-24 (ref.)		
Ages 25-29	0.113	0.109
Ages 30-34	0.109	0.118
Ages 35-39	0.111	0.107
Ages 40-44	0.117	0.114
Ages 45-49	0.115	0.106
Ages 50-54	0.116	0.106
Ages 55-59	0.101	0.106
Ages 60-64	0.084	0.099
Male	0.492	0.486
Race dummies: White (ref.)		
Black	0.285	0.258
Hispanic	0.150	0.108
Other	0.153	0.182
Married	0.554	0 .486
Education dummies: Less than high school (ref.)		
High school graduate	0.247	0.206
Some college	0.069	0.083
College graduate	0.223	0.279
Unemployed	0.173	0.157
Student	0.044	0.064
Family income as % of poverty line: Poor (ref)		
Near poor	0.037	0.023
Low income	0.122	0.082
Middle income	0.306	0.267
High income	0.417	0.543

Table 2.2: Means of outcome variables

	Other states mean	Massachusetts mean	Difference
Counts			
Prescriptions	8.98	7.92	1.603**
Inpatient visits	0.09	0.09	-0.00536
Outpatient visits	0.48	0.79	-0.312***
Office visits	4.97	6.12	-1.156***
ER visits	0.18	0.16	0.0144
Total	14.21	14.30	-0.0840
Probability of any use			
Prescriptions	0.64	0.65	-0.0132
Inpatient visits	0.07	0.08	-0.00535
Outpatient visits	0.16	0.21	-0.0534***
Office visits	0.70	0.75	-0.0544***
ER visits	0.13	0.12	0.00912
Any utilization	0.78	0.81	-0.0415***
$ Spending			
Prescriptions	557.65	478.90	78.75**
Inpatient visits	657.34	627.85	29.50
Outpatient visits	239.73	251.04	-11.31
Office visits	532.59	555.69	-23.10
ER visits	86.01	63.31	22.70
Total	1833.60	1725.75	107.8

(a) Differences between other states and Massachusetts is significant. *** significant at the 0.01% level, ** significant at the 0.05% level, * significant at the 0.1%. MEPS sampling weights are used.

Table 2.3: Effects of the reform on health care utilization of counts: Average marginal effects from negative binomial

	Prescriptions	**Inpatient**	**Outpatient**
MA*Post	-0.595**	0.0296***	0.328***
	(0.287)	(0.00344)	(0.0345)
	Office	**ER**	**Total**
MA*Post	0.844***	0.0533***	1.183***
	(0.142)	(0.00448)	(0.352)
State and year f.e	Y	Y	Y
State specific linear time trends	Y	Y	Y
Sample size	301,368	301,368	301,368

(a) Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. Controls include all the controls listed in Table 2.1. *** p<0.01, ** p<0.05, * p<0.1. MEPS sampling weights are used.

Table 2.4: Effects of the reform on health care utilization of counts: Average marginal effects from hurdle model

	Prescriptions	**Inpatient**	**Outpatient**
MA*Post	-0.521**	0.0346***	0.343***
	(0.260)	(0.00327)	(0.0235)
	Office	**ER**	**Total**
MA*Post	0.710***	0.0559***	0.928***
	(0.121)	(0.00526)	(0.311)
State and year f.e	Y	Y	Y
State specific linear time trends	Y	Y	Y
Sample size	301,368	301,368	301,368

(a) Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. Controls include all the controls listed in Table 2.1. *** p<0.01, ** p<0.05, * p<0.1. MEPS sampling weights are used.

Table 2.5: Effects of the reform on health care utilization on spending: Average marginal effects from two-part model

	Prescriptions	**Inpatient**	**Outpatient**
MA*Post	203.8***	386.1***	203.6***
	(35.60)	(69.11)	(22.40)
	Office	**ER**	**Total**
MA*Post	229.2***	84.95***	942.0***
	(23.97)	(9.215)	(93.97)
State and year f.e	Y	Y	Y
State specific linear time trends	Y	Y	Y
Sample size	301,368	301,368	301,368

(a) Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. Controls include all the controls listed in Table 2.1. *** p<0.01, ** p<0.05, * p<0.1. MEPS sampling weights are used.

Table 2.6: Effects of the reform on health care utilization on the extensive margin: Average marginal effects from probit

	Prescriptions	**Inpatient**	**Outpatient**
MA*Post	0.0363***	0.0148***	0.0882***
	(0.00619)	(0.00206)	(0.00544)
	Office	**ER**	**Any utilization**
MA*Post	0.0676***	0.0451***	0.0574***
	(0.00469)	(0.00264)	(0.00420)
State and year f.e	Y	Y	Y
State specific linear time trends	Y	Y	Y
Sample size	301,368	301,368	301,368

(a) Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. Controls include all the controls listed in Table 2.1. *** p<0.01, ** p<0.05, * p<0.1. MEPS sampling weights are used.

Table 2.7: Effects of the reform on health care utilization on the intensive margin: Average marginal effects from GLM

	Prescriptions	**Inpatient**	**Outpatient**
MA*Post	277.5***	1,855**	243.2*
	(58.50)	(930.4)	(139.3)
Sample size	176,730	18,748	34,788
	Office	**ER**	**Any utilization**
MA*Post	271.7***	240.9***	1,113***
	(36.77)	(73.25)	(125.0)
State and year f.e	Y	Y	Y
State specific linear time trends	Y	Y	Y
Sample size	163,614	31,689	210,453

(a) Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. Controls include all the controls listed in Table 2.1. *** p<0.01, ** p<0.05, * p<0.1. MEPS sampling weights are used. GLM uses log for link function and gamma as its distributional family.

Table 2.8: Match on pretreatment levels: Average marginal effects from negative binomial

	Prescriptions	**Inpatient**	**Outpatient**
MA*Post	-0.453	0.0201	0.568***
	(1.229)	(0.0123)	(0.120)
Sample size	71,913	84,665	81,051
	Office	**ER**	**Any utilization**
MA*Post	0.948	0.0519**	1.720
	(0.736)	(0.0212)	(1.680)
State and year f.e	Y	Y	Y
State specific linear time trends	Y	Y	Y
Sample size	69,378	104,438	92,370

(a) Standard errors, heteroscedasticity-robust and clustered by state-year, are in parentheses. Controls include all the controls listed in Table 2.1. *** p<0.01, ** p<0.05, * p<0.1. MEPS sampling weights are used. GLM uses log for link function and gamma as its distributional family.

Table 2.9: Match on pretreatment trends: Average marginal effects from negative binomial

	Prescriptions	**Inpatient**	**Outpatient**
MA*Post	-0.720	0.0295*	0.149***
	(0.949)	(0.0177)	(0.0456)
Sample size	38,624	25,887	76,939
	Office	**ER**	**Any utilization**
MA*Post	0.964**	0.0382**	2.845***
	(0.452)	(0.0181)	(1.090)
State and year f.e	Y	Y	Y
State specific linear time trends	Y	Y	Y
Sample size	57,374	47,908	34,369

(a) Standard errors, heteroscedasticity-robust and clustered by state-year, are in parentheses. Controls include all the controls listed in Table 2.1. *** p<0.01, ** p<0.05, * p<0.1. MEPS sampling weights are used. GLM uses log for link function and gamma as its distributional family.

Table 2.10: Match on pretreatment coverage: Average marginal effects from negative binomial

	Prescriptions	**Inpatient**	**Outpatient**
MA*Post	-0.0955	0.0452***	0.682***
	(1.262)	(0.0159)	(0.113)
	Office	**ER**	**Any utilization**
MA*Post	1.331**	0.0664**	2.296
	(0.644)	(0.0266)	(1.588)
State and year f.e	Y	Y	Y
State specific linear time trends	Y	Y	Y
Sample size	44,856	44,856	44,856

(a) Standard errors, heteroscedasticity-robust and clustered by state-year, are in parentheses. Controls include all the controls listed in Table 2.1. *** p<0.01, ** p<0.05, * p<0.1. MEPS sampling weights are used. GLM uses log for link function and gamma as its distributional family.

Table 2.11: New England states: Average marginal effects from negative binomial

	Prescriptions	**Inpatient**	**Outpatient**
MA*Post	2.200*	0.0926***	0.734***
	(1.237)	(0.0181)	(0.183)
	Office	**ER**	**Any utilization**
MA*Post	3.389***	0.0463	6.393***
	(0.587)	(0.0398)	(1.430)
State and year f.e	Y	Y	Y
State specific linear time trends	Y	Y	Y
Sample size	11,637	11,637	11,637

(a) Standard errors, heteroscedasticity-robust and clustered by state-year, are in parentheses. Controls include all the controls listed in Table 2.1. *** p<0.01, ** p<0.05, * p<0.1. MEPS sampling weights are used. GLM uses log for link function and gamma as its distributional family.

Table 2.12: Synthetic control group: Average marginal effects from negative binomial

	Prescriptions	**Inpatient**	**Outpatient**
MA*Post	-1.286	0.0624***	0.832***
	(1.013)	(0.0123)	(0.152)
Sample size	21,335	22,677	29,459
	Office	**ER**	**Any utilization**
MA*Post	3.089***	0.0683***	1.604
	(0.581)	(0.0173)	(1.440)
State and year f.e	Y	Y	Y
State specific linear time trends	Y	Y	Y
Sample size	21,787	29,755	21,787

(a) Standard errors, heteroscedasticity-robust and clustered by state-year, are in parentheses. Controls include all the controls listed in Table 2.1. *** p<0.01, ** p<0.05, * p<0.1. MEPS sampling weights are used. GLM uses log for link function and gamma as its distributional family.

Table 2.13: Event study: Average marginal effects from negative binomial

	Prescriptions	Inpatient	Outpatient	Office	ER	Total
MA*late before	0.201	0.0159***	-0.0201	-0.695***	0.0211***	-1.150**
	(0.363)	(0.00367)	(0.0491)	(0.168)	(0.00508)	(0.470)
MA*during	0.526	0.0704***	0.421***	0.993***	0.0741***	1.920***
	(0.511)	(0.00567)	(0.0745)	(0.248)	(0.00736)	(0.640)
MA*early after	2.943***	0.0761***	0.353***	1.103***	0.138***	3.464***
	(0.636)	(0.00837)	(0.0971)	(0.325)	(0.00956)	(0.826)
MA*late after	5.777***	0.133***	0.564***	2.552***	0.171***	7.338***
	(0.773)	(0.0106)	(0.129)	(0.457)	(0.0142)	(1.067)
State and year f.e	Y	Y	Y	Y	Y	Y
State specific linear time trends	Y	Y	Y	Y	Y	Y
Sample size	301,368	301,368	301,368	301,368	3301,368	301,368

(a) Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. Controls include all the controls listed in Table 3.2. *** p<0.01, ** p<0.05, * p<0.1. MEPS sampling weights are used.

Table 2.14: Instrumental variables

First stage: any insurance coverage						
MA*Post	0.0530***					
	(0.00585)					
Second stage: health care utilization						
	Prescriptions	Inpatient	Outpatient	Office	ER	Total
Insurance	8.373***	0.0848***	0.449***	4.225	0.0639***	12.57***
	(0.408)	(0.00442)	(0.0233)	(4.790)	(0.00827)	(0.961)
First stage residual	-22.15***	1.661***	-1.792***	55.44***	2.867***	43.76***
	(3.576)	(0.0481)	(0.450)	(4.187)	(0.0676)	(4.853)
State and year f.e	Y	Y	Y	Y	Y	Y
State specific linear time trends	Y	Y	Y	Y	Y	Y
Sample size	301,368	301,368	301,368	301,368	301,368	301,368

(a) Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. Controls include all the controls listed in Table 3.2. *** p<0.01, ** p<0.05, * p<0.1. MEPS sampling weights are used.

Table 3.1: Summary statistics for outcome variables for all the samples

	2013-2014		2014-2015	
	mean	**s.d**	**mean**	**s.d**
Insured	0.83	0.37	0.87	0.34
Counts				
Prescriptions	9.90	20.51	10.12	20.58
Inpatient visits	0.08	0.36	0.07	0.36
Outpatient visits	0.42	2.36	0.46	2.48
Office visits	5.26	11.25	5.39	11.64
ER visits	0.19	0.64	0.19	0.63
Total	15.85	27.88	16.23	28.20
Probability of any use				
Prescriptions	0.61	0.49	0.61	0.49
Inpatient visits	0.06	0.24	0.06	0.23
Outpatient visits	0.15	0.36	0.15	0.36
Office visits	0.68	0.47	0.69	0.46
ER visits	0.12	0.33	0.12	0.33
Any utilization	0.76	0.43	0.77	0.42
$ Spending (among users)				
Prescriptions	1668	6115	1833	7554
Inpatient visits	16630	25317	19180	33911
Outpatient visits	2895	7165	3093	8371
Office visits	1529	3747	1603	4414
ER visits	1736	3478	1883	4278
Total	4914	13116	5253	15634

Table 3.2: Summary statistics for control variables

	2013-2014		2014-2015	
	mean	**s.d**	**mean**	**s.d**
Individual-level controls				
Demographic controls				
Age dummies: Ages 19-24 (ref.)				
Ages 25-29	0.11	0.32	0.12	0.32
Ages 30-34	0.11	0.31	0.11	0.31
Ages 35-39	0.10	0.31	0.10	0.30
Ages 40-44	0.10	0.30	0.10	0.30
Ages 45-49	0.10	0.30	0.10	0.30
Ages 50-54	0.12	0.33	0.12	0.32
Ages 55-59	0.11	0.32	0.11	0.32
Ages 60-64	0.10	0.30	0.10	0.30
Male	0.49	0.498	0.49	0.50
Race dummies: White (ref.)				
Black	0.13	0.33	0.13	0.33
Hispanic	0.17	0.38	0.17	0.38
Other	0.09	0.28	0.09	0.29
Married	0.53	0.50	0.53	0.50
Dummies for number of children in home: None (ref.)				
One child	0.19	0.39	0.19	0.40
Two children	0.16	0.37	0.16	0.37
Three children	0.07	0.25	0.07	0.25
Four children	0.02	0.14	0.02	0.14
Five children or more	0.01	0.08	0.01	0.08
Economic controls				
Education dummies: Less than high school (ref.)				
High school graduate	0.19	0.39	0.14	0.35
Some college	0.21	0.41	0.15	0.36
College graduate	0.29	0.46	0.22	0.41
Unemployed	0.18	0.39	0.18	0.38
Student	0.05	0.21	0.04	0.21
Family income as % of poverty line: Poor (ref)				
Near poor	0.04	0.20	0.04	0.19
Low income	0.13	0.33	0.12	0.33
Middle income	0.30	0.46	0.29	0.45
High income	0.40	0.49	0.42	0.49

Table 3.3: Effects of ACA on probability of having insurance coverage: LPM results

	2013-2014	**2014-2015**
Coefficient Estimates of Interest		
Post*Treatment	0.398***	0.332***
	(0.0189)	(0.0297)
Medicaid expansion*Post*	0.0717**	0.0789**
Treatment	(0.0298)	(0.0378)
Implied Effect of ACA		
Full ACA	0.469***	0.411***
	(0.0231)	(0.0242)
Sample size	39,914	39,032

(a) Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. Controls include all the controls listed in Table 3.2. *** p<0.01, ** p<0.05, * p<0.1. MEPS sampling weights are used.

Table 3.4: Effects of ACA on health care utilization of counts between 2013-2014: Average marginal effects from negative binomial

	Prescriptions	Inpatient	Outpatient	Office	ER	Total
Coefficient Estimates of Interest						
Post*Treatment	0.487	-0.000723	0.228***	1.481***	0.00511	2.197
	(1.165)	(0.0144)	(0.0857)	(0.498)	(0.0272)	(1.391)
Medicaid expansion*Post*	0.0165	0.0386	-0.155	0.985	-0.0115	1.650
Treatment	(1.355)	(0.0238)	(0.162)	(0.699)	(0.0387)	(1.568)
Implied Effect of ACA						
Full ACA	0.504	0.0379***	0.0723	2.465***	-0.00637	3.847***
	(0.730)	(0.0187)	(0.132)	(0.474)	(0.0279)	(0.783)
Sample size	39,914	39,914	39,914	39,914	39,914 7	39,914

(a) Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. Controls include all the controls listed in Table 3.2. *** p<0.01, ** p<0.05, * p<0.1. MEPS sampling weights are used.

Table 3.5: Effects of ACA on health care utilization of counts between 2014-2015: Average marginal effects from negative binomial

	Prescriptions	Inpatient	Outpatient	Office	ER	Total
Coefficient Estimates of Interest						
Post*Treatment	1.656*	0.0499**	0.208	0.536	-0.0106	2.244
	(0.864)	(0.0215)	(0.169)	(0.844)	(0.0237)	(1.559)
Medicaid expansions*Post*	0.563	-0.0370	-0.170	-0.202	0.0218	0.207
Treatment	(1.518)	(0.0409)	(0.303)	(1.018)	(0.0404)	(2.055)
Implied Effect of ACA						
Full ACA	2.219*	0.0129	0.0385	0.334	0.0112	2.451*
	(1.266)	(0.0341)	(0.246)	(0.580)	(0.0344)	(1.351)
Sample size	39,032	39,032	39,032	39,032	39,032	39,032

(a) Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. Controls include all the controls listed in Table 3.2. *** p<0.01, ** p<0.05, * p<0.1. MEPS sampling weights are used.

Table 3.6: Effects of ACA on health care Utilization of Counts Between 2013-2014: Average marginal effects from poisson

	Prescriptions	Inpatient	Outpatient	Office	ER	Total
Coefficient Estimates of Interest						
Post*Treatment	1.764	0.00442	0.268***	1.725***	0.0139	3.688**
	(1.272)	(0.0138)	(0.102)	(0.455)	(0.0244)	(1.446)
Medicaid expansions*Post*	-0.621	0.0327	-0.314*	0.946	-0.00846	0.294
Treatment	(1.463)	(0.0232)	(0.170)	(0.728)	(0.0364)	(1.872)
Implied Effect of ACA						
Full ACA	1.143	0.0371**	-0.0454	2.670***	0.00548	3.982***
	(0.757)	(0.0185)	(0.131)	(0.585)	(0.0271)	(1.230)
Sample size	39,914	39,914	39,914	39,914	39,914 7	39,914

(a) Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. Controls include all the controls listed in Table 3.2. *** p<0.01, ** p<0.05, * p<0.1. MEPS sampling weights are used.

Table 3.7: Effects of ACA on health care utilization of counts between 2014-2015: Average marginal effects from poisson

	Prescriptions	Inpatient	Outpatient	Office	ER	Total
Coefficient Estimates of Interest						
Post*Treatment	1.441	0.0478**	0.0727	0.582	-0.0110	2.053
	(1.054)	(0.0204)	(0.136)	(0.732)	(0.0232)	(1.391)
Medicaid expansions*Post*	0.0990	-0.0294	-0.0798	-0.924	0.0278	-0.923
Treatment	(1.607)	(0.0377)	(0.262)	(1.094)	(0.0402)	(2.034)
Implied Effect of ACA						
Full ACA	1.540	0.0184	-0.00710	-0.342	0.0168	1.130
	(1.266)	(0.0313)	(0.218)	(0.823)	(0.0341)	(1.527)
Sample size	39,032	39,032	39,032	39,032	39,032	39,032

(a) Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. Controls include all the controls listed in Table 3.2. *** p<0.01, ** p<0.05, * p<0.1. MEPS sampling weights are used.

Table 3.8: Effect of ACA on health care expenditures 2013-2014: Average marginal effects from two-part model (First part: Probit, Second part: GLM with a log link and gamma family)

	Prescriptions	Inpatient	Outpatient	Office	ER	Total
Coefficient Estimates of Interest						
Post*Treatment	-89.51	134.6	319.3**	19.92	-76.44	123.2
	(153.1)	(306.3)	(156.2)	(123.0)	(53.90)	(358.5)
Medicaid expansions*Post*	449.4*	-374.7	-315.5	395.8*	-51.03	434.9
Treatment	(241.9)	(511.2)	(210.6)	(227.8)	(96.24)	(642.8)
Implied Effect of ACA						
Full ACA	359.9	-240	3.797	415.8**	-127.5	558.1
	(194.4)	(403.7)	(139.3)	(185.7)	(79.01)	(551.9)
Sample size	39,914	39,914	39,914	39,914	39,914	39,914

(a) Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. Controls include all the controls listed in Table 3.2. *** p<0.01, ** p<0.05, * p<0.1. MEPS sampling weights are used.

Table 3.9: Effect of ACA on probability of health care utilization 2013-2014: Average marginal effects from probit model

	Prescriptions	Inpatient	Outpatient	Office	ER	Total
Coefficient Estimates of Interest						
Post*Treatment	0.0188	0.0114	0.0709***	0.0630***	0.0202	0.0260*
	(0.0162)	(0.0109)	(0.0184)	(0.0168)	(0.0130)	(0.0139)
Medicaid expansions*Post*	0.0163	0.0222	-0.0369	0.0356	-0.0165	0.0511**
Treatment	(0.0226)	(0.0181)	(0.0265)	(0.0266)	(0.0259)	(0.0227)
Implied Effect of ACA						
Full ACA	0.0351**	0.0335**	0.0339	0.0986***	0.00371	0.0770***
	(0.0151)	(0.0140)	(0.0200)	(0.0202)	(0.0226)	(0.0178)
Sample size	39,914	39,914	39,914	39,914	39,914	39,914

(a) Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. Controls include all the controls listed in Table 3.2. *** p<0.01, ** p<0.05, * p<0.1. MEPS sampling weights are used.

Table 3.10: Effect of ACA on probability of health care expenditures on the intensive margin 2013-2014: Average marginal effects from GLM

	Prescriptions	Inpatient	Outpatient	Office	ER	Total
Coefficient Estimates of Interest						
Post*Treatment	-216.4	-835.5	1,128	-102.2	-907.9**	23.61
	(291.3)	(3,942)	(1,178)	(200.8)	(407.9)	(506.9)
Medicaid expansions*Post*	823.4*	-11,796*	-1,779	576.7	-188.2	321.9
Treatment	(460.1)	(6,698)	(1,585)	(371.1)	(699.8)	(911.0)
Implied Effect of ACA						
Full ACA	607.1	-12632**	-651.3	474.6	-1096**	345.5
	(371.2)	(5457)	(1044)	(301.7)	(560.7)	(786)
Sample size	22,190	2,412	5,071	24,877	5,185	28,134

(a) Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. Controls include all the controls listed in Table 3.2. *** p<0.01, ** p<0.05, * p<0.1. MEPS sampling weights are used.

Table 3.11: Effect of ACA on health care utilization of counts 2011-2012: Average marginal effects from negative binomial

	Prescriptions	Inpatient	Outpatient	Office	ER	Total
Coefficient Estimates of Interest						
Post*Treatment	-0.456	0.0145	-0.0195	0.355	0.0273	-0.152
	(0.832)	(0.0197)	(0.0784)	(0.501)	(0.0352)	(1.132)
Medicaid expansions*Post*	2.372*	-0.0137	0.122	0.232	-0.0352	2.535
Treatment	(1.236)	(0.0259)	(0.131)	(0.877)	(0.0448)	(1.801)
Implied Effect of ACA						
Full ACA	1.916**	0.000768	0.102	0.587	-0.00792	2.383
	(0.970)	(0.0171)	(0.105)	(0.742)	(0.0292)	(1.494)
Sample size	41,361	41,361	41,361	41,361	41,361	41,361

(a) Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. Controls include all the controls listed in Table 3.2. *** p<0.01, ** p<0.05, * p<0.1. MEPS sampling weights are used.

Table 3.12: Effect of ACA on health care utilization of counts 2012-2013: Average marginal effects from negative binomial

	Prescriptions	Inpatient	Outpatient	Office	ER	Total
Coefficient Estimates of Interest						
Post*Treatment	0.958	-0.00929	0.0335	1.347***	0.0238	2.423**
	(1.033)	(0.0242)	(0.118)	(0.402)	(0.0234)	(1.154)
Medicaid expansions*Post*	0.418	0.0583	-0.00165	-1.411*	0.0460	-0.776
Treatment	(1.829)	(0.0362)	(0.150)	(0.726)	(0.0394)	(2.284)
Implied Effect of ACA						
Full ACA	1.375	0.0491	0.0319	-0.0640	0.0698**	1.647
	(1.573)	(0.0270)	(0.0862)	(0.610)	(0.0316)	(2.039)
Sample size	42,377	42,377	42,377	42,377	42,377	42,377

(a) Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. Controls include all the controls listed in Table 3.2. *** p<0.01, ** p<0.05, * p<0.1. MEPS sampling weights are used.

Appendix

Table A1.1: Other medicines Ages 18-39 (Extensive margin)

	Non-opioid painkillers		Antidepressants		Anticonvulsants		Sedatives	
Any MML	0.00330		0.00537		0.00446*		0.00443	
	(0.00741)		(0.00354)	(0.00266)		(0.00326)		
Retail dispensary		0.00885*		-0.000818		-0.000414		0.00928***
		(0.00503)		(0.00311)		(0.00233)		(0.00240)
Home cultivation		0.0159		0.00195		-0.00288		-0.00490
		(0.0107)		(0.00335)		(0.00327)		(0.00636)
Non-specific pain		-0.0116		0.00678**		0.00648**		0.0141**
		(0.000806)		(0.00346)		(0.00329)		(0.00649)
Patient registry		0.000806		-0.00339		0.00126		-0.0133***
		(0.00578)		(0.00494)		(0.00401)		(0.00286)
N	186,144	186,144	186,144	186,144	186,144	186,144	186,144	186,144
Baseline means of outcomes	0.131	0.131	0.0531	0.0531	0.0261	0.0261	0.0411	0.0411

(a) Coefficients are average marginal effects from probit. Standard errors in parantheses are clustered at the state level. Controls include state and year fixed effects, state-specific linear time trends, and all the controls listed in Table 3. *** p<0.01, ** p<0.05, * p<0.1.

Table A1.2: Other medicines Ages 40-64 (Extensive margin)

	Non-opioid painkillers		Antidepressants		Anticonvulsants		Sedatives	
Any MML	0.000403		-0.00296		0.00377		-0.00650	
	(0.00501)		(0.00652)		(0.00443)		(0.00527)	
Retail dispensary		-0.00754		0.00113		-0.0104***		0.00661*
		(0.00475)		(0.00547)		(0.00327)		(0.00368)
Home cultivation		-0.000335		-0.00363		0.0169***		-0.0185**
		(0.00876)		(0.00990)		(0.00455)		(0.00835)
Non-specific pain		-0.00157		0.0167*		0.00103		0.00616
		(0.00732)		(0.00980)		(0.00470)		(0.00789)
Patient registry		0.00714		-0.0130		-0.00448		-0.00735
		(0.00658)		(0.00857)		(0.00465)		(0.00581)
N	180,723	180,723	180,723	180,723	180,723	180,723	180,723	180,723
Baseline means of outcomes	0.214	0.214	0.118	0.118	0.0656	0.0656	0.0932	0.0932

(a) Coefficients are average marginal effects from probit. Standard errors in parantheses are clustered at the state level. Controls include state and year fixed effects, state-specific linear time trends, and all the controls listed in Table 3. *** p<0.01, ** p<0.05, * p<0.1.

Table A1.3: Other medicines Ages 65+ (Extensive margin)

	Non-opioid painkillers		Antidepressants		Anticonvulsants		Sedatives	
Any MML	-0.00427		-0.000350		0.0116		-0.0130	
	(0.0161)		(0.00971)		(0.0129)		(0.00800)	
Retail dispensary		0.0336***		-0.00293		-0.00920		0.00536
		(0.0116)		(0.00849)		(0.00729)		(0.00461)
Home cultivation		0.0125		-0.00440		-0.0159		-0.00448
		(0.0245)		(0.0166)		(0.0163)		(0.0108)
Non-specific pain		-0.0498*		-0.00466		0.0350**		-0.0189**
		(0.0269)		(0.0148)		(0.0136)		(0.00820)
Patient registry		0.0102		0.0190		0.00331		0.00228
		(0.0172)		(0.0175)		(0.0145)		(0.00811)
N	68,168	68,168	68,168	68,168	68,168	68,168	68,168	68,168
Baseline means of outcomes	0.278	0.278	0.129	0.129	0.0921	0.0921	0.102	0.102

(a) Coefficients are average marginal effects from probit. Standard errors in parantheses are clustered at the state level. Controls include state and year fixed effects, state-specific linear time trends, and all the controls listed in Table 3. *** p<0.01, ** p<0.05, * p<0.1.

Table A1.4: Other medicines Ages 18-39 (Intensive margin)

	Non-opioid painkillers		Antidepressants		Anticonvulsants		Sedatives	
Any MML	21.13		-43.84		-40.78		-51.58***	
	(15.93)		(65.38)		(147.3)		(17.98)	
Retail dispensary		-31.88*		-85.83**		-223.5**		-3.666
		(17.17)		(36.74)		(93.73)		(17.85)
Home cultivation		16.67		72.80		37.45		-31.14
		(30.88)		(133.1)		(221.2)		(24.77)
Non-specific pain		14.38		-112.9		-344.0		13.75
		(33.90)		(136.6)		(216.8)		(21.99)
Patient registry		26.97		51.59		359.6*		-66.01***
		(25.79)		(79.91)		(216.3)		(24.60)
N	24,466	24,466	9,890	9,890	4,859	4,859	7,655	7,655
Baseline means of outcomes	99.93	99.93	424.8	424.8	586.6	586.6	148.8	148.8

(a) Coefficients are average marginal effects from GLM with a log link and gamma family. Standard errors in parantheses are clustered at the state level. Controls include state and year fixed effects, state-specific linear time trends, and all the controls listed in Table 3. *** p<0.01, ** p<0.05, * p<0.1.

Table A1.5: Other medicines Ages 40-64 (Intensive margin)

	Non-opioid painkillers		Antidepressants		Anticonvulsants		Sedatives	
Any MML	-16.83		61.54		-76.96		-15.30	
	(19.73)		(43.36)		(68.35)		(47.77)	
Retail dispensary		-17.46		21.07		79.66		-55.76*
		(31.66)		(28.20)		(134.5)		(31.94)
Home cultivation		-50.85		-20.52		-305.7***		121.3**
		(32.16)		(97.30)		(91.13)		(59.62)
Non-specific pain		41.11		-2.468		149.3*		-62.28
		(27.86)		(100.9)		(78.26)		(56.84)
Patient registry		-10.54		59.52		-34.29		-32.54
		(42.12)		(52.72)		(183.7)		(49.76)
N	38,681	38,681	21,299	21,299	11,851	11,851	16,837	16,837
Baseline means of outcomes	224.7	224.7	517.6	517.6	559.6	559.6	233.3	233.3

(a) Coefficients are average marginal effects from GLM with a log link and gamma family. Standard errors in parantheses are clustered at the state level. Controls include state and year fixed effects, state-specific linear time trends, and all the controls listed in Table 3. *** p<0.01, ** p<0.05, * p<0.1.

Table A1.6: Other medicines Ages 65+ (Intensive margin)

	Non-opioid painkillers		Antidepressants		Anticonvulsants		Sedatives	
Any MML	-14.71		-7.324		-57.67		-38.28	
	(32.29)		(64.61)		(65.80)		(26.38)	
Retail dispensary		-13.45		-25.56		-113.8**		37.24
		(27.00)		(57.09)		(47.11)		(24.69)
Home cultivation		-21.64		-243.9***		-83.10		-26.97
		(59.78)		(68.39)		(128.3)		(49.07)
Non-specific pain		-66.02		137.7*		-60.90		19.46
		(57.38)		(72.04)		(115.4)		(44.32)
Patient registry		48.37		60.54		79.17		-104.6***
		(45.09)		(70.11)		(70.68)		(36.78)
N	18,959	18,959	8,822	8,822	6,280	6,280	6,931	6,931
Baseline means of outcomes	240.2	240.2	417.1	417.1	371.5	371.5	219.7	219.7

(a) Coefficients are average marginal effects from GLM with a log link and gamma family. Standard errors in parantheses are clustered at the state level. Controls include state and year fixed effects, state-specific linear time trends, and all the controls listed in Table 3. *** p<0.01, ** p<0.05, * p<0.1.

Table A2.1: Effects of the reform on health care utilization of counts: Average marginal effects from poisson

	Prescriptions	**Inpatient**	**Outpatient**
MA*Post	0.218	0.0305***	0.312***
	(0.249)	(0.00370)	(0.0246)
	Office	**ER**	**Total**
MA*Post	0.429***	0.0566***	0.624*
	(0.142)	(0.00425)	(0.324)
State and year f.e	Y	Y	Y
State specific linear time trends	Y	Y	Y
Sample size	301,368	301,368	301,368

(a) Standard errors, heteroscedasticity-robust and clustered by state, are in parentheses. Controls include all the controls listed in Table 2.1. *** p<0.01, ** p<0.05, * p<0.1. MEPS sampling weights are used.

Table A2.2: Regressions with aggregated data

	Prescriptions	**Inpatient**	**Outpatient**
MA*during	2.068***	0.0338***	0.539***
	(0.786)	(0.00851)	(0.0790)
MA*after	4.450***	0.0102	0.209*
	(0.936)	(0.0132)	(0.116)
	Office	**ER**	**Total**
MA*during	1.118**	0.0139	2.068***
	(0.552)	(0.0179)	(0.786)
MA*after	1.400***	0.0513***	4.450***
	(0.475)	(0.0173)	(0.936)
Dummies for during and after periods	Y	Y	Y
Sample size	813	813	813

(a) Standard errors, heteroscedasticity-robust and clustered by state-year, are in parentheses. Controls include all the controls listed in Table 2.1. *** p<0.01, ** p<0.05, * p<0.1. The control group consists of all 50 states. MEPS sampling weights are used when aggregating.

Table A2.3: Regressions with aggregated data

	Prescriptions	**Inpatient**	**Outpatient**
MA*during	-0.212	0.0337***	0.801***
	(0.238)	(0.00205)	(0.00293)
MA*after	2.144***	0.0125	0.512***
	(0.512)	(0.0103)	(0.0930)
	Office	**ER**	**Total**
MA*during	2.042***	9.54e-05	1.863**
	(0.551)	(0.00946)	(0.778)
MA*after	2.775***	0.0375***	4.969***
	(0.398)	(0.0135)	(0.872)
Year f.e	Y	Y	Y
Sample size	32	32	32

(a) Standard errors, heteroscedasticity-robust are in parentheses. Controls include all the controls listed in Table 2.1 and a dummy for MA. *** p<0.01, ** p<0.05, * p<0.1. The control group consists of one cross-sectional unit collapsing all individuals from the other 50 states. MEPS sampling weights are used when aggregating.

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