ESSAYS ON THE AFFORDABLE CARE ACT MANDATES AND THEIR EFFECTS ON LABOR SUPPLY AND HEALTH OUTCOMES

by

Tiphanie Magne

A dissertation submitted to the Faculty of the University of Delaware in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics

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ABSTRACT

In this dissertation, I study the effects of the Affordable Care Act advance premium tax credits, or ACA "subsidy", on labor supply for households that are not offered employer-sponsored health insurance using premium data from the Robert Wood Johnson Foundation linked to the Medical Expenditure Panel Survey from 2010 to 2017. Due to a sharp decrease to zero in the subsidy for households above 400 percent of the poverty line, households near this cutoff have a financial incentive to reduce their income by decreasing their labor supply at the intensive and/or extensive margins. Thus, I calculate the "potential lost subsidy" (PLS) for households near the cutoff as the subsidy they would receive at exactly 400 percent of the poverty line but may lose if earning just above it. On average, the PLS equals $100 a month for younger workers but is four to six times larger for older workers and greatly varies by geographic location and family size. Using OLS and probit regressions with interaction terms to capture the relevant households, I estimate the impacts of the discontinuity in subsidy on labor supply. I find that income and hours of work do not statistically decrease from one year to another in response to the PLS. Moreover, there is no evidence that the probability one of the household members stops working increases as the PLS gets larger. This null effect is a moderately precise zero and suggests

that the ACA subsidy design does not distort labor supply nor creates income manipulation for households near the 400% FPL cutoff.

I also study the impacts of the Medicaid coverage gap in non-expansion states on labor supply for low-income households. As a result of the Medicaid non-expansion and premium tax credits starting at 100 percent of the poverty line, households just below this threshold face a new labor supply incentive and upward discontinuity in their budget at the poverty line. Using a difference-in-differences approach and the Annual Social and Economic Supplement (ASEC) of the CPS from 2010 to 2018, I estimate labor supply changes within very poor households in Medicaid non-expansion states. I find a significant increase in labor supply at the intensive margin. In particular, childless adults in non-expansion states increase their usual weekly hours by 2 hours a week (estimates equal to 1.7 and 2.3 depending on the specification). However, the coverage gap does not affect the extensive margin of labor supply, and there is no evidence that overall, very poor households adjust their income in response to the Medicaid non-expansion. It is crucial for policy programs to provide affordable health coverage to very poor households, especially as some of them try to respond to the unintended incentive of low-priced health insurance at the poverty line and more individuals may fall into the coverage gap due to adverse income shocks.

Finally, I reassess the impact of medical marijuana laws on opioid overdose mortality in light of the ACA Medicaid expansion using the 1999-2018 multiple cause-of-death mortality data from the National Vital Statistics System. Recent studies find that medical marijuana dispensaries reduce opioid-related mortality

as medical marijuana patients tend to substitute marijuana for opioids. However, the opioid epidemic is getting worse, and little is known about Medicaid expansion impacts on opioid mortality rates. I revisit Powell et al. (2018) on the effects of medical marijuana laws on opioid-related mortality by (1) controlling for early Medicaid expansion, a potentially confounding variable in their study from 1999 to 2013, and (2) extending the analysis to 2018 to provide long-term effects of both medical marijuana dispensaries and Medicaid expansion on opioid overdose mortality. While controlling for early Medicaid expansion does not change the magnitude of Powell's results, I find that early Medicaid expansion significantly reduced opioid-related death rates by 25% before 2014. In addition, when extending the data to 2018, the strong effect of dispensaries persists as they are associated with a 25% reduction in opioid-related death rates. Finally, I find no evidence that higher opioid death rates are caused by Medicaid expansion by 2018 suggesting that Medicaid expansion does not exacerbate the opioid epidemic. Rather, both medical marijuana dispensaries and Medicaid expansion via the coverage of opioid use disorder treatments may be part of the solution in addressing the opioid crisis.

Keywords: Affordable Care Act, Labor Supply, Subsidies, Medicaid Expansion, Opioids, Substance Abuse, Medical Marijuana Laws

Chapter 1

INTRODUCTION

1.1 The Effects of the Affordable Care Act Advance Premium Tax Credits on Household Labor Supply

One of the most important provisions of the Affordable Care Act (ACA) is the advance premium tax credits (APTC), or subsidy, to lower the cost of non-group health insurance for households who are not offered employer-sponsored insurance (ESI) and who earn between 100% and 400% of the federal poverty line (FPL). Since the subsidy falls sharply to zero for households above 400% FPL, there is a discontinuity in the subsidy structure at this threshold, which I call "*potential lost subsidy*"(PLS). The PLS is the subsidy a family would be eligible for if their annual income were *exactly* 400% FPL. Thus, *some* households would significantly benefit from earning just below 400%, rather than just above it. In this Chapter, I study whether households near the top cutoff and not offered ESI reduce their income and labor supply at both the intensive and extensive margins in response to the PLS.

I contribute to the literature in several ways. First, I focus on moderate- and middle- income households and the consequences of an unprecedented and understudied means-tested program for such income group – previous studies

in the field have thoroughly examined low-income households via the Medicaid expansion. In addition, I exploit the geographic variation in the cost of premiums and use premium data from all 50 states and the District of Columbia. Finally, I provide the first empirical estimates of the PLS affecting households near the top cutoff of the subsidy design.

In this work, I use data from the Medical Expenditure Panel Survey (MEPS) combined with premium data from the Robert Wood Johnson Foundation (RWJF) from 2010 to 2017. I compute the family premium cost for each household and construct their PLS and discontinuity had they earned exactly 400% FPL. I find that the PLS equals $100 a month for younger workers but reaches $400 to $600 a month for older workers on average. The data also confirm that the PLS greatly varies by geographic location and family composition. With OLS regressions and a probit model including interaction terms based on the PLS, I estimate the labor supply effects of the subsidy for households near the cutoff and not offered ESI. My identification tends to capture the labor supply impacts of the exogenous variation in the PLS from the household's perspective.

I find no statistical evidence that affected households by the subsidy discontinuity reduce their income, hours of work, or downgrade their employment status. Economically, the null effect of the PLS on household labor supply is a reasonably precise zero. Thus, I find that the ACA subsidy design does not create income manipulation or labor market distortion among moderate- and middle-income households. However, the uncertainty about the future of the ACA exacerbates the rise in premiums, and by consequence the increase in the PLS. As the

unemployment rate is skyrocketting due to the coronavirus economic crisis, the Marketplace can become a growing alternative to the traditional employer-based health insurance system. Thus, it is urgent to increase the affordability of health insurance plans as well as Marketplace awareness and outreach.

1.2 The Coverage Gap in Medicaid Non-Expansion States and its Labor Supply Effects

Two major provisions of the Affordable Care Act (ACA) in 2014 are (1) the advance premium tax credits (APTC) for households buying their own private health insurance, and (2) Medicaid expansion to non-elderly and non-pregnant individuals with an income up to 138% of the poverty line. However, some states decided not to expand Medicaid and a "coverage gap" emerged in 25 states as of January 2014. As a results, in Medicaid non-expansion states some households have an income too low to qualify for the APTC but too high for Medicaid eligibility. Both the ACA subsidy design and Medicaid non-expansion creates a new labor supply incentive for households to reach the poverty line and obtain a zero- or very-low cost health insurance on the Marketplace.

In Chapter 3, I study the impacts of the coverage gap on labor supply behavior within low-income adults. To do so, I examine whether households in non-expansion states who are just below the poverty line increase their labor supply and income to qualify for APTC for health insurance at the poverty line. This work contributes to the previous literature in several ways. First, I study the effects of a means-tested program that indirectly creates work incentives while all other studies on labor supply elasticity evaluate the disincentives to earnings of

such low-income transfer programs. Second, I study labor supply decisions within very poor households in non-expansion states often understudied and used as a control group in traditional Medicaid expansion studies. Finally, in this study, I exploit a triple exogenous variation from the perspective of households across income groups, states, and time.

With data from the Current Population Survey (CPS) and its Annual Socio-Economic Supplement (ASEC) from 2010 to 2018, I construct a short-panel of households earning between 70% and 150% FPL to examine changes in labor supply within households over two consecutive years. I use a difference-in-differences (DID) framework to estimate the coverage gap impacts on three measures of labor supply: (1) household income growth, (2) change in hours of work, and (3) change in employment status and labor force participation within households from one year to the other. To do so, I use three different estimation models. My first model is a DID for low-income households in never-expander states and compare differential changes in labor supply measures for very poor households to poor households just above the poverty line before and after 2014. Second, I use a DID comparing very poor households between non-expansion states (treatment) and expansion states (control) before and after the ACA. Finally, I use a DIDID to compare average changes in labor supply outcomes by income groups (below FPL vs above), states (expansion vs non-expansion), and time (pre-ACA vs post-ACA).

I find that very poor households do not adjust or manipulate their income to reach the APTC income-eligibility threshold as I find no significant effect of the

Medicaid non-expansion on income growth. However, I find that some households increase their labor supply in response to the upward discontinuity at the poverty line. In particular, childless adults increase significantly their usual weekly hours by 2 hours a week (estimates equal to 1.7 and 2.3 depending on the specification). Finally, the coverage gap does not affect the extensive margin of labor supply as I find no statistically significant effect in changes in employment status and labor force participation within very poor households.

Future research questions emerge as very poor people in non-expansion states are more likely to be uninsured and to face higher medical expenses than in expansion states. The uncompensated care generated by the uninsured is a crucial question that future research may want to study. Also, as the coronavirus pandemic starts to reveal its considerable economic consequences, a recent report from Kaiser Family Foundation (2020) estimates that up to 1.9 million of low-income childless adults would join the coverage gap by January 2021 as their unemployment benefits expire and the decline in income would make them too poor to qualify for APTC. Thus, the importance of closing the Medicaid coverage gap urgently re-appears with the current health crisis.

1.3 A Reassessment of the Effect of Medical Marijuana Law on Opioid Misuse in light of Medicaid Expansion

Drug overdose is the leading cause of deaths from injuries in the United States, e.g., in 2018, 10.3 million people have misused prescription opioids, or pain medications (SAMHSA, 2019). The current opioid epidemic in the U.S. has generated a large debate about the causes and consequences of opioid misuse and

abuse which goes beyond the scope of this dissertation. However, I contribute to the literature in this topic by revisiting a recent study by Powell et al. (2018) on the effects of medical marijuana laws on opioid addiction and mortality from 1999 to 2013 in two ways.

First, I replicate their study by controlling for early Medicaid expansion which started in 2010 in five "early-expander" states and the District of Columbia (DC), representing 20% of the U.S. population. I find that medical marijuana dispensaries rather than medical marijuana laws per se are associated with a reduction in opioid-related mortality confirming Powell et al. (2018)'s findings. I find that early Medicaid expansion, a potentially confounding variable, does not affect the scale of Powell et al. (2018)'s results; however, early Medicaid expansion significantly reduces opioid mortality death rates by 25% before 2014.

Second, I extend the analysis to 2018 and provide, to the best of my knowledge, the most recent long-term effects of medical marijuana dispensaries and Medicaid expansion on opioid mortality. Medical marijuana dispensaries persist in reducing opioid-related death rates by 2018 and Medicaid expansion is not related to opioid-related mortality over my entire study period implying that such policy does not exacerbate the opioid crisis.

I construct a panel dataset from 1999 to 2018 for all 50 states and the District of Columbia (DC) with information on opioid-related mortality rates, medical marijuana laws, and Medicaid expansion. To measure opioid-related mortality rates, I use data from the 1999 to 2018 multiple cause-of-death mortality (MCOD) files from the National Vital Statistics System (NVSS), available

at the Wonder online database of the Centers for Disease Control and Prevention (CDC). I obtain the total death rates involving opioids. Data about state medical marijuana laws and active medical dispensaries are coming from three main sources: (1) the Prescription Drug Abuse Policy System (PDAPS); (2) original search on webpages from marijuana advocacy groups; and (3) state agency websites responsible for regulating these laws.

By 2018, 28 states have passed medical marijuana laws from which 24 states have opened medical marijuana dispensaries. Additionally,Medicaid expansion has been adopted by 33 states providing Medicaid coverage to all non-elderly adults earning up to 138% of the federal poverty line (FPL). In Chapter 4, I use a difference-in-differences method to estimate the differential changes in opioid mortality rates between states passing medical marijuana laws to those not adopting these laws. I use population-weighted regressions, and control for Medicaid expansion and various other state-year characteristics affecting opioids misuse that I independently collect (prescription drug monitoring program, unemployment rate, beer alcohol tax, population estimates, etc.)

I confirm that medical marijuana laws do not significantly reduce opioid-related death rates by 2013. Also, controlling for early Medicaid expansion does not affect the magnitude of Powell et al. (2018)'s results. However, it allows me to find that Medicaid expansion is associated with a 25% decrease in opioid mortality rate from 1999 to 2013. Second, I confirm that active medical dispensaries are associated with a decline in opioid overdose deaths until 2013. Second, active medical dispensaries persist in reducing opioid overdose death rates by 33% by

2018. Third, there is no evidence that Medicaid expansion is associated with higher opioid overdose death rates by 2018.

With the rise in opioid-related mortality, more states are passing medical marijuana laws. Therefore, a reverse causality problem might emerge making it difficult to infer the causal effects of such laws on opioid harm reduction. Additionally, the endogeneity of states deciding to pass such laws may bring some doubts about the validity of standard difference-in-differences models to study opioid misuse and abuse. Future research may want to either look for instrument variables predicting the passage of such laws independently of opioid misuse level or exploit an exogenous variation in the policies if any.

Chapter 2

THE EFFECTS OF THE AFFORDABLE CARE ACT ADVANCE PREMIUM TAX CREDITS ON HOUSEHOLD LABOR SUPPLY

2.1 Introduction

One of the most important provisions of the Affordable Care Act (ACA) is the advance premium tax credits (APTC, hereafter also referred to as the "subsidy") to lower the cost of individual health insurance plans sold on state-based exchanges.[1] By design, only households who are not offered employer-sponsored insurance (ESI) and who earn between 100 percent and 400 percent of the federal poverty line (FPL) are eligible for the subsidy (i.e., between $12,140 and $48,500 in 2019 for a single individual or between $25,100 and $100,400 for a family of four with two adults and two children). The subsidy is structured so that the out-of-pocket premium for the benchmark insurance plan of the local area is capped to a specific percentage of the household's annual income. Since the subsidy falls sharply to zero for households above 400 percent of the FPL, there is a discontinuity in the subsidy structure at this threshold, which I call "*potential lost subsidy*"(PLS). The PLS is defined as the subsidy a family would be eligible for if their annual income were *exactly* 400 percent of the poverty line

[1] I provide more detail about the ACA subsidy design in section 2.3. However, a complete summary of the ACA is available at https://www.kff.org/health-reform/fact-sheet/summary-of-the-affordable-care-act/

– with a dollar more of income the family would *lose* this subsidy. Thus, *some* households would significantly benefit from earning just below 400 percent, rather than just above it.

Using the Medical Expenditure Panel Survey (MEPS) with premium data from the Robert Wood Johnson Foundation (RWJF) from 2010 to 2017, I study whether households between 300 and 500 percent of the FPL who are not offered ESI after 2014 reduce their income and labor supply due to a large PLS. To do so, I construct their PLS by computing the family premium cost for each household and their discontinuity had they earned exactly 400 percent of the FPL. I find that the PLS equals $100 per month for younger workers but reaches $400 to $600 per month for older workers on average. The data also confirm that the PLS greatly varies by geographic location and family composition. I use standard OLS regressions and a probit model with several interaction terms based on the PLS to estimate the labor supply effects of the subsidy for households near the cutoff and not offered ESI. I find no evidence that such households reduce their income or hours of work due to a larger PLS. The probability that one of the adults stops working increases by 1 percentage point as the PLS increases by $100 per month; however, the coefficient estimate is not statistically different from zero. Using a large definition for being near the cutoff may actually attenuate the results towards zero since households at both extremes are unlikely to try adjusting their income. –¿ while I find no statistically significant effect that such households reduce their income or labor supply, the economic interpretation of the coefficient estimate suggest that I cannot rule out a negative effect of the PLS on labor

supply. In fact,

Several prior studies have examined the effects of various provisions of the ACA on household labor supply. Most of the prior work has focused on low-income households, mainly studying the effects of Medicaid expansion on employment or earnings (Baicker et al., 2014; Garthwaite et al., 2014; Dague et al., 2017). To date, little attention has been paid to the effects of the ACA premium tax credits. Shi (2016) estimates the effects of the subsidy for households in Massachusetts and their income responses; Finkelstein et al. (2017) analyze the effects of the ACA subsidy on coverage enrollment for the low income in Massachusetts; and more recently, Kucko et al. (2018) use a bunching analysis to study the impacts of the ACA premium tax credits in states that did not expand Medicaid. However, these previous studies on the ACA subsidy mainly focus on low-income individuals.

In contrast, I focus on moderate- and middle- income households around 400 percent of the FPL and try to provide a better understanding of the consequences of an unprecedented means-tested program for such income group. Rather than focusing on a single state, I exploit the geographic variation in the cost of premiums and use premium data from all fifty states and the District of Columbia. The variation in income net of out-of-pocket premiums generates a quite large PLS for *some* but not all households. The goal of this paper is to examine the labor supply effects of such discontinuity and to investigate whether there are unintended consequences from the ACA subsidy design at a time of major debate about the health care system.

The rest of this Chapter is organized as follows. In Section 2.2, I present

a review of the literature related to the ACA and the premium tax credits. In Section 2.3, I explain the ACA subsidy design at the foundation of my analysis. In Sections 2.4 and 2.5, I describe the data and explain the method I use to study the labor supply of households near the subsidy cutoff, respectively. Finally, in Sections 2.6 and 2.7, I present and discuss the results, respectively, before concluding in Section 2.8 with future research directions.

2.2 Literature Review

In this paper, I focus on the effects of the income-conditional ACA premium tax credits on labor supply. There are several reasons why means-tested programs would generally reduce labor supply and among them, the fear of losing health insurance which results in creating a situation of "employment lock" in which workers are primarily employed to receive health insurance coverage (Garthwaite et al., 2014; Madrian, 1994). Also, health insurance may reduce labor force participation and income for the self-employed (Chetty and Saez (2010)). More theoretically, Moffitt (2002) presents graphical representations of the labor supply responses to welfare programs, illustrating how working hours and income negatively evolve once the Medicaid program is in place. He further argues that the effects of welfare programs on labor supply are understudied and need more attention.

In 2010, the Patient Protection and Affordable Care Act (ACA) implemented several provisions to improve the affordability of health insurance and to lower the rate of uninsured people. This generated substantial research about the effects of the ACA on various topics such as insurance take-up, employment and

other labor market outcomes, to name a few. Akosa Antwi et al. (2013) estimate the effects of the ACA dependent-coverage mandate, which allows adults under 26 to stay insured under their parents' coverage. Along with finding a large decrease in the uninsured rate for young adults, they find a reduction of 3 percent in hours of work for these newly insured individuals. Garthwaite et al. (2014) investigate the 2005 health insurance disenrollment in Tennessee and its effects on labor force participation. They find that the loss of public health insurance generates a 6 percent increase in employment for low-income childless adults. The authors suggest that the ACA Medicaid expansion may lead to a large decrease in labor supply for this income group. However, other studies such as Dague et al. (2017) raise some concerns about the external validity of such results and the interpretation of the Tennessee study. In particular, they mention that the group of individuals studied by Garthwaite et al. (2014) might have been richer and more educated than the national average and may not be nationally representative. Moreover, they point out that losing health insurance may have different consequences than gaining it. Thus, the Tennessee results should not prevent further research in this area.

Overall, the literature is still quite ambiguous about the effects of health insurance on labor supply. Yelowitz (1995) finds a positive impact of health insurance expansion on labor supply. However, several studies find little or no significant change in employment and earnings due to an increase in public health insurance by studying either the impact of Medicaid expansion or the Oregon experiment (Finkelstein et al., 2012; Baicker et al., 2014; Gooptu et al., 2016;

Leung and Mas, 2016; Kaestner et al., 2017; Peng et al., 2018). However, Duggan et al. (2017) find that labor force participation increases in areas where numerous people are potentially Medicaid-eligible and decreases in areas where individuals are more likely to enroll in the Exchange.

Closer to my work, Shi (2016) studies the impact of the health insurance subsidy on income using the 2006 Massachusetts healthcare reform. The author finds evidence of income manipulation for self-employed individuals around 150% FPL and for workers around 300% FPL using a regression discontinuity approach. Also, Kucko et al. (2018) study the influence of the ACA premium tax credits on low-income individuals near 100% FPL in states that did not expand Medicaid. Using tax data and the American Community Survey (ACS), they find no evidence for a change in labor market outcomes and earnings in general, but a significant bunching or excess mass occurs for the self-employed around 100% FPL. According to the authors, this may reflect a change in the reported income rather than a true change in labor supply.

Despite a large body of literature on the effects of the ACA and health insurance on coverage and labor outcomes, there is a gap in knowledge about the effects of the premium credits on labor supply for the moderate- and middle-income households. Therefore, the goal of this paper is to calculate the discontinuity in the subsidy for such population and its effects on labor outcomes such as income growth, changes in working hours and employment status. Beyond previous research, I use the geographic variation in the cost of premiums and quantify the potential lost subsidy (PLS) of households near the top threshold.

2.3 Background

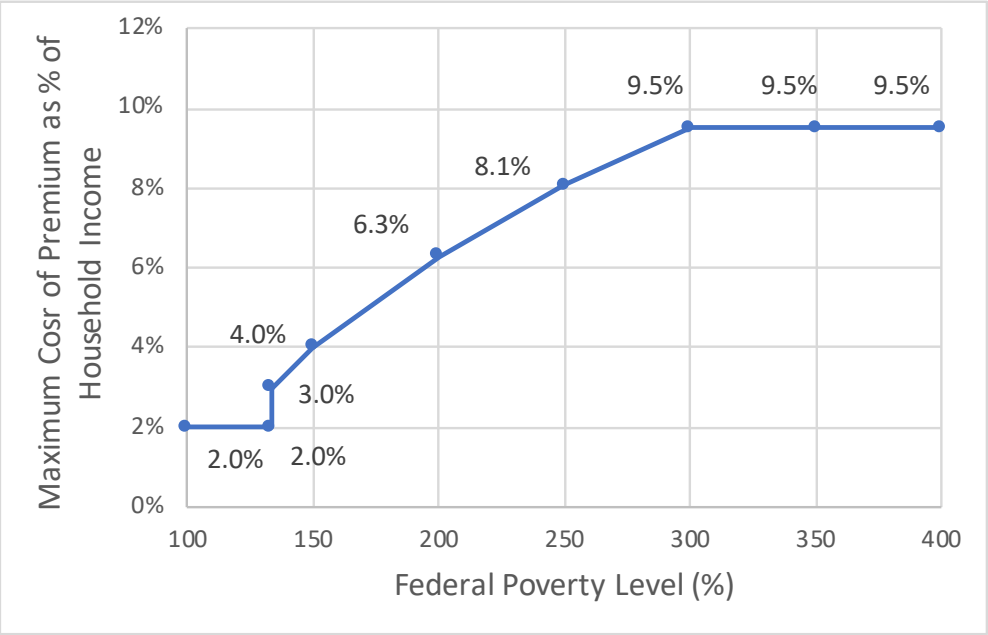
Since 2014 and the implementation of the ACA, households who are not offered affordable employer-sponsored health insurance (ESI)[2] and have an annual income between 100% and 400% FPL can qualify for advance premium tax credits (APTC, or the "subsidy") to reduce their out-of-pocket premiums for medical insurance sold on the Exchange. In 2015, around 11 million people purchased their own private health insurance on a marketplace; among them, more than 8 million individuals enrolled via Healthcare.gov, the Federally-Facilitated Exchange (FFE) platform, and almost 3 million people enrolled through State-Based Marketplaces (SBM). Overall, 7 million individuals were eligible for the subsidy (ASPE, 2016).

By design, the ACA subsidy depends on (1) the cost of the household's benchmark plan – the second-lowest cost Silver plan in its area of residence – and (2) the household's annual income. On the marketplace, premiums depends on the county of residence, family size, and age of each family member.[3] The amount of the subsidy varies by the family's annual income as a percentage of the FPL for the size of the household, and drops sharply to zero above 400% FPL. For subsidy-eligible households, annual expenses for health insurance are capped and vary by income. The required income contribution for the benchmark premium is shown in Figure 2.1.

[2] In 2014, an ESI plan must cost less than 9.5% of household Modified Adjusted Gross Income (MAGI) to be affordable.

[3] More precisely, premiums depends on the rating area, for which county is a near perfect proxy.

Figure 2.1: Cap on Required Premium for Subsidy-Eligible Individuals in 2014



In 2014, subsidy-eligible families near the poverty line must contribute 2-3% of their income for their annual premium while moderate income families (300-400% FPL) must pay 9.50% of their income.[4] The subsidy for each household is the difference between the cost of their benchmark plan and their required premium contribution, bringing down the benchmark premium to the required cap. The subsidy can be applied to any exchange metal plan even though its size depends on the Silver plan. With data on the benchmark premium in each U.S. county, I construct the cost of a family benchmark plan as the sum of premiums for each family member. To compute the family premium, no more than the cost of three children under the age of 21 can be included.

[4] I use the year-specific IRS percentages in my calculations even though they have hardly changed since 2014.

Table 2.1: Monthly Calculation of a Family's Subsidy in New Castle County, DE (2014)

	(A)	(B)	(C) = (9.5% x B)	(A) - (C)
HH Composition	**Full Premium**	**Monthly Income at 400% FPL**	**Required Premium at 400% FPL**	**PLS**
(27)	$237	$3,830	$364	$0
(48)	$370	$3,830	$364	$6
(48, 50)	$774	$5,170	$491	$283
(35) + 2C	$564	$6,510	$618	$0
(38,40) + 2C	$858	$7,850	$746	$112
(48,50) + 2C	$1,061	$7,850	$746	$315
(52,55) + 2C	$1,234	$7,850	$746	$488

Notes: The full premium is the cost of the unsubsidized benchmark plan per month for a non-smoker in New Castle County (DE) as listed by the 2014 Kaiser Family Foundation (KFF) Calculator.

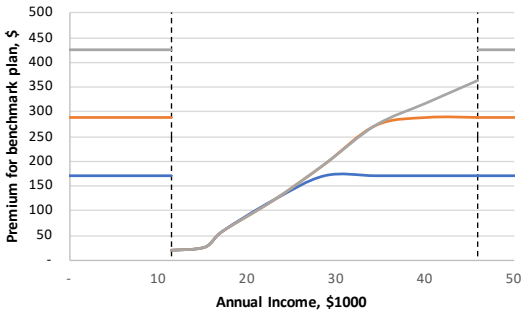
Table 2.1 shows how to calculate the subsidy and *potential lost subsidy* (PLS) per month using examples of various family sizes and age profiles for households living in New Castle County, Delaware in 2014. The PLS is equivalent to the subsidy at 400% FPL or the difference between the full (unsubsidized) benchmark premium and the required premium at that exact threshold. A 27-year-old adult earning exactly 400% of the FPL in 2014 would not receive any subsidy, as the cost of the benchmark plan is below his annual required contribution for premiums; even a 48-year-old single individual would receive only a $6 subsidy per month, as his benchmark premium barely exceeds his required contribution. However, a couple in their late 40s with no child would receive a subsidy of $283 per month: their monthly benchmark plan costs $774, much higher than their required premium of $491 per month. Thus, the PLS for a couple in their late 40s is equal to 6% of their income (or $3,394 a year), but is trivial or even zero for younger singles. Finally, for a family of four (two adults and two children),

the full premium is $1,234 but the subsidy at exactly 400% FPL would be $488 per month representing a PLS of 7% of their income. In general, the subsidy and PLS greatly vary by age and family size.

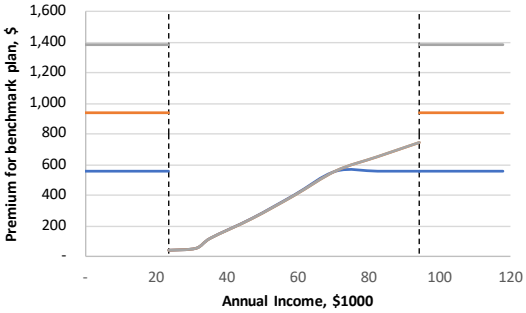
The APTC structure generates important variation in income net of out-of-pocket (OOP) premiums across households as household demographic characteristics have different impacts on premiums and lost subsidy. In fact, households expecting to earn around 400% FPL may lose a significant insurance subsidy if their income even slightly exceeds the top threshold. Thus, *some* households might reduce their labor supply to prevent their income from exceeding the eligibility threshold, as they would have a greater income net of medical insurance premium. The incentive to reduce labor supply depends on the size of the PLS, which varies both geographically and with household structure.

Premiums in rural areas in the Midwest and Great Plains states tend to be higher than in metropolitan areas along the east and west coasts. In Figure 2.2, I show the geographic variation in the PLS by plotting the cost of the 2014 benchmark plan for various family structures not offered ESI and living in three areas: Fayette County (PA) (low price), New Castle County (DE) (mid price) and Hot Springs (WY) (high price). Outside the eligibility range for APTC, the benchmark premium is unsubsidized while inside the thresholds the benchmark premium takes into account the appropriate subsidy. Panel (a) shows that the cost of premiums for a 40-year-old adult is similar around the top cutoff if they live in Fayette County (PA) or in New Castle County (DE), premiums being $170 or $290 per month on both sides of the cutoff, respectively. In these rating areas,

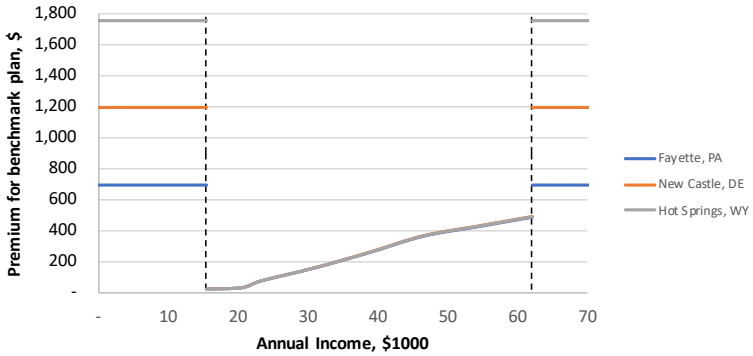
Figure 2.2: Variation of Monthly Premiums by Geographic Location, Family Structure and Annual Income (2014)



(a) Premiums for a 40-year-old Single



(b) Premiums for a Family of Four



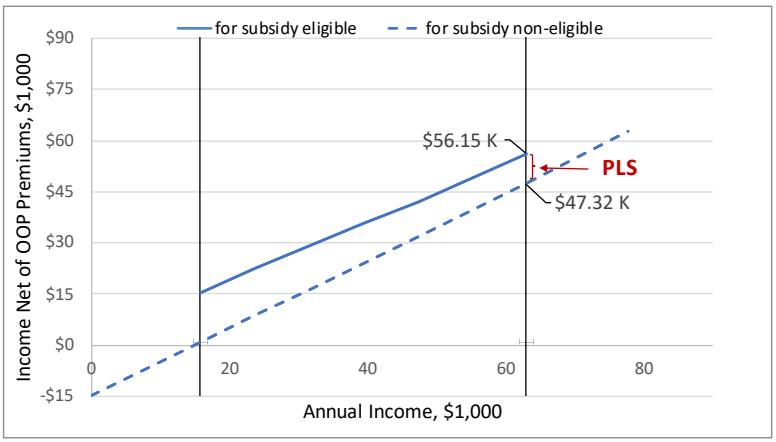
(c) Premiums for an Older Couple

Notes: Premiums are from the 2014 Kaiser Family Foundation (KFF) Calculator for non-smoker individuals living in Fayette County, Pennsylvania, in New Castle County, Delaware or in Hot Springs County, Wyoming. Here, older couple is a 58- and 60- year old couple; a family of four is a 45-year-old couple with 2 children under 21. The vertical dashed lines represent the 100% and 400% FPL subsidy cutoffs for the corresponding household. In the subsidy-eligible range, the premium cost is the subsidized premium while outside this range it is the full (unsubsidized) premium paid by households in their local rating area.

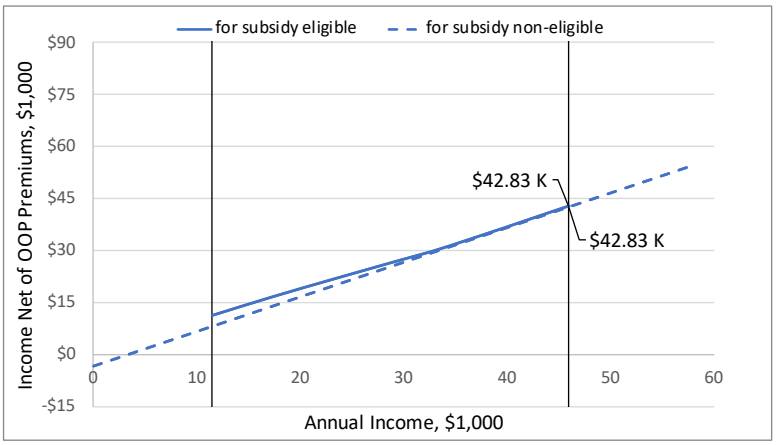
such individuals are not facing any PLS or discontinuity in the cost of premium had they earn just above the eligibility threshold. However, similar individuals living in Hot Springs (WY) would have to pay $62 more per month (or 1% of their income) for health insurance had they earned just above 400% FPL. Overall, single adults are not highly affected by the PLS regardless of their rating area even if there is some variation. However, panels (b) and (c) describe a very different situation for families with two or more children and older couples. Their monthly premiums are almost the same across the three regions and equal around $745 for a family of four and $490 for older couples when earning just *below* the top eligibility threshold. However, these households face a large discontinuity in their final out-of-pocket premiums when earning just *above* 400% FPL. A family of four would not face any PLS in Fayette but would have to pay $195 more per month in New Castle (2% of their income) and $640 more per month in Hot Springs (8% of their income). Finally, older couples in their late 50s pay $208 more per month (4% of their income) in Fayette, $700 more (13% of their income) in New Castle and $1,263 more in Hot Springs (24% of their income). Thus, there is a large geographic variation in the PLS for such types of households.

Aside from the geographic variation, the PLS also greatly varies by household composition. In Figure 2.3, I present the PLS in 2014 for a 62-year-old couple (Panel 2.3a) and a 35-year-old single individual (Panel 2.3b) by plotting their income net of OOP premiums as a function of annual income for subsidy-eligible and non-eligible families, assuming they purchase the benchmark plan.

Figure 2.3: Income Net of Out-Of-Pocket Premiums (2014, U.S. average)



(a) For a 62-year-old couple



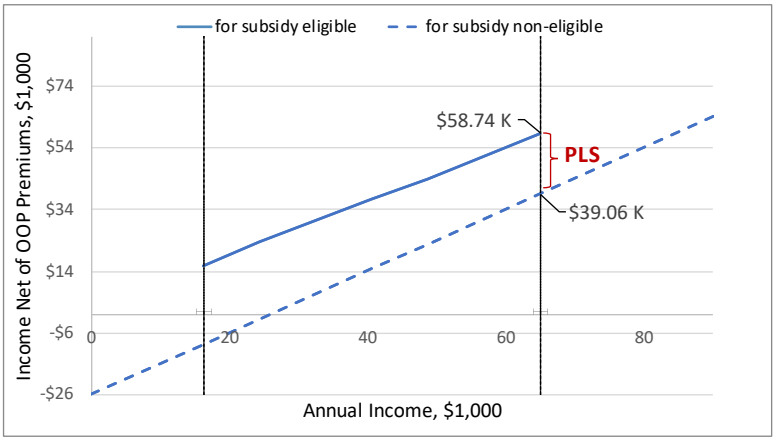
(b) For a 35-year-old individual

Notes: The vertical lines represent the 100% and 400% of the FPL for a couple (panel a) or a single individual (panel b) using the 2013 FPL guidelines. The income net of out-of-pocket premiums refers to the annual income minus the subsidized premium (for subsidy-eligible) or minus the full premium (for non-eligible). Premium data are obtained from the Kaiser Family Foundation calculator for 2014 for non-tobacco users.

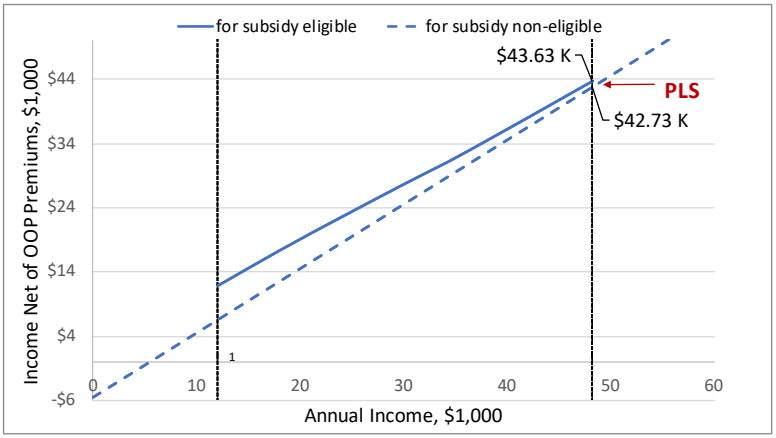
For subsidy-eligible households, the income net of OOP premiums is their annual income minus the (annual) subsidized benchmark premium; however, for non-eligible households, it is equal to their annual income minus the full, unsubsidized benchmark premium. In this context, non-eligible households correspond to families that buy their health insurance on the Marketplace but with an income outside the eligibility range, or families that buy the same policy off the Exchange for some reason, or those ineligible for APTC for other reasons. The difference between the solid and the dashed line represents the U.S. average subsidy received in the eligibility income range. At the 400% FPL cutoff, this difference is the PLS – the dollar amount of subsidy that eligible-households would lose by earning just above the threshold. For example, in 2014, a 62-year-old couple earning 400% FPL would have an annual net income of $56,150 after purchasing health insurance on the Marketplace with the subsidy, whereas their net income would be only $47,320 without the subsidy. Thus, they would have lost around $9,000 a year had they earned just above the eligibility threshold (and continued to purchase this insurance). However, for a 35-year-old adult the PLS is equal to zero as the OOP premium is the same for subsidy eligible and non-eligible around the 400% FPL cutoff. As Marketplace premiums have been rising since 2014, the PLS has become larger. Figure 2.4 shows that in 2018, the PLS for a young adult averaged around $1,000 a year and almost $20,000 a year for an older couple.

Consequently, some households that anticipate earning about 400% FPL would be better off by earning slightly less (mainly older couples or families with children); they might choose to reduce their income and labor supply so as not to

Figure 2.4: Income Net of Out-Of-Pocket Premiums (2018, U.S. average)



(a) For a 62-year-old couple



(b) For a 35-year-old individual

Notes: The vertical lines represent the 100% and 400% of the FPL for a couple (panel a) or a single individual (panel b) using the 2017 FPL guidelines. The income net of out-of-pocket premiums refers to the annual income minus the subsidized premium (for subsidy-eligible) or minus the full premium (for non-eligible). Premium data are obtained from the Kaiser Family Foundation calculator for 2018 for non-tobacco users.

risk losing their large APTC. Therefore, the goal of this paper is to evaluate such potential behavior since the ACA marketplaces and subsidies were implemented in 2014, and to examine whether some unintended market distortions or income manipulation are generated by a means-tested program like the premium tax credits.

2.4 Data

In this paper, I use the Medical Expenditure Panel Survey and the Household Component (MEPS-HC) files from 2010 to 2017 combined with premium data from the Robert Wood Johnson Foundation (RWJF).

The longitudinal data from the MEPS provide detailed information on health insurance coverage, medical expenses, and demographic and socioeconomic characteristics of around 17,000 households surveyed over a two-year panel by the Agency for Healthcare Research and Quality (AHRQ). The survey design consists of five rounds of in-person interviews in which information is reported for each family member during two consecutive years.[5] By combining or stacking the two-year panel data from 2010 to 2017, I obtain a final repeated cross-section of 33,412 households (equivalent to 94,409 individuals) with a non-negative income for which the head of household is between 21 and 64 years old.[6] Additionally, the MEPS data are a subsample of households from the National Health Interview Survey (NHIS) from the previous year, which is a nationally representative sample

[5] For simplicity, I based my demographic analysis using information collected in the first round of interviews of each year of survey. At each round, the respondent is asked the same questions.

[6] Individuals 65 and older are very likely to be eligible for Medicare.

of the U.S. population with an oversampling of Blacks and Hispanics. Since I work with survey data at the family level rather than a simple random sample, I use the family weight provided by the MEPS to generate my estimates.[7]

In this study, I also use premium data from the Robert Wood Johnson Foundation (RWJF), particularly the HIX Compare data files from 2014 to 2017; these files gather the premiums for every insurance plan sold on the Exchanges for various family structure and age profiles. The HIX Compare dataset is the most complete dataset I find for this research as it collects monthly premiums in each county for all 50 states and D.C. since 2014. For states using the Federally Facilitated Marketplace (FFM), premium data were obtained from Healthcare.gov; for states using their own State-Based-Marketplace (SBM), premiums were collected from individual state marketplace websites. However, the RWJF data for the 2014 premiums are missing for the 17 states (including D.C) that use their own SBM. To the best of my knowledge, this information had never been aggregated for 2014. Therefore, I collect the missing monthly premiums by rating area from each state exchange website to construct a unique dataset of premiums covering all U.S. states at the county level since the implementation of the ACA. Finally, I merge the complete premium dataset onto the post-ACA MEPS datasets (2014-2017) by county FIPS code to precisely compute the benchmark plan cost for households in their specific place of residence.[8] For the pre-ACA period, I merge

[7] Standard errors are adjusted to account for the multi-stage sampling design of the MEPS.

[8] MEPS data at the state and county level are restricted information for reasons of confidentiality. Thus, I performed the merge and subsequent analysis at the AHRQ Data Center in Rockville, MD, where the final dataset is now stored and accessible upon request.

the 2014 premiums data onto the pre-ACA MEPS panels and use these premiums as a placebo: the PLS is nonexistent and should not have any effect prior to 2014.

Using my master dataset of 7 panels since 2010 along with the ACA age-rating function[9] described in Table A.1 of the Appendix, I calculate the cost of the benchmark plan and the PLS that every household would potentially face if they were purchasing their own private health insurance on the Marketplace. As shown in the background section, premiums greatly vary across and within states, which is why detailed premium data for all U.S. states and counties are crucial to provide complete insights on whether the PLS affects household's labor supply. In this way, this paper tries to capture an important feature of the ACA subsidy design and fills a gap in the literature regarding the impact of the premium tax credits on income growth, change in hours of work and employment status.

In my analysis, I use the health insurance family unit definition proposed by State Health Access Data Assistance Center (SHADAC) (2012) to compute family income as a percentage of the FPL and create one of my main variables. I define households not offered ESI using the variable from the MEPS HC asking whether individuals have been offered health insurance through their current main job. Overall, 7,572 households are not offered ESI from 2010 to 2017. I only keep the reference person record for each household along with socioeconomic information on their children, spouse and other adults in the household, so that my unit of analysis is a household-year pair.[10] Table 2.2 shows the descriptive

[9] The age rating function links the premium of a 21-year-old to the premium for an individual of any age.

[10] I also use the health insurance information of the spouse to make sure that if the spouse is

statistics of households with an annual income near the top cutoff in pre- and post-ACA panels. In the 2011-2012 and 2014-2015 panels, I have 5,227 and 4,677 households respectively, for which the reference person is between 21 and 64 years old. Among them, around 20% of the sample is near the top cutoff (300-500% FPL) during these panels. The mean statistics are very close to each other across panels, but some differences appear. The income growth is 3.4% for households just below the cutoff in pre-ACA period and 1.7% in post-ACA period while for households just above it, income growth is 1.9% and 3.7%, respectively. The usual hours of work are between 34 to 37 hours per week and stay relatively constant across panels for the two income groups. The percentage of employed heads of household also stays constant to around 87% over the panels for both income groups and 7 to 10% of the sample is self-employed.[11] Households near 400% FPL are mainly single adults (39%), couples with no child (15%) or family of four (14%) and with a head of household between 42 to 45 years old and mostly white, on average. Over the panels, most households (86% to 94%) have a private health insurance (through either their employer or the Marketplace in post-ACA). As expected by the level of income, the percentage of households with public health insurance is low and around 1 to 4%. Also, the rate of uninsured families between 401% and 500% FPL dropped from 6% to 3% from pre- to post-ACA panels. However, almost 10% of households are still uninsured with a subsidy-eligible income just below 400% FPL in post-ACA period. Lastly, total

offered ESI then the reference person as well. However, no adjustments were needed.

[11] A head of household is considered employed if the individual has a full-time job or a job to return during the round of interview.

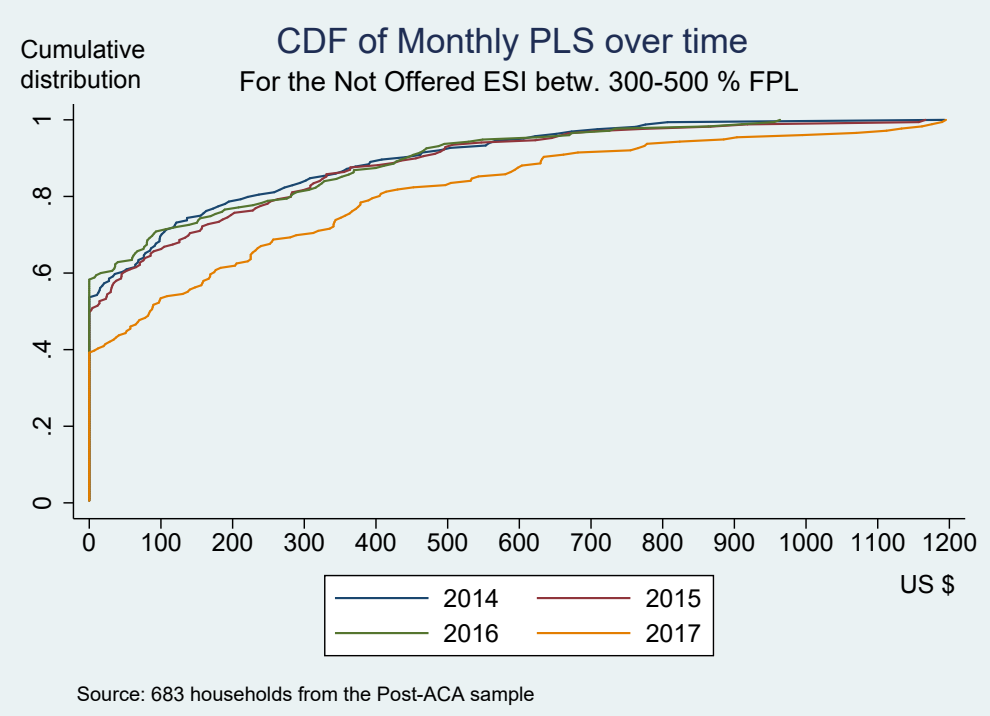
Table 2.2: Mean characteristics of households near the 400% FPL subsidy threshold in a pre-ACA (2011-2012) and post-ACA panel (2014-2015)

	2011-2012		2014-2015	
Income as % FPL	***300 - 400***	***401 - 500***	***300 - 400***	***401 - 500***
Sample (# of HH)	**717**	**462**	**579**	**407**
% of my sample	14%	9%	12%	9%
Estimated Pop. (million)	7.7	5.3	5.7	4.9
At household level:				
Annual Income	$ 58,139	$ 76,068	$ 61,998	$ 78,840
Income growth	3.4%	1.9%	1.7%	3.7%
Employment characteristics				
Hours of work	35	35	34	37
Employed	85%	87%	86%	89%
Self-employed	7%	10%	6%	6%
Demographics				
Family Structure				
single: with no child	40%	33%	39%	33%
with 1 child	7%	4%	4%	4%
with 2 children	2%	2%	2%	1%
with 3 children or +	0%	0%	0%	0%
couple: with no child	17%	20%	15%	22%
with 1 child	7%	11%	11%	14%
with 2 children	12%	16%	14%	13%
with 3 children or +	7%	4%	6%	3%
3 adults or +	8%	11%	9%	9%
Age of Household head	42.5	45	42.8	44
White	82%	82%	81%	82%
Black	12%	11%	12%	10%
Other (Asian, Hispanic etc.)	6%	7%	7%	8%
Health status (Self reported)				
Excellent	30%	31%	27%	23%
Very good	35%	36%	38%	41%
Good	26%	23%	24%	24%
Fair	8%	9%	8%	11%
Poor	2%	2%	2%	1%
Insurance Coverage				
Any private insurance	87%	93%	86%	94%
Public insurance only	3%	1%	4%	3%
Uninsured	10%	6%	9%	3%
Not offered ESI	11%	10%	16%	11%
Tot. ann. healthcare expenses	$ 4,085	$ 3,721	$ 4,113	$ 3,670

medical expenditures are on average around $4,000 a year in both panels.

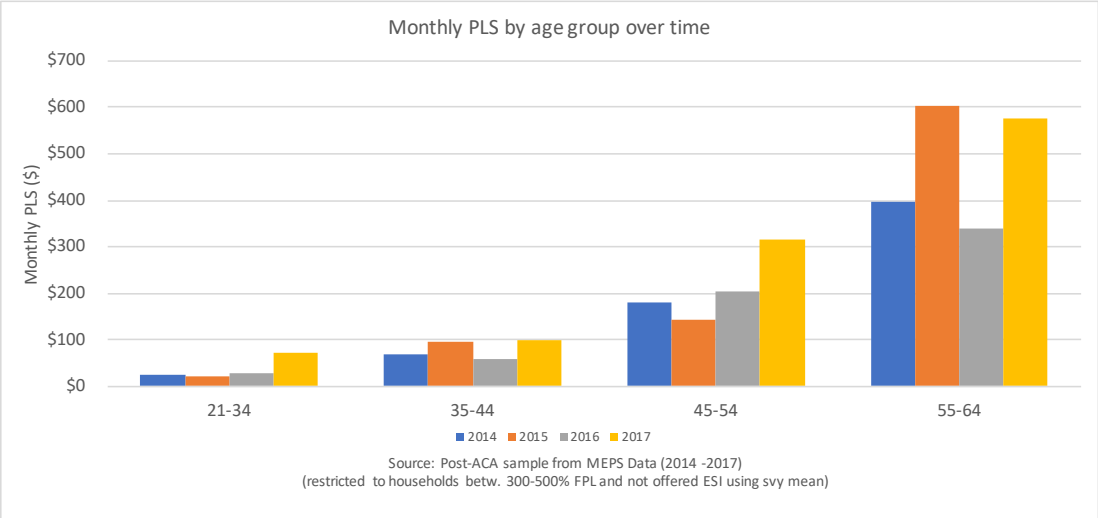
Figure 2.5 shows the cumulative distribution of the PLS over time since the ACA premium tax credits implementation. Around 40% to 60% of the households near 400% FPL and not offered ESI have a zero PLS from 2014 to 2016. However, there is a clear shift to the right in 2017 in the distribution of the PLS as a consequence of much higher PLS and premiums. In fact, the marketplace average benchmark premiums increased by 16%, from $299 to $349, from 2016 to 2017.

Figure 2.5: Distribution of the PLS (2014-2017)



Finally, Figure 2.6 illustrates the evolution of the monthly PLS since 2014 by age group for the relevant households. They represent 685 households in my dataset. For households with a reference person between 45 and 54 years old, the

Figure 2.6: PLS per month by age group over time (2014-2017)



PLS is around $200 per month, twice the PLS faced by younger households (aged 21-34 or aged 35-44), and rises over time. For older households with a head aged 55 to 64, the PLS is around $400 per month with a peak to almost $600 in 2015 and 2017 – years in which premiums rose significantly.

2.5 Method

The goal of this paper is to study the effects of the ACA subsidy discontinuity or tax notch on labor supply. Kleven and Waseem (2013) show that large notches generate strong incentives for excess mass below cutoffs and missing mass above cutoffs; however, agents face adjustment cost or other type of frictions that tend to make them unresponsive to tax incentive (e.g., hours constraints). The authors offer an important framework to better study notch using a bunching analysis. However, such approach requires very large and often restricted datasets

such as administrative data on tax returns like in Kucko et al. (2018). In this paper, I instead use survey data from the MEPS in which taxable income is self-reported and cross-checked with the employment section survey. While survey data may introduce some measurement error in the income variable, it will be uncorrelated with the implementation of the subsidies. In addition, the MEPS survey is primarily focused on health information rather than income compared to tax returns data from the IRS. As such, motivations to lie about household annual income is quite unlikely in the MEPS data while it may be questionable when using IRS data.

Given that the 400% FPL cutoff clearly defined the eligibility for the ACA subsidy, a regression discontinuity (RD) design seems appropriate at first. However, a major doubt emerges about the strength of this approach in the context of my study. Income volatility has been rising over the years (Dynan et al., 2012) and large year-on-year income fluctuations are common among American households.[12] Income volatility prevents households from accurately predicting their subsidy eligibility based on their projected income for the upcoming tax year. Thus, households are not sure in advance where they stand toward the subsidy cutoff which makes the cutoff unclear from their perspective. Thus, households are not sure in advance and even during the ongoing coverage year where their income will stand toward the subsidy cutoff at the tax season. This makes the cutoff unclear from the households' perspective and generates some ambiguity

[12] According to a recent study from the Pew Research Center (2017), more than a third of households experienced an income change larger than 25% from 2014 to 2015.

about the strength and use of a RD in this specific context.

Instead, in this paper, I use a linear regression model with interaction terms capturing the relevant households affected by the PLS. There is a plausible exogenous variation in the PLS from the household's perspective as the amount of the discontinuity created in 2014 depends on household size, age of each member, and geographic location. Once controlling for households being around the cutoff, households do not adjust the composition of their family or place of residence due to the ACA premium tax credit. With cross-sectional data on income and labor supply changes from 2010 to 2017, several regressions can be used to study the labor supply impacts of the PLS depending on the control group chosen. First, I use the post-ACA sample (year-on-year changes starting from 2013 to 2016) to estimate the impact of an increase in household's PLS on the change in labor supply for households near the cutoff and not offered ESI using the following regression:

$$\begin{aligned} Y_{it} = {} & \beta_0 \mathbf{X}_{it} + \beta_1 NearCutoff_i + \beta_2 NotOfferedESI_i + \beta_3 PLS_{it} \\ & + \beta_4 NearCutoff_i \times NotOfferedESI_i + \beta_5 NearCutoff_i \times PLS_{it} \\ & + \beta_6 NotOfferedESI_i \times PLS_{it} \\ & + \beta_7 NearCutoff_i \times NotOfferedESI_i \times PLS_{it} + \epsilon_{it}, \end{aligned} \tag{2.1}$$

where Y_{it} is measuring the year-on-year percent changes in income for household i, defined as the difference in log annual income over two consecutive years of survey. In a second specification, Y_{it} represents the year-on-year changes in the (usual) weekly hours of work within household i. $NearCutoff_i$ and $NotOfferedESI_i$

are indicators for whether households have in the first year of interview an annual income between 300% - 500% FPL and are being offered ESI at their current main job. Households just *above* the cutoff are clearly those with the largest incentive to reduce their income. However, while households earning just *below* 400% are already income-eligible, I include them among the "treated" group as they may also try to reduce their income due to the PLS – the larger their PLS, the smaller their incentive to increase their income. The smaller the bandwidth around 400% FPL, the more precise the effect of the PLS on households labor supply will be. However, based on a power analysis I have enough power only when I define treated households as earning between 300% - 500% of the FPL; however, households in both extremes do not have any incentive to adjust their income. Thus, this may add noise to the data and attenuate the results toward zero. The PLS_{it} variable is computed as the difference between the family's benchmark premium and the statutory contribution based on the percentage of income required at 400% FPL (9.50% of the annual income in 2014, 9.56% in 2015, 9.66% in 2016 and 9.69% in 2017), with a minimum of zero. The vector of covariates $\mathbf{X}_{it}$ includes other household-level factors that potentially affect the intensive margin of labor supply such as age, sex, race, education, family size, marital status, self-reported health status, self-employment status, job industry and health care expenditures from the previous year. I also include in $\mathbf{X}_{it}$ year and state or region dummies to control for different years and trends in regional labor markets as the difference in labor supply and income growth may come from specific characteristics of the state households live in, e.g., rural/urban state or

state with low/high unemployment rate. Finally, the coefficient of interest is β_7 from the three-way interaction; this coefficient captures the partial effect of the PLS on the intensive margin for the affected households, holding everything else constant. The coefficient on PLS alone, β_3, should have no explanatory power for income growth or change in hours when the double interaction term is included.

In the above specification, the control group consists of households offered ESI and the model compares labor outcomes between households offered and not offered ESI, who may be intrinsically different. Therefore, a second model can instead compare changes in labor supply for *only* households not offered ESI in pre- versus post-ACA such that:

$$\begin{aligned} Y_{it} = {} & \beta_0 \mathbf{X}_{it} + \beta_1 NearCutoff_i + \beta_2 Y2post_{it} + \beta_3 PLS_{it} \\ & + \beta_4 NearCutoff_i \times Y2post_{it} + \beta_5 NearCutoff_i \times PLS_{it} \\ & + \beta_6 Y2post_{it} \times PLS_{it} \\ & + \beta_7 NearCutoff_i \times Y2post_{it} \times PLS_{it} + \epsilon_{it}, \end{aligned} \tag{2.2}$$

where $Y2post$ is an indicator for whether or not the second year of interview is in 2014 or later. When using the "not offered" sample, the coefficient of interest is β_7 from the three-way interaction; this coefficient captures the partial effect of the PLS on the intensive margin for the affected households, holding everything else constant.

Finally, the full model (2010-2017) uses a four-way interaction term following the same pattern as the specifications above. In this last model, the

primary variable of interest takes into account the fact that the PLS is only relevant since 2014 and only for households near the top threshold who are not offered ESI. Therefore, the main variable of interest becomes $NearCutoff \times NotOfferedESI \times Y2post \times PLS$ and its estimated coefficient represents the partial effect of the PLS on labor supply outcomes (such as income growth or hours of work) for households near the top threshold and not offered ESI in post-ACA period. A significant and negative coefficient estimate would suggest that a $100 increase in the PLS generates a particular decrease in labor supply for households that reasonably risk losing the subsidy. In this last specification, the coefficient estimate on $NearCutoff \times NotOfferedESI \times PLS$ tests whether the PLS had any effect on labor supply *prior* to 2014 and is expected to be not significant – the PLS in pre-ACA years does not exist, and placebo values based on 2014 premiums are used for the analysis. The counterfactual is what would have happened to labor supply if households had *zero* PLS while having the same income level, family age and size profiles. Thus, in this specification the control group consists of households with similar income (similar % FPL).

The main identification threat in linear regressions is endogeneity bias in the form of omitted variable bias. Thus, I try to control for most of the variables that could be correlated with my main variables of interest via all the above covariates and interaction terms. Moreover, the (short) panel structure of my data is crucial for my identification strategy, as it allows me to analyze changes in labor supply *within* households over two consecutive years and to effectively control for each household's unobserved factors that generated their outcomes in

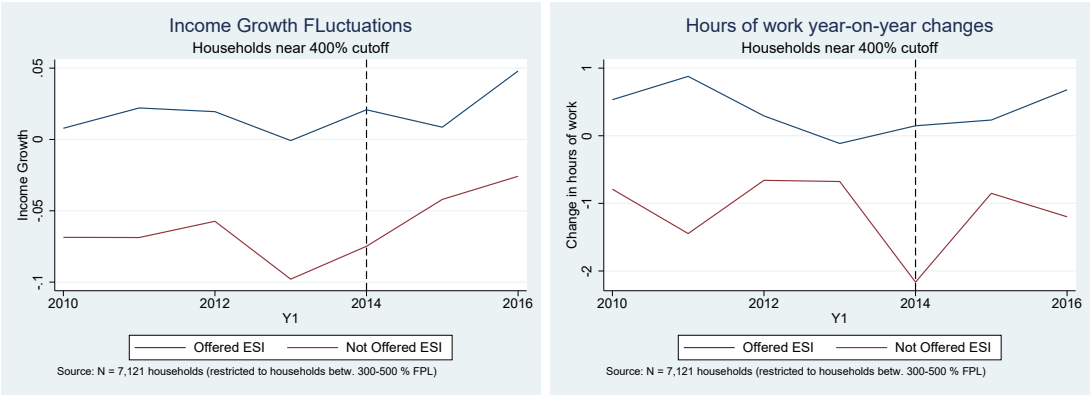
the previous year. Lastly, I do not believe that my identification suffers from a serious endogeneity problem as households may not have adjusted their location due to the PLS.

To measure the effect of the ACA subsidy on the extensive margin, I use a probit model and estimate changes in employment status for any adult in the household over the two consecutive years of interview. I use the same regressions and models as above (1-2). Here, my goal is to evaluate whether there is switching behavior in labor force participation among adults in the household as the PLS becomes larger. The outcome variable is now binary and equals one if any adult in household i (head of household, spouse or other adult dependent) stops working in the second year of the survey, and equals zero if all individuals stay employed (currently employed or with a job to return in the current round of interview). A positive and significant coefficient estimate would then suggest that an adult member is more likely to stop working if the household is near 400% FPL, not offered ESI and has a larger PLS after ACA implementation.

2.6 Results

First, I check the parallel trend assumption in labor supply outcomes between households near the 400% FPL cutoff by ESI status in Figure 2.7. The year-on-year changes in income is trending parallel between the two types of households prior to 2014. However, a parallel trend in the year-on-year changes in hours of work is less clear among the two groups of households prior to the implementation of the subsidy.

Figure 2.7: Common Trends in Labor Supply Outcomes



(a) Trends in year-on-year Income Growth (b) Trends in year-on-year Hours of Work

Table 2.3 presents the main coefficient estimates from the income growth regressions using the post-ACA sample in column 1, the sample of not offered ESI in column (2) and the full sample in column (3). The regression tables with the coefficients for socio-economic variables can be found in Tables A.2, A.3, and A.4 of the Appendix. Using either the post-ACA sample or the full model, I find a significant and negative effect of not being offered ESI on income growth. Over 2010 to 2017, households who are not offered health insurance through their employer experience an income growth 5.4% lower than those offered ESI, holding everything else constant. Such households in the post-ACA sample face an even larger differential as their income growth is 7.1% lower, which could suggest that the earnings gap between the ESI and non-ESI households is getting wider since the ACA.

In all three specifications the coefficients of interest are not statistically

Table 2.3: Main OLS regression results on household income growth

	(1) Post	(2) Not ESI	(3) Full
Near 400% FPL	.0882***	.09497***	.09486***
300% FPL < Annual Income < 500% FPL	(.011)	(.026)	(.008)
Not Offered ESI	-.07125***		-.05418***
	(.024)		(.018)
Monthly PLS ($100)	-3.73e-04	-4.70e-04	.00634***
	(.0024)	(.0065)	(.0024)
Near 400% FPL × Not Offered ESI	.006494		-.01024
	(.035)		(.027)
Not Offered ESI × PLS	.00641*		.00382
	(.0039)		(.0034)
Near 400% FPL × PLS	.00631**	.00325	.00702
	(.003)	(.0092)	(.0044)
Near 400% FPL × Not Offered ESI × PLS	-.003		-.00817
	(.0082)		(.0086)
Post-ACA × PLS		.00106	-.00591**
		(.0067)	(.0027)
Near 400% FPL × Post-ACA × PLS		6.69e-04	-.00249
		(.011)	(.0046)
Near 400% FPL × Not Offered ESI × Post-ACA × PLS			.00927
			(.0098)
N	18,905	7,572	33,412
R^2	.06844	.08252	.06261
Industry dummies	X	X	X
State FE	X		X
Region FE		X	
Year FE	X	X	X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Dependent variable is the income growth. All variables are at the household level and use the MEPS family weights. Models control for socio-economic variables not reported in this table such as head of household's age, age squared, sex, marital status, self-reported health status, self-employment status, medical expenses of the first year of survey, industry dummy and income level dummy (below 150% FPL, between 150 and 300% FPL, between 450 - 600% FPL and above 600% FPL, the omitted group) both linear and interacted with the not offered ESI variable.

significant; however, the economic interpretation of these results provides important insights. In the post-ACA sample (column 1), the coefficient of interest of -0.003 means that as the PLS increases by $100 per month, households with an income near 400% of the FPL and not offered ESI decrease their income growth by 0.3%, representing almost 14% of their average income growth.[13] The standard error of the coefficient of interest generates a 95% confidence interval that spans between -0.0194 and 0.0134 for the average PLS of the treated households ($100). The effect on income growth for the average PLS of households aged 50+ ($400) then spans between -0.07 and 0.05 representing 1/5 of the income volatility experienced by such households. These magnitudes are large but plausible. Thus, the null effect of the PLS on income growth can be considered as a fairly precise null effect.

To test the validity of the main parameter estimate, it is also important to check its potential impact in the pre-ACA period for households near the cutoff and not offered ESI. Prior to 2014, the PLS should not have any effect as the Marketplace and the subsidies do not exist – the "PLS" used is a placebo value, calculated using 2014 premiums. As a consequence, income growth should not vary due to a change in PLS. Indeed, the coefficient estimate for the PLS for the affected households in the pre-ACA period are very close to zero (and economically insignificant), and their respective p-values confirm that these estimates are not statistically significant. Thus, my models confirm that the PLS is not coincidentally correlated with excluded predictors of income growth.

[13] Households near 400% FPL have a year-on-year income growth of 2.2% on average.

Table 2.4: Main OLS regression results on change in hours of work

	(1) Post	(2) Not ESI	(3) Full
Near 400% FPL	-.1113	-.7994	-.3635
300% FPL < Annual Income < 500% FPL	(.34)	(.77)	(.25)
Not Offered ESI	.5364		.2785
	(.69)		(.51)
Monthly PLS ($100)	-.1288*	-.2611	-.1889**
	(.066)	(.21)	(.074)
Near 400% FPL × Not Offered ESI	-1.646		-.6084
	(1.1)		(.84)
Not Offered ESI × PLS	.1876*		.2007**
	(.11)		(.099)
Near 400% FPL × PLS	.05364	.1841	.3169***
	(.081)	(.34)	(.099)
Near 400% FPL × Not Offered ESI × PLS	.2128		-.2382
	(.22)		(.33)
Post-ACA × PLS		.1982	.03655
		(.19)	(.077)
Near 400% FPL × Post-ACA × PLS		-.1131	-.2573**
		(.35)	(.11)
Near 400% FPL × Not Offered ESI × Post-ACA × PLS			.3238
			(.31)
N	18,905	7,572	33,412
R^2	.1414	.03083	.1388
Industry dummies	X	X	X
State FE	X		X
Region FE		X	
Year FE	X	X	X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The dependent variable is the change in hours of work from one year to another. All variables are at the household level and use the MEPS family weights. Models control for socio-economic variables reported in the Appendix such as age of head of household, age squared, sex, marital status, self-reported health status, self-employment status, medical expenses in the first year of survey, industry dummies, year dummies and state or region fixed effects as well as both linear and interacted income level dummies (below 150% FPL, between 150 and 300% FPL, and above 500% FPL the omitted group) with the not offered ESI variable.

Table 2.4 shows the estimates of the change in weekly hours of work over two consecutive years – the intensive margin of the labor supply. I find that the main coefficients of interest are negative in the "not offered" sample (-0.11) but not significant in the post-ACA sample or in the full sample. However, it is important to note that it may be difficult for households to adjust their hours of work in general. One of the limits of this paper is that the MEPS data do not provide the total amount of hours worked during a year, but rather the usual hours of work per week.

Finally, Table 2.5 reports the impact of the PLS on the extensive margin of family labor supply: the probability that any adult aged 21 to 64 stops working in the second year of interview conditional on being employed in the first year. For households in both the post-ACA and the not offered ESI samples, an increase in the PLS by $100 increases the probability that an adult stops working by 5 to 8% points, respectively. For households in the full sample, the probability increases by almost 4% points. However, in all three specifications these main coefficients of interest are not significant. Thus, the PLS has no clear significant impact on the intensive or extensive margins of the family labor supply.

2.7 Discussion

To further evaluate the sensitivity of my results, I restrict the sample to households living in states that have their own Marketplace or Exchange, rather than using Healthcare.gov, the federally facilitated exchange (FFE). Previous studies like Frean et al. (2017) have shown that in such states, individuals are

Table 2.5: Probability that any household member stops working

	(1) Post	(2) Not ESI	(3) Full
Near 400% FPL	.02165*	.03233	.01097
300% FPL < Annual Income < 500% FPL	(.011)	(.026)	(.0079)
Not Offered ESI	-.001949		.01645
	(.018)		(.015)
Monthly PLS ($100)	.00696***	.01764***	.00868***
	(.0014)	(.0051)	(.0013)
Near 400% FPL × Not Offered ESI	.0294		.01461
	(.032)		(.02)
Not Offered ESI × PLS	2.84e-04		6.41e-04
	(.0021)		(.0019)
Near 400% FPL × PLS	-.00266	-.00279	-2.71e-04
	(.0021)	(.0073)	(.0023)
Near 400% FPL × Not Offered ESI × PLS	.00518		4.37e-04
	(.0047)		(.0051)
Post-ACA × PLS		-.00728	-.00172
		(.0047)	(.0015)
Near 400% FPL × Post-ACA × PLS		.00819	-4.77e-04
		(.0089)	(.0025)
Near 400% FPL × Not Offered ESI × Post-ACA × PLS			.0039
			(.0055)
N	18,905	7,572	33,412
Year FE	X	X	X

Marginal effects; Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: The dependent variable is the probability that one of the adult of the household leaves the labor force. The results are the marginal effects of the probit regression. All variables are at the household level and use the MEPS family weights. Models control for socio-economic variables reported in the Appendix such as age of head of household, age squared, sex, marital status, self-report health status, self-employment status, medical expenses in the first year of survey, year dummies and both linear and interacted income level dummies (below 150% FPL, between 150 and 300% FPL, and above 500% FPL the omitted group) with the not offered ESI variable.

more aware of the premium tax credits because those states tend to provide more accessible information and support during the open enrollment period.

Table 2.6: State-Based Marketplace Regressions: main variables

	(1) Income Growth	(2) Change in Hours of Work	(3) Probability to stop working
Near 400% FPL	.09521***	.2466	.03633*
300% FPL < Annual Income < 500% FPL	(.018)	(.64)	(.02)
Not Offered ESI	-.08285**	-.4722	.04153
	(.034)	(1.8)	(.041)
Monthly PLS ($100)	-7.01e-05	-.0667	.00969***
	(.0043)	(.1)	(.0028)
Near 400% FPL × Not Offered ESI	.03071	-1.908	-.01476
	(.049)	(2.1)	(.034)
Not Offered ESI × PLS	.0045	.1712	2.18e-04
	(.0065)	(.25)	(.004)
Near 400% FPL × PLS	.00881	.1214	-.00661*
	(.0059)	(.19)	(.0038)
Near 400% FPL × Not Offered ESI × PLS	-.0062	.1617	.00204
	(.011)	(.41)	(.0088)
N	5,331	5,331	5,331
R^2	.08442	.149	
Industry dummies	X	X	
Year FE		X	X

Marginal effects; Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: States using their own state-based marketplace from 2014 to 2017 are the 11 following states CA, CO, CT, ID, MD, MA, MN, NY, RI, VT, WA, and DC. These regressions are using the post-ACA sample restricted to households living in those states. All variables are at the household level and use the MEPS family weights. Models control for socio-economic variables not reported in the Appendix such as age of head of household, age squared, sex, self-employment status, medical expenses in the first year of survey, industry dummies, and both linear and interacted income level dummies (below 150% FPL, between 150 and 300% FPL, and above 500% FPL the omitted group) with the not offered ESI variable.

Table 2.6 presents the results on income growth, hours of work and change in employment status for households living in the twelve states with State-Based

Marketplaces (SBM) and D.C. since 2014 (5,331 households). As in the all-state sample in Table 2.3, I find no statistical evidence that relevant households reduce their income or hours of work in response to the PLS, and the probability that one of the adults in the household stops working as a result is also not significant. Thus, when restricting the analysis to states using the SBM there is also no evidence for a distortion of labor supply due to the PLS either at the intensive or extensive margin.

Another specification of great interest is to study the probability that households have an annual income below 400% FPL due to a change in the PLS, conditional on being near the cutoff. Table 2.7 shows the results from a specification using a restrictive sample for households near the cutoff, not offered ESI and interviewed in post-ACA (Column 1), and from a second specification comparing households near the cutoff and not offered ESI in pre- and post-ACA (Column 2). The coefficient on the annual income in first year is negative and significant in both specifications – as households have a larger annual income they are less likely to be below 400% FPL in the second year of interview which is consistent. In specification (1), the variable of interest is the PLS alone. If households were reducing their income due to the PLS, a change in the PLS would increase the probability they fall below 400% FPL and the coefficient estimate would be positive. However, I find its marginal effect to be negative and not statistically significant. Thus, households highly likely to be affected by the PLS do not reduce their income from one year to another. In specification (2), the variable of interest is $PostACA \times PLS$. One would expect its marginal effect to be positive;

however, it is also negative and not statistically significant. Finally, the coefficient on PLS itself in specification (2) corresponds to the impact of the PLS in pre-ACA, and as expected, is very close to zero but not statistically significant.

Table 2.7: Probability to be below 400% FPL in the following year

	(1) Near & Not ESI & Post	(2) Near & Not ESI
Income as % of FPL in year 1	-.003721***	-.003657***
	(.00041)	(.0003)
Monthly PLS ($100)	-.0001123	3.28e-06
	(.00012)	(.00014)
Post-ACA × PLS		-.0001083
		(.00017)
Post-ACA		.04284
		(.066)
N	685	1,157

Marginal effects; Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Some limitations occur in the current analysis. First, among the treated group, only the ones aged 50+ have a strictly positive PLS while the rest and majority of them often faces a near-zero PLS, at least in the first years of the ACA (cf. Figures 2.5 and 2.6). This may create some noise in the data (from those with a zero PLS) and potentially attenuates the results toward zero. Second, one may want to use a larger dataset to narrow the bandwidth when considering households *near* 400% FPL since with the current 300-500% FPL range, households at both extremes have very little or zero incentive to adjust their income in response to the PLS. Using a smaller income range would then avoid combining households with near-zero intensive margin with households who would have larger effects on

the intensive margin. Finally, a larger dataset would also allow to compared self-employed workers to regular wage earners and study whether the self-employed adjust their income since they have more flexibility when reporting their earnings.

2.8 Conclusion

In this Chapter, I study the labor supply effects of a unique means-tested program for moderate income households, the premium tax credit. Because the subsidy falls sharply to zero above 400% FPL, households near this cutoff experience a potential lost subsidy (PLS). This discontinuity in the subsidy greatly varies by geographic location and family structure. For most households the PLS represents a small dollar amount or even zero. However, for older couples, families of four, or families living in states with high-cost premiums, the PLS can represent around 8% to 15% of their income.

Using the MEPS and premium data from 2010 to 2017, I find that the PLS equals $100 per month for younger workers but reaches $400 to $600 per month for workers above 55 years old, on average. However, I find no statistical evidence that households not offered ESI and earning near 400% FPL reduce their income and labor supply at the intensive or extensive margin. The current null result is economically plausible when taking into consideration household income volatility and suggests the effect is a moderately precise null. Thus, I find that the ACA subsidy design does not distort labor supply nor creates income manipulation for households near the 400% FPL cutoff.

Future research may want to look at recent policy reforms about the subsidy design as some states like California are extending the APTC to middle-income households up to 600% FPL for the 2020 marketplace insurance plans. Providing more affordable health insurance plans for middle-income households may impact coverage enrollment, financial well-being, and health outcomes. Health insurance on the Marketplace is about to become more prominent as millions of Americans are losing their employer-based health coverage due to the coronavirus economic crisis. Affordability of insurance plans is crucial for However, the current uncertainty about the future of the ACA exacerbates the rise in premiums and thus, the increase in the PLS. When the federal government ended the cost-sharing reduction (CSR) payments to insurers in October 2017, the announced premiums for the following year show a surcharged from 7% to 38% (Kamal et al., 2017). A larger PLS may accentuate changes in labor supply over time for moderate- and middle-income households. Thus, it is crucial to build on the ACA, eliminate the subsidy discontinuity by extending income-eligibility, and increase the Marketplace outreach to improve the affordability of health insurance plans for middle-income households.

Chapter 3

THE COVERAGE GAP IN MEDICAID NON-EXPANSION STATES AND ITS LABOR SUPPLY EFFECTS

3.1 Introduction

One of the main provisions of the Patient Protection and Affordable Care Act (ACA) is the expansion of Medicaid in 2014 to non-elderly and non-pregnant individuals with an income up to 138% of the poverty line.[1] As of 2019, more than 65 million of low-income and disabled people (21% of Americans) are enrolled in Medicaid, one of the largest means-tested welfare program. However, in *National Federation of Independent Business v. Sebelius* (2012), the Supreme Court of the United States declared the Medicaid expansion unconstitutional and gave states the option to not expand coverage without losing their federal Medicaid funding. In January 2014 and as the ACA went into effect, 25 states did not expand Medicaid; as of today, there are still 12 non-expansion states. In such states, Medicaid coverage is limited to households with income up to 44% of the poverty

[1] Equivalent to earn $16,753 a year for a single individual and $34,638 for a family of four with two children in 2019.

line on average, and childless adults remain mostly ineligible (Kaiser Health News, 2018).[2]

Although the Medicaid expansion drastically decreased uninsured rates in expansion states, states that did not expand Medicaid now have a "coverage gap". Some households have an income too low to qualify for advance premium tax credits (APTC) starting right at the federal poverty line (FPL), but too high for Medicaid eligibility under their state's existing policy. In 2018, around 2.3 million non-elderly adults fall into this coverage gap, equivalent to 9% of the uninsured population (Sommers, 2020). If states decided to expand Medicaid 4.4 million adults would become eligible, including those already eligible but not currently enrolled (Garfield et al., 2020). As a result of the ACA subsidy design and Medicaid non-expansion, the coverage gap creates a health policy change which has potentially large socioeconomic consequences on a state-by-state basis as of January 2014.

Notably, households expecting to earn just below the poverty line in non-expansion states face a large financial incentive in their after-tax income at the poverty line. The tax credit generates an upward discontinuity in their budget at this threshold which changes their labor supply incentives. In this Chapter, I study the labor supply behavior within low-income households. Specifically, I examine whether households in non-expansion states who are just below the poverty line increase their labor supply and income to qualify for APTC for

[2] Among non-expansion states, only Wisconsin provides Medicaid coverage to childless adults up to the poverty line. Figures B.1 and B.2 in the Appendix display the map of Medicaid non-expansion states and their eligibility thresholds as of January 2017, respectively.

health insurance on the marketplace after 2014. Thus, I investigate whether there are differences in labor supply behavior at the subsidy income-eligibility threshold among low-income households and between Medicaid expansion and non-expansion states.[3]

With data from the Current Population Survey (CPS) and its Annual Socio-Economic Supplement (ASEC) from 2010 to 2018, I use a difference-in-differences framework to examine the impacts of the coverage gap on (1) household income growth, (2) change in hours of work, and (3) change in employment status and labor force participation within households from one year to another. This work contributes to the current literature in several ways. First, I study the effects of a means-tested program that indirectly creates work incentives rather than generating disincentives to earnings. Second, I estimate labor supply decisions within very poor households and focus on non-expansion states often understudied and used as a control group in Medicaid expansion studies. Finally, in this work I exploit a triple exogenous variation from the ACA healthcare reform across income groups, states, and time to provide new evidence about labor supply responses to tax credits.

The rest of this Chapter is organized as follows. In Section 3.2, I provide more details about the financial incentive at the cutoff and review the related literature. In Section 3.3, I describe the data I use and present descriptive statistics about low-income households before explaining my identification strategy in

[3] Households below the poverty line do not face the ACA tax penalty (repealed in 2019) as they are often below the IRS tax filing requirements or are exempt if living in a non-expansion state and would have qualified had the state expanded Medicaid.

Section 3.4. Finally, in Section 3.5, I discuss the results and conclude in Section 3.6.

3.2 Background and Literature Review

Since 2014, very poor households in non-expansion states suddenly face (1) the arrival of the premium tax credits available starting at the poverty line and (2) the absence of Medicaid expansion. In the meantime, such households are less likely to receive employer-sponsored insurance than moderate-income families – only 11% of low-income families are enrolled in ESI coverage in 2018 (cf. Figure B.3 in the Appendix). In Medicaid non-expansion states, crossing the poverty line will make households who are almost surely uninsured become eligible for a zero or very low-cost health insurance on the Marketplace.[4] On the contrary, in Medicaid expansion states, crossing the poverty line does not bring any similar incentive since Medicaid is extended up to 138% FPL.[5]

For low-income households, the ACA subsidy reduces the cost of a silver plan on the Marketplace to 2% of their income which corresponds to $21 per month for a single or $43 per month for a family of four. In addition, if subsidy-eligible households decide to purchase a Silver plan, they will be eligible for cost-sharing reductions (CSR), extra savings on deductibles, copayments, and other

[4] The actual "discontinuity" will depend on their own valuation of the health insurance they can get on the Marketplace.

[5] Even crossing the 138% FPL in Medicaid expansion states has a small and ambiguous effect. Households can switch from Medicaid to private insurance plans providing them with greater access to doctors; however, they have to pay a small premium. Thus, only the cutoff at the poverty line is truly differentiating labor supply incentives between expansion and non-expansion states.

out-of-pocket costs when they need care. The actuarial value of their Silver plan will then be 94%, meaning that the insurer will cover 94% of their medical expenses.[6] The subsidy can be used to purchase any category of Marketplace plans (Bronze, Silver, Gold, or Platinum) and very often covers the entire premium of a Bronze plan. Such plans provide the minimum level of coverage with lower premiums but higher out-of-pocket costs than a Silver plan (higher deductibles and coinsurance). Thus, there is a large financial incentive at the poverty line for poor households in non-expansion states to increase their income and labor supply.

Several papers in labor economics and health economics are related to my work. The ACA Medicaid expansion motivated substantial research, especially regarding states that have chosen to adopt the policy. An extensive literature exists on the impacts of the Medicaid expansion on health insurance coverage, health care access, health outcomes, and labor outcomes. First, several studies have shown that the Medicaid expansion greatly improved health insurance coverage rates (e.g., among the low-income adults, young dependent, low-educated, and childless adults) by increasing the number of newly insured as well as the enrollment of previously eligible individuals, known as the woodwork effect (Frean et al., 2017; Akosa Antwi et al., 2013; Kaestner et al., 2017; Leung and Mas, 2016).

Second, providing health insurance to a larger population can create some concerns about a potential reduction in labor supply, for those with high expected

[6] The actuarial value of the silver plan is 87% (151-200%), and 73% (201-250% FPL)

medical expenses (Moffitt and Wolfe, 1990), childless adults (Dague et al., 2017) or the newly covered as workers are no longer locked to their job for their health insurance coverage (Garthwaite et al., 2014). However, this same effect also increases job mobility, potentially improving worker-employer match (Gruber and Madrian, 2002). So far, numerous studies find no significant negative impact of Medicaid expansion on employment, labor force participation, and earnings (Baicker et al., 2014; Peng et al., 2018; Leung and Mas, 2016; Kaestner et al., 2017).

Despite extensive research on the effects of the Medicaid expansion, the consequences of the ACA healthcare reform in non-expansion states have so far been understudied. To the best of my knowledge, only one paper has investigated the labor supply impacts of the coverage gap in Medicaid non-expansion states. Kucko et al. (2018) study the effects of the upward tax notch on labor supply from 2012 to 2015 for households near the poverty line using a bunching analysis with tax returns data from the Internal Revenue Service (IRS) and labor supply data from the American Community Survey (ACS). They find statistical evidence of bunching near the poverty line among self-employed filers, but no evidence that the bunching behavior is a change in labor supply. Rather, the authors suggest that it might be a change in reported income from the self-employed, who are more able to adjust their reported income than wage and salary earners.

In this Chapter, I contribute to the current literature in several ways. First, I use the short-panel structure offered by the CPS ASEC data to examine changes in labor supply within households over two consecutive years. With this

data structure, I can explore the intra-family labor supply dynamics, including changes in employment behavior among family members. Second, I use data up to 2018 (the most recent available) to provide an analysis of the effects of the coverage gap on labor supply beyond its initial introduction. That is, it may take some time for individuals in non-expansion states to be aware of the new tax incentive at the poverty line and to adjust their labor supply or income accordingly. Finally, I separately estimate the impacts by family composition (childless adults versus couples with children) and employment status (self-employed versus wage and salary earners).

3.3 Data

3.3.1 Longitudinal Matching in the CPS data

In this study, I take advantage of the longitudinal component of the Current Population Survey (CPS) and its Annual Socio-Economic Supplement (ASEC).

The CPS consists of a monthly survey of around 98,000 households who are interviewed for four consecutive months before leaving the survey for eight months and being interviewed again the following year for the same four consecutive months (known as the 4-8-4 rotation pattern). Thus, the structure of the CPS data provides researchers with the opportunity to construct short panels by following the same households over two consecutive years. Its longitudinal potential has been made possible by the innovative and ongoing project of Integrated Public Use Microdata Series (IPUMS) (Flood et al., 2019) based on the valuable work of Madrian and Lefgren (2000) and Rivera Drew et al. (2014) that created a

unique person record identifier in IPUMS-CPS files.[7] To convert the CPS ASEC into repeated two-year panels, I simply match individuals interviewed in March of year t ("month-in-sample" or MIS 1-4) to the same individuals in March of year $t+1$ (MIS 5-8) by CPSIDP.[8] Technically, half of a CPS sample can be linked from one year to the next; however, due to attrition, migration, and mortality, actual linkage rates are lower than possible rates, and I could only match one-third of the CPS sample following other studies' estimates (Flood et al., 2019). Pooling the CPS ASEC from 2010 to 2018, I construct a repeated cross-sectional dataset of more than 140,000 households with 289,085 individuals for whom I have information over two consecutive years; the unit of observation in all specifications is a household-year pair.

3.3.2 Advantages and Limitations of the CPS ASEC data

The first advantage of the CPS ASEC in this study is its longitudinal component allowing a comparison of labor supply measures within households over time. While keeping one observation per household, I record labor supply information from both the head and spouse, as well as any other adults in the household (if any) regarding their hours of work, employment status, and labor income.

A second reason for using the CPS ASEC data is its sample size. Several

[7] Available at https://cps.ipums.org/cps/

[8] I drop CPS respondents from other months that are also included in the ASEC, extra sample known as the ASEC oversample (Hispanic oversample, and SCHIP oversample)

publicly available datasets gather similar labor supply information while providing a longer panel structure. For instance, the Survey of Income and Program Participation (SIPP) follows the same individuals for up to four years and provides detailed information on their income and health insurance coverage on a monthly basis. However, the sample sizes are limited to about 36,700 households interviewed. Likewise, the Panel Study of Income Dynamics (PSID) conducted by the University of Michigan since 1968 is the longest longitudinal household survey and collects socio-economic data over multiple generations of American families. However, in 2013, it has around 9,000 households, much smaller than the CPS ASEC. Thus, other similar available longitudinal surveys have limited sample sizes resulting in a statistical challenge for studying sub-population groups (e.g. the low-income, childless adults, self-employed). Therefore, CPS ASEC appears to be the most favorable dataset for this specific study.

Finally, a third advantage of the CPS data is that it includes extensive and highly accurate socioeconomic information. The ASEC is a unique CPS supplement because it contains additional questions related to income, employment, and health coverage (compared to the Basic Monthly CPS data files). Also, the income and tax-related variables in the CPS ASEC are verified by the Census Bureau and the IRS rather than coming directly from respondents' answers which make them more reliable.[9] As a result, the ASEC is used to produce U.S. national estimates on the poverty rate, unemployment, and health insurance coverage. To measure family income, I use adjusted gross income (AGI), which is very similar

[9] For more information, see Current Population Reports, Series P60-18RD

to the modified adjusted gross income (MAGI) used to determine eligibility for Medicaid and ACA subsidy.[10]

However, some limitations emerge from using the CPS ASEC data. First, the short duration of the CPS panels restricts the observation of the same family over two years which may be a period of time too short for households to be aware of the premium tax credits available at the poverty line and increase their labor supply in response. Second, the attrition between waves of survey is a potential source of bias. Some observations may be missing not at random, and respondents with lower income tend to have a greater tendency to drop out of survey. However, one of the reasons for the rotation pattern of the CPS is to actually decrease the burden on respondents and reduce this attrition problem.

Finally, the 2014 CPS ASEC differs from other yearly datasets. Part of the 2014 sample (5/8 of the original sample) receives traditional questions on health coverage and more importantly, income while the rest of the sample is asked redesigned questions.[11] Redesigned income questions are re-ordered in the survey to match household-specific characteristics. For example, if the household is a low-income family, questions about federal programs and food stamps are asked first. If the household is retired, questions about retirement pension plans are asked first. In that case, the suggestion from the Census Bureau is to use the 2014 traditional sample to compare income to 2013 and before, and to use

[10] MAGI includes AGI, foreign earned income, tax-exempt income, and non-taxable Social Security benefits

[11] The redesigned questions ask about current health insurance coverage rather than last year's insurance but this does not directly affect my analysis.

the 2014 redesign sample to compare income to 2015 and onward. However, this adjustment does not change my estimated results.

3.3.3 Descriptive Statistics

Several data adjustments are needed to compare changes in household labor supply depending on the Medicaid expansion status of their states of residence. After obtaining a repeated cross-sectional dataset from 2010 to 2018, I first replicate labor market information of the spouse and other adult members within each specific household and convert the full sample to the household level. Because financial eligibility for federal programs is based on the health insurance unit definition from the State Health Access Data Assistance Center (SHADAC), I use their family definition to determine the household unit of CPS respondents. The SHADAC family definition slightly differs from the Census definition of a family, as the former is narrower and does not include grandparents nor extended family as household members. Hence, I compute the household's annual income as a percentage of the poverty line based on the SHADAC health insurance unit and the federal poverty guidelines issued by the Department of Health and Human Services which vary by year and family size.[12] Additionally, I restrict the sample to households with an annual income between 70 percent and 150 percent of the poverty line in the first year of survey. Also, the head of the household is

[12] For more details, see SHADAC, Brief (2013). Federal Poverty Guidelines (FPG) are used to determine income eligibility for Medicaid coverage and the premium subsidies and cost-sharing on the exchanges. They vary by family size and year. The 48 contiguous states and DC use the same FPG while Alaska and Hawaii have their own FPG. Available at: https://aspe.hhs.gov/prior-hhs-poverty-guidelines-and-federal-register-references

between 21 and 64 year old and not in the armed forces. Moreover, to measure the intensive margin of labor supply, I use weekly usual hours of work at all jobs – actual hours of work have potentially more variability as they reflect hours from a single week before the survey. As for the extensive labor supply, I use employment status, labor force participation (in and out-of-the labor force), and work status (full-time vs part-time). Finally, in the rest of this study, I differentiate very poor households earning below the poverty line (equivalent to $12,140 for an individual and $25,100 for a family of two adults and two children in 2018) from poor households earning above and up to 150% FPL.

Table 3.1 presents the summary statistics of households interviewed before the ACA Medicaid expansion (2012-2013 panel) by income group and (future) expansion states status.[13] In both types of states, households below 100% FPL have a higher income growth than households just above. For example, in non-expansion states, very poor households have an income growth of 17% while poor households just above the FPL have an income growth of 10%. The double-digit income growth for low-income households mainly results from the high variability in their flow of earnings as well as their initially low level of income compared to middle-income families and their 2% income growth on average (for those around four times the poverty line, cf. previous Chapter). In both types of states, very poor households have much lower hours of work than poor households (13 hours a week compared to 20 hours a week), they are also more likely to be out-of-the

[13] For the summary statistics of pre-ACA panels, the (future) expansion status is based on the 2014 state information.

labor force (46% vs 26%).

Some important differences appear between the two types of states. Low-income households in (future) non-expander states are less likely to be unemployed, but more likely to be out of the labor force (difference of 4 to 7 percentage points), and less self-employed compared to low-income households in (future) expander states. As for health insurance measures, the uninsured rates is quite high in both types of states for low-income households (25% to 30%) and the share of households with a private insurance (ESI or non-group insurance from the Marketplace) is quite low (23% to 36%). Finally, low-income households in non-expansion states are less likely to be covered by the traditional Medicaid and more likely to have poor health than households in future expander states.

In the post-ACA period, previous socio-economic differences widen and new significant disparities appear. Table 3.2 presents descriptive statistics by income groups and states for households interviewed in 2017 and 2018. Among the pre-ACA differences previously observed, the gap in the percentage of households covered by Medicaid increases (at the detriment of those living in non-expansion states) and is now of 10 percentage point versus 6 percentage point in the pre-ACA panel. The percentage of uninsured dropped to 6% among households just below the poverty line in Medicaid expansion states and is disproportionately high in non-expansion states (17% to 24%). Finally, the percent of households with private insurance is still around 30% to 40% in both types of states.

New disparities emerge between the two types of states at the expense of those in non-expansion states. Statistics for heads of household in non-expansion

Table 3.1: Mean characteristics of low-income households in pre-ACA period (2012 - 2013)

	Expansion		Non-Expansion	
	Below (70-99 % FPL)	Above (100-150% FPL)	Below (70-99 % FPL)	Above (100-150% FPL)
N, # of Households (working age group 21-64)	465	759	388	584
Estimated Population (million)	697,283.94	1,129,963	714,848.89	1,016,959
At household level:				
Annual Income as % FPL	85.26	123.39	85.32	125.81
Annual Income ($)	12,017.69	17,591.49	11,827.16	18,219.06
Income growth	0.16	0.13	0.17	0.10
Total Annual Medical Expenses ($)	2,137.20	2,017.29	1,999.85	2,337.19
Head of household (in Y1):				
Usual Hours of work (/week)	13.51	20.98	12.82	21.34
Actual Hours of work (last week)	14.53	21.90	12.81	22.11
Employed	0.46	0.63	0.42	0.62
Unemployed	0.07	0.08	0.04	0.06
Out of Labor Force	0.46	0.28	0.53	0.32
Self-employed	0.05	0.07	0.02	0.05
Medicaid (last year)	0.46	0.26	0.40	0.18
Uninsured	0.25	0.30	0.30	0.35
Any private insurance	0.23	0.36	0.23	0.36
Excellent Health	0.16	0.23	0.14	0.17
Very good Health	0.19	0.27	0.17	0.27
Good Health	0.31	0.27	0.27	0.26
Fair Health	0.22	0.15	0.22	0.20
Poor Health	0.13	0.09	0.20	0.10
Age	41.32	42.01	44.49	42.82
White	0.75	0.80	0.71	0.78
Black	0.17	0.12	0.23	0.17
Other (Asian, Hispanic etc.)	0.08	0.08	0.06	0.04

Table 3.2: Mean characteristics of low-income households in post-ACA period (2017 - 2018)

	Expansion		Non-Expansion	
	Below (70-99% FPL)	Above (100-150% FPL)	Below (70-99% FPL)	Above (100-150% FPL)
N, # of Households (working age group 21-64)	394	600	221	329
Estimated Population (million)	692,219	1,042,029	387,392	591,206
At the household level:				
Annual Income as % FPL	83.71	122.88	82.56	124.93
Annual Income ($)	12,737.82	19,485.78	12,589.75	19,561.68
Income growth	0.22	0.09	0.19	0.16
Total Annual Medical Expenses ($)	2,207.40	2,697.65	2,258.21	2,898.38
Head of household (in Y1):				
Usual Hours of work (/week)	12.75	18.34	10.73	20.36
Actual Hours of work (last week)	12.83	18.62	10.97	20.47
Employed	0.43	0.56	0.35	0.59
Unemployed	0.03	0.05	0.03	0.03
Out of Labor Force	0.54	0.39	0.62	0.38
Self-employed	0.04	0.06	0.03	0.08
Medicaid (last year)	0.63	0.43	0.52	0.23
Uninsured	0.06	0.11	0.17	0.24
Any private insurance	0.29	0.42	0.29	0.47
Excellent Health	0.17	0.20	0.19	0.25
Very good Health	0.22	0.25	0.13	0.24
Good Health	0.27	0.27	0.29	0.25
Fair Health	0.21	0.20	0.24	0.17
Poor Health	0.13	0.08	0.16	0.09
Age	43.15	42.45	42.66	42.14
White	0.77	0.77	0.67	0.76
Black	0.15	0.11	0.23	0.19
Other (Asian, Hispanic etc.)	0.08	0.12	0.10	0.05

states greatly differ from those in Medicaid expansion states, especially for usual hours of work (2 hours a week lower) and the percentage of them employed (8 percentage points lower). In addition, total medical expenses equal 2,258 dollars a year on average for the very poor in non-expansion states, equivalent to 18 percent of their average annual income, a considerable amount as for such income group.[14] Finally, compared to moderate-income studied in the previous Chapter, low-income households in non-expansion states are 67% White (vs 81%), 23% Black (vs 12%), and 10% from other ethnic groups (vs 8%).

3.4 Identification Strategy

With repeated cross-sectional data from 2010 to 2018 on households between 70 percent and 150 percent of the poverty line, I use a difference-in-differences (DID) framework to compare within-household labor supply outcomes across time, states, and income groups. By the end of my study period in March 2018, 18 states had not expanded Medicaid. I first compare changes in labor supply measures for the never-expander states for very poor households to poor households just above the poverty line before and after the ACA. In this specification, households below the poverty line are the treated group as they face the incentive of the APTC available slightly higher in the income distribution from 2014 onward and households above the poverty line are the control group.[15]

[14] Total annual medical expenses include the sum of out-of-pocket costs, monthly premiums, and any other medical bills over a year.

[15] Since 2014, they are income-eligible for subsidized health insurance on the Marketplace.

Using a restricted sample of households living in never-expander states over the analysis period, I estimate the effects of the coverage gap as follows:

$$Y_{it} = \beta_0 X_{it} + \beta_1 BelowPoverty_i \times PostACA_t + \lambda_t + \epsilon_{it}, \tag{3.1}$$

where Y_{it} is the change in labor supply outcomes from one year to another for household i in their second year in the survey, year t. As measures of labor supply, I use (a) household's income growth defined as the difference in the log of household annual income,[16] (b) the change in usual weekly hours of work over two consecutive years, and (c) the change in employment status and labor force participation within households from one year to another.

For this last outcome, I estimate a probit model and provide the marginal effects that at least one of the household members (i) enters the labor force, (ii) starts working, or (iii) transitions from a part-time to a full-time job in the second year of the survey. $BelowPoverty_i$ is a binary variable for household i earning below the poverty line in their first year in the survey. $PostACA_t$ indicates that the second year of interview is taking place in 2014 or after, when a coverage gap could affect labor supply decisions. β_1 measures the impact of the arrival of the ATPC for households below the poverty line in non-expansion states. The covariates X_{it} include household characteristics based on the head of household's responses including age, race, level of education, marital status,

[16] When quantities do not vary much over time, the percent change is a linear approximation of the difference in log ($log(x) \approx x - 1$, with $x = InctotY2/InctotY1$). That is why I measure the percent change in income over two years as the difference in the log of income. However, using the standard percentage change formula for income growth does not change the results.

family size, self-reported health status in both years, and medical expenses. To control for socioeconomic differences among states, I also include the annual state unemployment rate obtained from the Bureau of Labor Statistics' Local Area Unemployment Statistics.

In my second specification, I compare the changes in labor supply outcomes among very poor households in the 2014 expander and never-expander states before and after the ACA. This DID model is using the restricted sample of very poor households earning just below the poverty line (70-99% FPL) and the regression is as follows:

$$Y_{ist} = \beta_0 X_{ist} + \beta_1 NonExpansion_s \times PostACA_t + \gamma_s + \lambda_t + \epsilon_{ist}, \tag{3.2}$$

where $NonExpansion_s$ is a binary variable for states that do not expand Medicaid in 2014. The other variables are similar to the ones in regression 3.1 described above. Here, β_1 measures the impact of the absence of Medicaid coverage in non-expansion states after 2014.

Finally, my third specification combines the two approaches above and uses a difference-in-difference-in-differences (DIDID) or triple difference. I assume that there is no interaction effect between the two policies, the arrival of the APTC and the absence of Medicaid expansion coverage. Both simultaneous policies have only an expected positive effect on labor supply in non-expansion states as crossing the poverty line in expansion states does not provide access to better health insurance. Thus, β_1 measures the average change in outcomes between

very poor and poor households within non-expansion states before and after the ACA to the average change across these two income groups in expansion states over time. With this last specification, I exploit the exogenous variation (from the perspective of households) created by the ACA Medicaid expansion and subsidy design in geography, time, and income for households around the poverty line. This specification estimates the regression:

$$\begin{aligned} Y_{ist} = \beta_0 X_{ist} + \beta_1 BelowPoverty_i \times NonExpansion_s \times PostACA_t \\ + \beta_2 BelowPoverty_i \times NonExpansion_s + \beta_3 BelowPoverty_i \times PostACA_t \\ + \beta_4 NonExpansion_s \times PostACA_t + \beta_5 BelowPoverty_i + \gamma_s + \lambda_t + \epsilon_{ist}. \end{aligned} \tag{3.3}$$

In each of the three regressions above, the coefficient of interest β_1 measures the impact of the coverage gap on the changes in labor supply of very poor households. For β_1 to be a valid causal effect of the policy on labor supply, I include γ_s, a set of state fixed-effects to control for time-invariant characteristics specific to states (e.g., local economic conditions), and λ_t, a set of year fixed-effects to control for time-varying factors common to all states that affect labor outcomes (e.g., national economic conditions). In fact, states that do not adopt Medicaid expansion may have different unobserved underlying economic conditions correlated with labor supply measures. In this case, the coefficient estimate would be biased without such additional controls. As a result, all three identifications presented above reduce this potential threat to identification by accounting for spatial heterogeneity affecting both the state decision of expanding Medicaid and

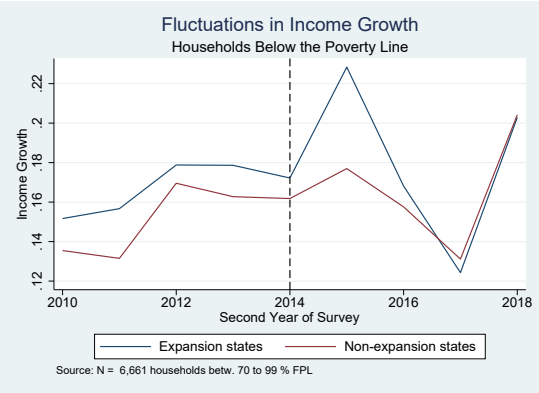
labor market outcomes of the residents.

3.5 Results

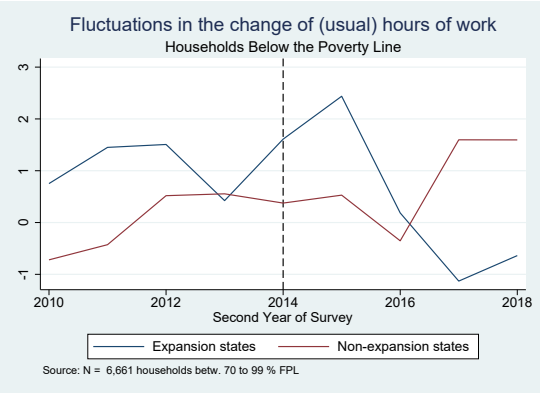
The main strength of the triple difference model is to control for two potentially confounding trends: (1) changes in the labor supply of very poor households across states that would not be related to the Medicaid expansion or coverage gap, and (2) changes in the labor supply of low-income households in non-expansion states possibly due to other local policies.

However, other potential identification concerns may still emerge in particular, if there exists some pre-trends in labor market outcomes across the two types of states and two income groups. For a DIDID to be correctly identified, the differences between households below and above the poverty line must be on parallel trends prior to 2014 in both types of states (Wooldridge, 2010). In Figure 3.1, I plot labor supply outcomes for households below the poverty line over time by state status to compare them before the coverage gap occurs. Panel 3.1a shows income growth over time for very poor households living in expansion versus non-expansion states. Prior to 2014, the parallel trend in households' income growth is quite clear between the two types of states and provides some evidence that my DID models are properly specified. Panels 3.1b illustrates the change in usual weekly hours over time and shows a common pre-existing trend between the treatment and control groups, prior to 2014. Finally, Panel 3.1c shows slightly constant differences in the fluctuations of households entering the labor force over time across states.

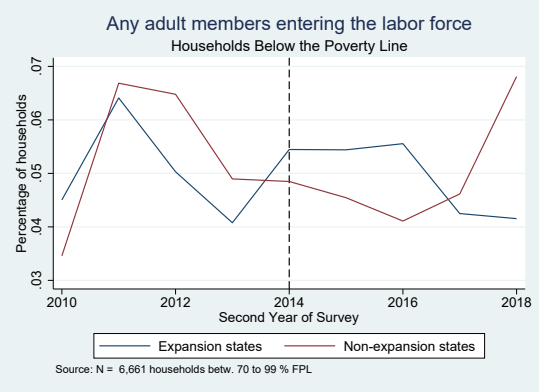
Figure 3.1: Main outcomes over time and across state status for household below the poverty line



(a) Income Growth



(b) Change in usual hours of work



(c) Percentage of households joining the workforce

3.5.1 Difference-in-Differences Within Non-Expansion States

Table 3.3 reports the impact of the coverage gap on income growth for very poor households within states that have never expanded Medicaid over the study period (equation 3.1). Such households experience a 10 percent increase in income growth compared to households just above the poverty line. However, the coefficient of interest measuring the differential change in income growth between income groups before and after the ACA is small and not statistically significant. Thus, there is no evidence that very poor households in never-expander states respond to the financial incentive at the poverty line by increasing their income. The regression results with all covariates and socioeconomic variables are reported in Table B.1 of the Appendix and have expected signs. For example, in never-expander states, women experience an income growth 8% lower than men; having a college degree increases income growth by 14% (or 24% for heads of a family with children) compared to someone dropping out of high school; individuals reporting poor health status in the second year experience a decrease in income growth of 12% compared to individuals reporting excellent health. Standard errors are robust and take into account the survey sampling design of the CPS ASEC data and household weight.

Table 3.4 shows the impact of the coverage gap on the intensive margin of labor supply for the usual weekly hours of work in non-expansion states. I find that among the very poor, childless adults statistically increase their hours of work by 1.7 hours, almost two hours a week, compared to childless adults earning just above the poverty line after 2014 in similar non-expansion states

Table 3.3: Income Growth for the Never Expanders

	All	Childless	With Children
Below 100% FPL × Post-ACA	-.0169	-.0339	.0225
	(0.027)	(0.034)	(0.044)
Below 100% FPL	.0985***	.100***	.0968**
	(0.021)	(0.026)	(0.039)
N	5,307	3,372	1,935
R-sq	.199	.225	.168

Notes: Standard errors in parentheses. Regressions include survey weights, and year fixed effects. The control variables are head of household's age, race, level of education, marital status, family size, self-reported health status, and medical expenses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.4: Changes in Usual hours in Never-Expander States

	All	Childless	With Children
Below 100% FPL × Post-ACA	0.551	1.671*	-0.902
	(0.844)	(0.994)	(1.699)
Below 100% FPL	-1.309**	-1.665**	-1.405
	(0.655)	(0.777)	(1.331)
N	5,307	3,372	1,935
R-sq	0.103	0.098	0.143

Notes: Standard errors in parentheses. Regressions include survey weights, and year fixed effects. The control variables are head of household's age, race, level of education, marital status, family size, self-reported health status, and medical expenses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

(significant at 10 percent). Thus, there is evidence that some households adjust to the coverage gap at the intensive margin by increasing their weekly usual hours worked. However, I find no evidence that employment status and labor force participation differentially change in non-expansion states over time and income groups, as shown in Table B.3 of the Appendix.

3.5.2 Difference-in-Differences Among The Very Poor

Table 3.5 presents the results of regression 3.2 comparing changes in labor supply outcomes for the very poor in expansion versus non-expansion states before and after 2014. Similar to the previous results on income growth in never-expander states, I find no evidence that the very poor increase their income compared to those in expansion states. Moreover, changes in usual hours of work do not exhibit statistically significant differences between the very poor in both types of states as shown in Table 3.6. Finally, the impact of the coverage gap on the probability that one of the household members changes their employment status do not display significant results either as presented in Table B.4 of the Appendix.

In this specification, it is important to note that the control group receives another treatment after 2014 that may attenuate the results. In expansion states, newly-eligible childless adults are 8.9 percentage points more likely to enroll in Medicaid insurance plans (Dworsky and Eibner, 2016). Also, households in expansion states (control group) experience better access to health care and better health outcomes after 2014 relative to individuals in non-expansion states, e.g.,

the unemployed workers (Buchmueller et al., 2019). Thus, they may also experience higher income and/or labor supply outcomes making it difficult to detect an increase in labor supply for very poor households in never-expander states by comparison.

Table 3.5: Changes in Income Growth (Below FPL Sample)

	All	Childless	With Children
Non-Exp × Post-ACA	.000915	-.00672	.0145
	(0.024)	(0.030)	(0.046)
N	5,897	3,965	1,932
R-sq	.234	.245	.255

Notes: Standard errors in parentheses. Regressions include survey weights, state and year fixed effects. The control variables are head of household's age, race, level of education, marital status, family size, self-reported health status, and medical expenses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.6: Changes in Usual hours (Below FPL Sample)

	All	Childless	With Children
Non-Exp × Post-ACA	.404	.54	-.13
	(0.776)	(0.866)	(1.809)
N	5,897	3,965	1,932
R-sq	.114	.109	.164

Notes: Standard errors in parentheses. Regressions include survey weights, state and year fixed effects. The control variables are head of household's age, race, level of education, marital status, family size, self-reported health status, and medical expenses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.5.3 DIDID Across Income Groups, States, and Time

I now present the results of the DIDID model (regression 3.3) comparing the average change in labor supply outcomes by income groups, states, and time.

Table 3.7 presents the main results on income growth. Similar to previous results, households below the poverty line have a larger increase in income growth than households earning just above the poverty line in both types of states (a 13 to 15% increase depending on the specification). However, the DIDID coefficient estimate is close to zero and not statistically significant for any subpopulation groups. Thus, from this model, I confirm that there is no evidence that very poor households in non-expansion states increase their income in response to the upward discontinuity in after-tax income at the poverty line.

Table 3.7: Changes in Income Growth (70-150% Sample)

	All	Childless	With Children
Below 100% FPL × Non-Exp × Post-ACA	-.00597	-.0102	.0133
	(0.034)	(0.043)	(0.055)
Below 100% FPL × Post-ACA	-.0101	-.00939	-.0246
	(0.022)	(0.028)	(0.036)
Below 100% FPL × Non-Exp	-.0248	-.0227	-.0362
	(0.024)	(0.030)	(0.046)
Non-Exp × Post-ACA	.00783	.000908	.0234
	(0.022)	(0.028)	(0.031)
Below 100% FPL	.13***	.131***	.147***
	(0.016)	(0.019)	(0.029)
N	15,261	9,806	5,455
R-sq	.191	.205	.185

Notes: Standard errors in parentheses. Regressions include survey weights, state and year fixed effects. The control variables are head of household's age, race, level of education, marital status, family size, self-reported health status, and medical expenses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

On the other hand, Table 3.8 about changes in the intensive margin of labor supply reinforces earlier results. Very poor childless adults increase their

Table 3.8: Changes in Usual hours (70-150% Sample)

	All	Childless	With Children
Below 100% FPL × Non-Exp × Post-ACA	1.06	2.26*	-1.31
	(0.955)	(1.168)	(2.122)
Below 100% FPL × Post-ACA	-1.07	-1.16	-.72
	(0.663)	(0.833)	(1.481)
Below 100% FPL × Non-Exp	-1.3*	-1.55*	-1.31
	(0.757)	(0.926)	(1.542)
Non-Exp × Post-ACA	-.659	-1.74*	1.44
	(0.757)	(0.923)	(1.464)
Below 100% FPL	.301	.113	.994
	(0.540)	(0.672)	(1.090)
N	15,261	9,806	5,455
R-sq	.108	.106	.131

Notes: Standard errors in parentheses. Regressions include survey weights, state and year fixed effects. The control variables are head of household's age, race, level of education, marital status, family size, self-reported health status, and medical expenses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

hours of work by 2.26 hours a week compared to poor childless adults in non-expansion states and the differences between these income groups in expansion states (DIDID estimate); this coefficient is statistically significant at 10 percent. Despite this larger effect, the increase in usual hours worked does not translate into a significant increase in income growth (Table 3.7).

3.6 Conclusion

In this Chapter, I study the labor supply impacts of the recent coverage gap created in 2014 as some states do not expand Medicaid insurance to all non-elderly adults up to 138% FPL. In 2014, very poor households in non-expansion states who expect to earn just below the poverty line now face a new labor supply incentive at the poverty line – critical threshold to be eligible for APTC for health insurance. With a difference-in-differences approach, I compare labor supply responses of very poor households to households just above the poverty line in non-expansion states before and after 2014 and net out the changes in labor supply for similar income groups in expansion states. This work contributes to the previous literature in labor economics by studying the effects of a means-tested program (the APTC) that indirectly increases work incentives rather than disincentives to earnings. Additionally, this work contribute to the health economics fields by evaluating an understudied consequence of the Medicaid expansion policy, the coverage gap non-expansion states. Finally, I exploit this exogenous variation from the households' perspective across income groups, states, and time.

I provide three main findings about the effects of the coverage gap on household labor supply. First, there is no evidence that very poor households increase their income growth as a result of the coverage gap. Such households do not respond to the upward discontinuity in their budget at the poverty line by increasing their earnings. However, to find a minimum detectable effect in my analysis among the never-expander states, very poor households need to have an annual income growth 8% higher than households just above the poverty line. With such data and limited sample size, an overall positive effect on labor supply is difficult to detect and the lack of effect on income growth may be due to low power. An even larger dataset than the CPS ASEC may be needed to significantly rule out the effect of the coverage gap on income growth.

Second, I find a positive and significant impact of the coverage gap at the intensive margin of labor supply. More specifically, childless adults increase their hours usually worked per week by 2 hours in response to the APTC at the poverty line. In fact, non-elderly adults without children are more likely to suffer from the Medicaid coverage gap than other population groups as they are very likely to be ineligible for Medicaid insurance under states' pre-expansion eligibility criteria. Additionally, they tend to have more flexibility in their working hours. Thus, this result is coherent and reflects an important labor supply adjustment to the upward discontinuity in after-tax income, despite not being translated into a significant increase in household income.

Third, there is no evidence of labor supply effects on the extensive margin. I find no evidence that the probability of any household members entering the

labor force, starting to work when previously unemployed, or transitioning from a part-time to a full-time job changes differentially for very poor households in non-expansion states.

To conclude, the lack of health coverage is still important in non-expansion states despite subsidized premiums at the poverty line. Very poor people in non-expansion states are more likely to be uninsured and face higher medical expenses than in expansion states. The uncompensated care generated by the uninsured is a crucial question that future research may want to study. Also, as the coronavirus pandemic starts to reveal its considerable economic consequences, the U.S. unemployment rate surged to 14.5% in April 2020 and more than 20 million people are losing their employer health insurance as a result. A recent report from Kaiser Family Foundation (2020) estimates that up to 1.9 million of low-income childless adults would join the coverage gap by January 2021 as the unemployment benefits expires and the decline in income would make them too poor to qualify for APTC. Thus, the importance of closing the Medicaid coverage gap urgently re-appears with the COVID-19 health and economic crisis.

Chapter 4

A REASSESSMENT OF THE EFFECT OF MEDICAL MARIJUANA LAWS ON OPIOID MISUSE IN LIGHT OF MEDICAID EXPANSION

4.1 Introduction

Drug overdose is the leading cause of deaths from injuries in the United States and opioids, or pain medications, are involved in approximately 70% of the 70,237 overdose deaths in 2017 (Wilson, 2020). From 1999 to 2017, the age-adjusted opioid overdose death rates more than quadrupled, from 2.9 to 14.9 per 100,000 population, with a recent increase in death among middle-aged white male Americans (Case and Deaton (2015); Appendix Figures C.1 and C.2). In 2018, the states with the highest drug overdose death rates were West Virginia (51.5 per 100,000) and Delaware (43.8 per 100,000) (Hedegaard et al., 2020). Recent projections estimate the number of opioid overdose deaths in the U.S. to reach 82,000 a year by 2025 and the opioid overdose crisis to get worse if no public policy change is made (Chen et al., 2019). The opioid epidemic started back in the late 1990s as pharmaceutical companies reassured medical providers about the non-addiction of manufactured opioid drugs (oxycodone, hydrocodone, codeine, morphine, etc.) which resulted in increasing medical prescriptions and misuse of opioid analgesics (Van Zee (2009); Manchikanti et al. (2012)). While the causes

behind the current opioid epidemic are the subject of very large debates, less is known about the impact of Medicaid expansion on opioid-related mortality.

The goal of this Chapter is to revisit a recent study by Powell et al. (2018) on the effects of medical marijuana laws on opioid mortality in light of the Medicaid expansion and examine the robustness of their findings in two ways. First, I replicate their study from 1999 to 2013 by controlling for the "early" Medicaid expansion, a potentially confounding policy. Signed into law in 2010 under the ACA, Medicaid expansion took fully effect in January 2014 at the discretion of states. However, five "early-expander" states and the District of Columbia (DC) implemented the law in 2010 and 2011 via state plan amendment or Section 1115 Waiver.[1] By the end of their study period, Medicaid is expanded to all non-elderly adults earning between 25% to 200% of the poverty line in states representing more than 66 million inhabitants or 20% of the U.S. population (California, Connecticut, Minnesota, New Jersey, Washington, and the District of Columbia). This major coexisting healthcare policy is absent in the original study and controlling for its early phase may increase the robustness of their findings. I find that by 2013 the impact of medical marijuana laws (MML) in reducing opioid death rates is only limited to states with active medical marijuana dispensaries which confirms their findings. In addition, I find that the early Medicaid expansion reduces opioid-related death rates by 25% before 2014.

Second, I extend Powell et al. (2018)'s analysis on opioid-related mortality

[1] States financed their early Medicaid expansion based on their pre-existing state-funded program and federal matching funds at around 50% of the total costs (the Federal Matching Assistance Percentage or FMAP).

to 2018 using the latest available data. To the best of my knowledge, I present the most recent effects of medical marijuana dispensaries and Medicaid expansion on opioid mortality death rates. I find that the strong association of medical marijuana dispensaries with a reduction in opioid-related mortality persists over time. In addition, I provide new findings about the impact of Medicaid expansion on opioid-related mortality. I find no evidence that higher opioid death rates are caused by the ACA Medicaid expansion from 1999 to 2018 despite higher rates in expansion states. The healthcare policy is not related to the increase in opioid death rates over my study period implying that Medicaid expansion does not exacerbate the opioid epidemic. Rather, both medical marijuana dispensaries and Medicaid expansion may be part of the solution in addressing the opioid crisis.

In Section 4.2, I provide a summary of the current literature about the impacts of medical marijuana laws and Medicaid expansion on opioid mortality. In Sections 4.3 and 4.4, I present the data and method used to replicate and extend Powell et al. (2018)'s study. Finally, in Section 4.5, I discuss the replicated results and new findings before concluding in Section 4.6.

4.2 Literature Review and Contributions

In the past two decades, two major policy interventions may largely impact the opioid epidemic: the medical marijuana laws and Medicaid expansion. First, marijuana helps in reducing chronic pain (Piper et al., 2017; Boehnke et al., 2019) and inflammation (Nagarkatti et al., 2009) which supports its medical use for treating pain. As of January 2017, 28 states and DC have passed medical

marijuana laws among which 25 states have active medical marijuana dispensaries. Several recent studies show that medical marijuana laws are associated with reduced opioid-related hospitalizations and lower opioid overdose death rates as marijuana may become a substitute for opioids and pain killer medications (Bachhuber et al., 2014; Pacula et al., 2015; Shi, 2017; Powell et al., 2018).

Second, after the passage of the ACA in 2010 mental health and substance use disorder (SUD) services are one of the ten essential health benefits any insurance plans must cover. As opioid use disorders and chronic pain are more prevalent among the low-income population, Medicaid enrollees may benefit more from covered SUD services (Kaiser Family Foundation, 2019; Dahlhamer et al., 2018). However, the effect of Medicaid expansion on opioid overdose deaths is a priori ambiguous. On the one hand, Medicaid expansion improves the access to lower-cost prescription medications potentially increasing the demand for prescribed opioids which may intensify their misuse and related overdose mortality. On the other hand, Medicaid programs cover medication-assisted treatment (MAT) including opioid treatment programs in SUD services and rehabilitation facilities which reduces opioid addiction and may decrease opioid-related deaths. In a recent study, Meinhofer and Witman (2018) find a 18% differential increase in opioid admissions to MAT facilities in Medicaid expansion states suggesting an increase in utilization of opioid use disorder treatments. So far, few studies have estimated the relationship between Medicaid expansion and opioid overdose mortality and results are mixed. Kravitz-Wirtz et al. (2020) find that at

the county-level Medicaid expansion is associated with lower death rates involving synthetic opioids (-10%) and heroin (-11%), but higher death rates involving methadone (+11%). Yan et al. (2020) find that Medicaid expansion is associated with a reduction in all-cause mortality as soon as drug overdose deaths are excluded and suggest that the opioid epidemic mitigates the mortality benefit of Medicaid expansion. Closer to my work, Averett et al. (2019) find a positive but not significant effect of Medicaid expansion on opioid death rates at the state level over 2010-2017. I contribute to this growing literature by (1) estimating both the impacts of medical marijuana laws/dispensaries and Medicaid expansion on opioid-related mortality; and (2) providing the most recent results up to 2018 at the state-level.

4.3 Data

In this study, I construct a panel dataset from 1999 to 2018 for all 50 states and DC with information on opioid-related mortality rates, medical marijuana laws, and Medicaid expansion status.

I construct opioid-related death rates as my outcome variable using data from the National Vital Statistics System (NVSS) multiple cause-of-death mortality files (MCOD) available from 1999 to 2018 (Centers for Disease Control and Prevention, CDC). To identify the cause of death, I use the CDC International Classification of Disease codes, ICD-10 codes, for deaths due to external causes of injuries: X40–44 (unintentional), X60–64 (suicide), X85 (homicide), or Y10–Y14 (undetermined intent). I use total opioid-related death rates based on the type of opioids involved in the cause of death including opium (T40.0), heroin (T40.1),

natural and semisynthetic opioids (T40.2), methadone (T40.3), and synthetic opioids other than methadone (T40.4).

Some mortality data are considered restricted by the CDC for privacy reasons: death rates are suppressed when the yearly death count per state is below 20, and rates are coded as unreliable when below 9 deaths per 100,000 population. This represents 62 missing state-years with observations from 15 low-population states before 2013.[2] Since the missing values are due to very low death rates, I impute them by matching the smallest value of opioid mortality death rates in the data over the study period (1999-2018) which equals to 0.4 (or equivalent to -.91 when using the natural logarithm of death rates). In that way, I retrieve the sample size of Powell et al. (2018) and preserve the distribution of opioid mortality rate. However, this single imputation may result in biased estimates with underestimated standard errors and p-values.

Medical marijuana dispensaries have been shown to have greater influence on marijuana use than medical marijuana laws per se (Pacula et al. (2015)). Thus, I collect data on active medical dispensaries along with state medical marijuana laws. To do so, I use three main sources: (1) the Prescription Drug Abuse Policy System (PDAPS), a publicly available dataset collecting key provisions of state medical marijuana laws and regulations from 2014 to 2017 (Klieger et al., 2017); (2) original search on webpages from marijuana advocacy groups such as Marijuana Policy Project (MPP) and the National Organization for the Reform of

[2] These missing observations correspond to AK, AR, DC, DE, IA, ME, MS, MT, ND, NE, NH, RI, SD, VT, and WY.

Marijuana Laws (NORML); and (3) state agency websites responsible for regulating these laws. States are indicated as passing medical marijuana laws when criminal penalties are removed for the use of medical marijuana by patients diagnosed with a severe illness.[3] I report state legislation and operational dispensary dates in Table C.1 in the Appendix. As of today, 28 states including DC allow patients to use medical marijuana, and 25 states have active dispensary providing qualifying patients with medical marijuana products.

My variable on active dispensary is based on the first year a medical dispensary has been opened in the state from these three sources of information. It might slightly differ from the definition used in Powell et al. (2018) – they identify states with legal and protected dispensaries if either "(1) the statute or agency rules explicitly allowed for a dispensary, or (2) the statute or agency rules allowed for organization that were functionally equivalent to dispensaries". Prior to 2013, three states are coded differently among their study and mine. Following my above definition of active medical dispensaries, I find Colorado having its first active one in 2005 (2010 in Powell et al. (2018)), Nevada in 2009 (2015 in their Appendix Table A1), and Oregon in 2009 (2014 in their Appendix Table A1).

Finally, I control for variables affecting opioid misuse and potentially correlated with medical marijuana laws and Medicaid expansion. For example, as middle-aged white males are disproportionately more likely to consume opioids,

[3] Diseases and health conditions qualifying patients for medical marijuana use vary by states. However, such conditions are for example, Cancer, Chronic Renal Failure, HIV/AIDS, Multiple Sclerosis, Epilepsy and other seizure disorders, Spasticity disorders, Intractable Pain, Glaucoma, Crohn's Disease, Anorexia, Appetite Loss, Nausea, Cachexia or Wasting Syndrome, PTSD, Traumatic brain injury.

I collect state-year data about the fractions of Whites, of males, and the proportion of the population aged 18-64 and aged 65 and above from the Census Bureau. I also control for economic conditions such as state alcohol beer tax (from Tax Policy Center) and state unemployment rate obtained from Bureau of Labor Statistics' Local Area Unemployment Statistics. In addition, I control for variables affecting the market of opioids such as whether states have regulated pain clinics via "pill mill" laws (Rutkow et al., 2017) and implemented prescription drug monitoring programs (PDMP). PDMP are a promising tool that states implement for medical providers to access patients' prescription histories before making their prescription decisions. While operational in 49 states and often voluntary, there are some evidence that states with mandatory PDMP are associated with a larger reduction in opioid misuse (Grecu et al., 2019). As a potential key factor in the opioid epidemic fight, I collect such control variable using data from the National Alliance for Model State Drug Laws (NAMSDL). This nonprofit organization provides a summary of legislative bills about states that approved mandatory PDMP over the years.

Table 4.1 presents summary statistics from 1999 to 2013 by MML adoption timing and Medicaid expansion using opioid mortality data I collected and Powell et al. (2018)'s covariates from their replication dataset. States that never adopted medical marijuana laws by 2013 had lower opioid death rates. Mandatory access to PDMP was very low by 2013 in all types of states. Only 4 of the 7 states with medical marijuana laws have active and legally protected dispensaries between 2010 and 2013.

Table 4.1: Summary Statistics for Replication work (1999-2013)

	Early MML Adopters by 2009	Late MML Adopters (2010-2013)	Never MML Adopters
Total Opioid Deaths /100,000	5.14	4.66	4.62
Age-Adj. Opioid Deaths /100,000	5.06	4.68	4.68
White	0.78	0.81	0.80
Male	0.50	0.49	0.49
18-64	0.63	0.63	0.62
65+	0.12	0.14	0.13
Unemployment rate	7.04	6.02	6.07
Beer Tax	0.22	0.14	0.29
PDMP Mandatory Access	0.01	0.00	0.02
Pill Mill Laws	0.00	0.00	0.07
Population	19,852,367	6,358,593	11,350,899
Number of states	13	7	31
Number of states with active & protected dispensaries	8	4	
Number of states with active dispensaries	12	11	

Notes: Population weighted. Suppressed death rates imputed by 0.4, the lowest values over the period. Active and protected dispensaries are from Powell et al. replication dataset.

Table 4.2: Summary Statistics for Data Used in Analyses (1999-2018)

	Early MML Adopters by 2009	Late MML Adopters by 2018	Never MML Adopters	Never Medicaid Expanders
Total Opioid Deaths /100,000	6.02	7.48	6.18	6.27
Age-Adj. Opioid Deaths /100,000	5.95	7.62	6.32	6.40
White	0.78	0.79	0.79	0.77
Male	0.50	0.49	0.49	0.49
18-64	0.62	0.62	0.61	0.61
65+	0.13	0.15	0.13	0.13
Unemployment rate	6.71	5.85	5.62	5.67
Beer Tax	0.23	0.21	0.34	0.43
PDMP Mandatory Access	0.11	0.09	0.10	0.06
Pill Mill Laws	0.00	0.11	0.23	0.26
Population	21,463,292	12,096,125	9,831,729	12,411,118
Number of states	13	16	22	18
Number of states with active dispensaries	12	13		1

Notes: Population weighted. Suppressed death rates imputed by 0.4, the lowest values over the period.

Table 4.2 shows summary statistics using my extending dataset up to 2018. Over the full period, opioid death rates are now lower in states that adopted medical marijuana early (6.02 per 100,000 population) and late adopters have higher opioid-related overdose death rates (7.48). Mandatory access to PDMP by medical providers increased by 2018, especially in states that early adopted MML by 2009. In 2018, 22 states have not yet legalized medical marijuana and 18 states have not yet expand Medicaid.

4.4 Method

In this study, I use a similar difference-in-differences identification strategy as in Powell et al. (2018) to compare changes in opioid-related death rates in states passing medical marijuana laws to those not adopting these laws. However, I augment their model by controlling for states expanding Medicaid in some specifications and estimate the following regression:

$$Y_{st} = \beta_0 X_{st} + \beta_1 MML_{st} + \beta_2 Expansion_{st} + \gamma_s + \lambda_t + \epsilon_{st}, \tag{4.1}$$

where Y_{st} is the natural logarithm of age-adjusted opioid mortality death rates in states s and year t. MML_{st} indicates (1) any medical marijuana laws adopted and (2) active medical marijuana dispensary opened in year t. $Expansion_{st}$ is an indicator variable for state expanding Medicaid in year t. X_{st} includes a set of state-level covariates associated with opioid misuse such as unemployment rate, fraction of white, fraction of male, proportion of the population aged 18-64 and aged 65 and more, mandatory access to prescription drug monitoring program

(PDMP), state alcohol beer tax, and whether a state has a pill mill law. Standard errors are clustered at the state level and regressions include state and year fixed effects (γ_s and λ_t) as well as population weights.

4.5 Results

4.5.1 Replicated Results

To begin, I replicate the findings of Powell et al. (2018) about the impact of medical marijuana laws and dispensaries on opioid overdose death rates. There are some differences between my model and their approach – for example, I control for early Medicaid expansion in some specifications, estimate population-weighted regressions, and use publicly available death rates rather than restricted CDC data.

In Table 4.3, I present results when omitting missing death rates (Panel A) and imputing them (Panel B). In both panels, I find that medical dispensaries are associated with a significant 33% or 36% decrease in opioid-related mortality while MML per se are not statistically significant. Thus, I confirm Powell's results that by 2013, medical marijuana dispensaries mainly contribute to the reduction in opioid overdose death rates compared to the simple passage of medical marijuana laws. Controlling for the early Medicaid expansion does not affect the magnitude of Powell et al. (2018)'s coefficients of interest on medical marijuana laws and dispensaries. However, I find that the early Medicaid expansion is associated with a 25% decrease in opioid death rates over the 1999-2013 period, statistically significant at 5%.

Table 4.3: Log Opioid-Related Mortality Rate

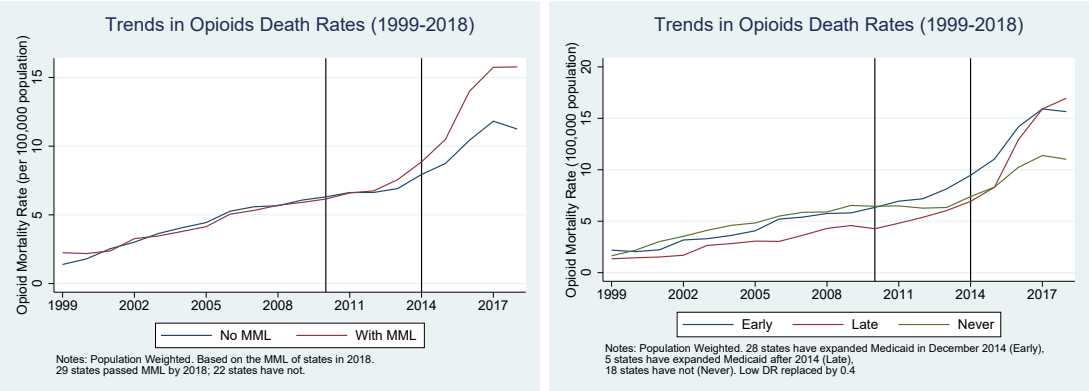
	(1)	(2)	(3)	(4)	(5)	(6)
	1999-2010			1999-2013		
Panel A:						
Missing very low death count						
MML	-0.145	-0.033	-0.034	0.016	0.130	0.129
	(0.151)	(0.168)	(0.168)	(0.140)	(0.138)	(0.122)
Disp.		-0.334***	-0.336***		-0.343***	-0.326***
		(0.115)	(0.114)		(0.118)	(0.109)
Exp			-0.467***			-0.263**
			(0.108)			(0.117)
N	553	553	553	703	703	703
R-sq	0.960	0.961	0.961	0.964	0.966	0.967
Panel B:						
Very low death count imputed						
MML	0.013	0.131	0.130	0.097	0.221	0.221*
	(0.177)	(0.175)	(0.174)	(0.139)	(0.133)	(0.116)
Disp.		-0.375***	-0.376***		-0.377***	-0.359***
		(0.102)	(0.102)		(0.109)	(0.101)
Exp			-0.314			-0.249**
			(0.284)			(0.111)
N	612	612	612	765	765	765
R-sq	0.949	0.951	0.951	0.956	0.958	0.959

Notes: State clustered standard errors in parentheses. The regressions include population weight, state and year fixed effects. Control variables include fraction of white, fraction of male, proportion of the population aged 18-64 and aged 65 and more, state alcohol beer tax, state unemployment rate, prescription drug monitoring program (PDMP), and whether a state has a pill mill law. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.5.2 New Findings up to 2018

To test the parallel trend and identification assumption of my DID model, I plot in Figure 4.1 the population-weighted time trends in opioid mortality rates from 1999 to 2018 by medical marijuana laws (Panel 4.1a) and Medicaid expansion status (Panel 4.1b) Panel 4.1a provides evidence of a parallel upward trend in

Figure 4.1: Trends in Opioid Mortality (1999-2018)



(a) By medical marijuana laws (MML)

(b) By Medicaid expansion

opioid mortality rate before 2013 between states that adopted medical marijuana laws and those that did not. Both types of states have very similar opioid death rates which coincides with the small and insignificant effect of medical marijuana laws on opioid mortality found in Table 4.3. Panel 4.1b shows evidence of a parallel upward trend in opioid-related mortality rates prior to 2014 between Medicaid expansion and non-expansion states. Additionally, there is some evidence for a parallel trend between expanders and never-expanders, prior to 2010. The graphical evidence presented above seems to validate the parallel pre-trends in absence

of each treatment (MML and Medicaid Expansion) and validates the use of a DID. However, future work may want to perform an event study and estimates the impact of such laws on opioid overdose mortality in the years before their passage to confirm that the coefficient estimates are not significant.

Table 4.4: Log Opioid-Related Mortality Rate

	(1) 1999-2014	(2) 1999-2018
Panel A:		
Missing very low death count		
MML	0.319***	0.620***
	(0.112)	(0.127)
Disp.	-0.332***	-0.246**
	(0.112)	(0.095)
Exp	-0.084	0.137
	(0.110)	(0.102)
N	754	958
R-sq	0.967	0.970
Panel B:		
Very low death count imputed		
MML	0.375***	0.642***
	(0.111)	(0.127)
Disp.	-0.351***	-0.256***
	(0.102)	(0.093)
Exp	-0.084	0.140
	(0.107)	(0.102)
N	816	1020
R-sq	0.961	0.967

Notes: State clustered standard errors in parentheses. The regressions include population weight, state and year fixed effects. Control variables include fraction of white, fraction of male, proportion of the population aged 18-64 and aged 65 and more, state alcohol beer tax, state unemployment rate, prescription drug monitoring program (PDMP), and whether a state has a pill mill law. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4.4 presents regression results on the opioid mortality rate when adding the largest Medicaid expansion phase in 2014 to Powell's period study (column 1) and over my entire study period (column 2). I again differentiate whether very low death counts are omitted or imputed among Panels A and B; however, both panels provide very similar results and I do not think that the imputation bias introduced in my study is very severe over 1999-2018.

I find that by 2018 medical marijuana dispensaries are still associated to a reduction in opioid-related mortality rates by a large amount – 25% over the entire period and statistically significant at 1%. The persistence of this negative relationship between dispensaries and opioid mortality strengthens recent findings in the related literature and suggest that medical marijuana dispensaries might be one solution to the opioid epidemic fight. However, to provide causal effects, an exogenous variation in the enactment of medical marijuana dispensary will be necessary. In this study, I only present correlation results between MML or dispensaries and opioid mortality rates. The coefficient estimate on MML per se is however positive and highly significant by 2018 which may cancel out the mortality reduction associated with dispensaries. However, it is important to note that having passed MML is a necessary condition for states to have active dispensaries. According to the timing of both policies (Table C.1 in the Appendix), states allow medical marijuana dispensaries to open three years on average after the passage of the state MML. Thus, the coefficient estimate on dispensary may instead capture long-term correlations of MML with opioid mortality, and may provide a more precise relationship. This would resonate with previous literature

that insist on the greater role of dispensaries compared to the simple passage of MML (Pacula et al., 2015).[4]

In addition, I find that Medicaid expansion is not causing opioid mortality rates as my coefficient estimates are not significant. Although my coefficients are positive by 2018, there is no statistically significant evidence that Medicaid expansion exacerbates the opioid epidemic which reinforces Averett et al. (2019)'s initial results. This is an important result as several states have not yet expanded Medicaid and may defer it if its expansion was intensifying the opioid epidemic which the results above do not support.

Several limitations and future research directions emerge from my study. First, using individual-level data may allow researchers to estimate more precisely the causal effects of MML and Medicaid expansion on opioid deaths by controlling for individual characteristics predictors of opioid uses (e.g., gender, age, race, education, income, and insurance). It may also be valuable to restrict the analysis to Medicaid enrollees as they have been found disproportionately affected by drug overdose in previous literature. Individual-level data may then allow researchers to provide more conclusive evidence about the possible substitution behavior of opioid users toward marijuana for managing chronic pain and reducing opioid addiction.

Second, one may want to differentiate the results by class of opioids and

[4] In states with MML and no dispensaries, medical marijuana patients have one main and legal source to access medical cannabis which is the Office of Medical Marijuana and their medical compassion centers strictly regulated by the state Department of Health and Social Services.

distinguish the effects of MML and Medicaid expansion between prescribed opioids, heroin, and synthetic opioids including illegally manufactured fentanyl which became the most common causes of overdose deaths in the past few years. In addition, methadone is one of the synthetic opioids used to treat severe opioid use disorders and addiction. Future research may want to exclude methadone from their total opioid deaths even though the number of deaths from such opioids is very low and may not change drastically the results.

Third, Medicaid coverage of opioid use disorder treatments and the MAT coverage widely vary geographically. Some states cover both buprenorphine and methadone to reduce opioid addiction while other states do not cover methadone treatments despite their proven cost-effectiveness. Thus, it may be interesting to take into account the heterogeneity in state Medicaid insurance programs and differentiate results by state Medicaid coverage.

Finally, in this study, I do not evaluate the supply side of opioids. Despite a successful effort in decreasing the supply of legally prescribing opioids to 51.4 prescriptions by 100 U.S. residents in 2018, its lowest level since 2005, large disparities exist across states with, for example, prescription rates above 100 in Arkansas and Alabama. Thus, assessing the impact of opioid distribution on its misuse and abuse may provide relevant insights. Although measuring the supply of illegal opioids is a recurrent challenge, data on the legal supply of drugs to dispensers and medical providers can still be worth exploring and are available from the Drug Enforcement Administration's Automation of Reports and Consolidated Orders System (ARCOS).

4.6 Conclusion

In this study, I reevaluate previous findings on the effects of medical marijuana laws (MML) on opioid-related mortality rates by controlling for Medicaid expansion. I further expend the analysis to 2018 with the most recent available data and provide three main findings. First, I find that medical marijuana dispensaries are associated with a 33% reduction in opioid-related death rates prior to 2013. However, MML per se are not related to opioid mortality rates confirming previous findings on the larger role played by medical dispensaries. In addition, controlling for early Medicaid expansion does not affect the magnitude of Powell et al. (2018)'s results; however, it allows to find that from 1999 to 2013 opioid mortality rates decrease by 21% in early Medicaid expansion compared to non-expansion states.

Second, I find a strong and persistent relationship of medical marijuana dispensaries with reducing opioid overdose deaths when extending the analysis until 2018. Thus, legalizing medical marijuana dispensaries in states with already MML but not only may be part of the solution to curb the opioid epidemic. However, more research is needed to provide causal effects and estimate at the individual level whether (1) opioid users are effectively substituting marijuana for opioids, and (2) marijuana is a medically safe alternative to opioids in managing chronic pain.

Third, I find no evidence that higher opioid overdose death rates are caused by the ACA Medicaid expansion despite reducing the cost of prescription medications including prescribed opioids. Studies at the individual level may provide

more insights about the key role that health insurance may play in addressing the opioid epidemic, notably as some non-expansion states experience higher rate of opioid abuse (Abraham et al., 2017).

Finally, concurrent with the rise in opioid-related mortality, more states are passing or extending MML. The main reasons raised by legislatures for passing state MML are (1) medical, with marijuana being proved to ease chronic pain, (2) political, as medical marijuana receives a large popular support by voters,[5] and (3) economic, from increased tax revenue on marijuana sales.[6] However, as the opioid epidemic gets worse states may decide to pass such laws as to offer a plausible alternative to opioids. In that case, a reverse causality problem might emerge making it difficult to infer causal effects of such laws on opioid harm reduction. Additionally, the endogeneity of states deciding to pass MML may cast some doubts on the validity of difference-in-differences models to study MML and their impacts on opioid misuse. Future research may want to either look for instrument variables predicting the passage of MML/dispensaries independently of states' opioid misuse level or exploit an exogenous variation in these policies if any.

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[5] 17 states have legalized medical marijuana by ballot initiatives (NCS, 2020)

[6] Colorado generated $1 billion in annual tax revenue in 2018 from sales taxes on recreational marijuana products (Davis et al., 2019)

Chapter 5

CONCLUSIONS AND FUTURE RESEARCH DIRECTIONS

This dissertation evaluates two main mandates of the Affordable Care Act (ACA) and their effects on labor supply and health outcomes.

Advance premium tax credits (APTC), or subsidy, are offered on the ACA Marketplace to reduce the cost of individual private health insurance. In Chapter 2, I study the labor supply impacts of APTC. The subsidy design falls sharply to zero right above 400% FPL and creates a "*potential lost subsidy*"(PLS) for households near this threshold. The PLS is defined as the subsidy a family would be eligible for if their annual income were *exactly* 400% FPL. As a result, *some* households would significantly benefit from earning just below 400% FPL, rather than just above it. Thus, in Chapter 2, I study whether households near 400% FPL (300%-500%) and not offered ESI reduce their income and labor supply at both the intensive and extensive margins due to a large PLS. Using data from the Medical Expenditure Panel Survey (MEPS) and premium data from the Robert Wood Johnson Foundation (RWJF) over 2010-2017, I estimate that on average, the PLS corresponds to $100 a month for younger workers and reaches $400 to $600 a month for older workers. Additionally, the PLS greatly varies by geographic location and family composition. In my identification strategy, I estimate the exogenous variation in the PLS (from household size, age, and

geographic location) that potentially impacts household labor supply. I find that middle-income households near the threshold and not offered ESI do not adjust their income and labor supply at both the intensive and extensive margins in response to an increase in their PLS. Thus, the ACA does not create income manipulation or labor supply distortion. This study contributes to the growing literature on the labor supply impacts of the ACA by focusing on the consequences of an understudied and unique means-tested program for moderate- and middle-income households.

In Chapter 3, I focus on the lower cutoff of the ACA subsidy design and study the exogenous variation perceived by very poor households in states that do not expand Medicaid in 2014. Households that expect to earn just below the poverty line in non-expansion states experience an unprecedented labor supply incentive at the poverty line and an upward discontinuity in their after-tax income. Using CPS-ASEC data from 2010 to 2018 and a difference-in-differences method, I estimate the coverage gap impacts within households on income growth, change in hours of work, and change in employment status and labor force participation. I find that very poor households do not increase or manipulate their income to reach the APTC income-eligibility threshold. However, childless adults increase their usual weekly hours significantly (i.e., by 2 hours a week) which implies a large labor supply adjustment on the intensive margin. On the other hand, I find no significant effect on the extensive margin of household labor supply. As of today, 12 states have still a coverage gap and the risks of losing employer-based coverage or suffering from an adverse income shock are getting higher for low

income people. Thus, health insurance is getting very fragile for some Americans and targeted policy interventions are needed to close the Medicaid coverage gap and provide affordable health insurance to low-income people.

Lastly, in Chapter 4, I reevaluate a recent study from Powell et al. (2018) on the effects of medical marijuana laws on opioids addiction, recently declared as a public health emergency. Using state-year panel data from the 1999-2018 multiple cause-of-death mortality (MCOD) from the National Vital Statistics System (NVSS), I find that early Medicaid expansion and medical marijuana dispensaries mainly are associated with a 25% and 33% reduction in opioid-related death rates, respectively. In addition, the effects of medical marijuana dispensaries persist up to 2018. However, as more states expand Medicaid, this ACA provision is not anymore related to opioid overdose mortality rates implying that Medicaid expansion does not intensify the opioid epidemic. Rather, both medical marijuana dispensaries and Medicaid expansion may be part of the solution in addressing the opioid crisis. Thus, future research may want to study at the individual level how health insurance can potentially address the rising opioid mortality rates.

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Appendix A

SUPPLEMENTARY INFORMATION FOR CHAPTER 2

Figure A.1: IRB/Human subjects approval

RESEARCH OFFICE

210 Hullihen Hall
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DATE:	April 12, 2018
TO:	Tiphanie Magne
FROM:	University of Delaware IRB
STUDY TITLE:	[1201575-1] The effects of the Affordable Care Act premium tax-credits on US labor markets.
SUBMISSION TYPE:	New Project
ACTION:	DETERMINATION OF EXEMPT STATUS
DECISION DATE:	April 12, 2018
REVIEW CATEGORY:	Exemption category # *(4)*

Thank you for your submission of New Project materials for this research study. The University of Delaware IRB has determined this project is EXEMPT FROM IRB REVIEW according to federal regulations.

We will put a copy of this correspondence on file in our office. Please remember to notify us if you make any substantial changes to the project.

If you have any questions, please contact Nicole Farnese-McFarlane at (302) 831-1119 or nicolefm@udel.edu. Please include your study title and reference number in all correspondence with this office.

Table A.1: Federal Standard Age Curve

AGE	**Premium Ratio**	**AGE**	**Premium Ratio**
0-14	0.765	40	1.278
15	0.833	41	1.302
16	0.859	42	1.325
17	0.885	43	1.357
18	0.913	44	1.397
19	0.941	45	1.444
20	0.970	46	1.500
21	1.000	47	1.563
22	1.000	48	1.635
23	1.000	49	1.706
24	1.000	50	1.786
25	1.004	51	1.865
26	1.024	52	1.952
27	1.048	53	2.040
28	1.087	54	2.135
29	1.119	55	2.230
30	1.135	56	2.333
31	1.159	57	2.437
32	1.183	58	2.548
33	1.198	59	2.603
34	1.214	60	2.714
35	1.222	61	2.810
36	1.230	62	2.873
37	1.238	63	2.952
38	1.246	64	3.000
39	1.262		

Source: CMS Insurance Standard Bulletin Series, 2016

Table A.2: Income Growth Regression: socio-economic variables

	(1) Post	(2) Not ESI	(3) Full
Age	-.0009251	-.009133**	-.001368
	(.0024)	(.0045)	(.0019)
Age squared	-.0000108	.0000912*	-5.05e-06
	(.000029)	(.000055)	(.000023)
Female	-.01667**	-.03207**	-.02085***
	(.0072)	(.014)	(.0056)
High school	-.004368	.002396	-.01873
	(.015)	(.031)	(.014)
Some college	.02785*	.04284*	.0144
	(.015)	(.026)	(.014)
College	.07238***	.05109**	.04934***
	(.012)	(.024)	(.011)
Black	-.02441***	-.05425***	-.0285***
	(.0079)	(.015)	(.0067)
Other race	.03753	.06415	.03383*
	(.024)	(.042)	(.02)
Log of medical expenses	.003776***	.004056**	.003094***
in year 1	(.0011)	(.0019)	(.00081)
Self-employed in year 1	.003635	.06639***	.005882
	(.017)	(.019)	(.012)
Married	-.01128***	-.02241***	-.01098***
	(.0028)	(.0038)	(.0021)
Family size	-.01375***	-.02109***	-.01494***
	(.0029)	(.0041)	(.0021)
Very good health	-.009487	-.008744	-.01363*
	(.0089)	(.016)	(.0073)
Good health	-.03709***	-.0549***	-.03872***
	(.0095)	(.015)	(.0076)
Fair health	-.06436***	-.06568***	-.07023***
	(.015)	(.024)	(.011)
Poor health	-.05032**	-.09239*	-.08266***
	(.024)	(.053)	(.017)
constant	.01339	.2044**	.01773
	(.06)	(.095)	(.052)
N	18,905	7,572	33,412
R^2	.06844	.08252	.06261

Notes: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.3: Change in hours Regression: socio-economic variables

	(1) Post	(2) Not ESI	(3) Full
Age	.2734***	-.0954	.2561***
	(.094)	(.14)	(.067)
Age squared	-.004271***	.0006911	-.003904***
	(.0011)	(.0018)	(.0008)
Female	-.6069***	-.5476	-.5674***
	(.23)	(.4)	(.17)
High school	.4383	.4163	.3596
	(.43)	(.76)	(.41)
Some college	.1005	.03247	.07572
	(.51)	(1.3)	(.49)
College	.2617	-.4076	.04438
	(.44)	(.96)	(.35)
Black	-.04512	-1.06**	-.05745
	(.27)	(.48)	(.21)
Other race	-.865	-2.971	-.12
	(1.3)	(3.1)	(.88)
Log of medical expenses	-.2436***	-.1591**	-.2394***
in year 1	(.04)	(.07)	(.034)
Self-employed in year 1	1.851***	-.1777	1.417***
	(.33)	(.56)	(.25)
Married	.2561***	.04376	.2915***
	(.083)	(.13)	(.06)
Family size	-.1005	-.2435*	-.03664
	(.098)	(.14)	(.071)
Very good health	-.1854	-.5075	-.2514
	(.27)	(.49)	(.2)
Good health	-.3723	-.7958	-.423**
	(.3)	(.5)	(.21)
Fair health	-1.965***	-3.075***	-2.407***
	(.38)	(.83)	(.32)
Poor health	-5.331***	-2.705*	-4.712***
	(.59)	(1.5)	(.41)
constant	9.15***	4.299	9.313***
	(2.1)	(3.3)	(1.6)
N	18,905	7,572	33,412
R^2	.1414	.03083	.1388

Notes: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.4: Probability that any household member stops working: socio-economic variables

	(1) Post	(2) Not ESI	(3) Full
Age	-.007608***	-.01454***	-.007458***
	(.0018)	(.0036)	(.0013)
Age squared	.0000778***	.000155***	.0000744***
	(.000021)	(.000045)	(.000016)
Female	-.0182***	-.01548	-.025***
	(.0046)	(.01)	(.0036)
High school	-.001002	-.02523	-.0003483
	(.)	(.021)	(.0094)
Some college	.01761*	.008426	.01868*
	(.01)	(.025)	(.011)
College	-.007211	-.001558	-.005223
	(.008)	(.019)	(.0073)
Black	-.002651	.008835	-.0002665
	(.0065)	(.013)	(.0046)
Other race	.04378*	.1007*	.04998***
	(.025)	(.055)	(.017)
Log of medical expenses	.001046	.003782**	.0006859
in year 1	(.00079)	(.0018)	(.00065)
Self-employed in year 1	.007525	-.02898	.01148*
	(.0098)	(.018)	(.0067)
Married	-.01752***	-.01749***	-.01768***
	(.0019)	(.0041)	(.0014)
Family size	.002344	.003747	.001284
	(.0018)	(.0036)	(.0014)
Very good health	.006724	-.004279	.004821
	(.0065)	(.013)	(.0045)
Good health	.01002	.008373	.01233**
	(.0071)	(.014)	(.0055)
Fair health	.009122	.09188***	.01834**
	(.0095)	(.025)	(.0081)
Poor health	.01163	.09357**	.00404
	(.015)	(.046)	(.011)
N	18,905	7,572	33,412
Year FE	X	X	X

Marginal effects; Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.5: Probability to be below 400% in Y2: socio-economic variables

	(1) Near & Not ESI & Post	(2) Near & Not ESI
Age	-.03971**	-.0248*
	(.017)	(.013)
Age squared	.0005035**	.0003146**
	(.0002)	(.00016)
Female	.01963	.05681
	(.052)	(.04)
High school	-.05514	-.009902
	(.067)	(.075)
Some college	-.1469	-.1234
	(.11)	(.1)
College	-.08416	-.05583
	(.068)	(.063)
Black	.09888	.05512
	(.073)	(.054)
Other race	.05844	.04565
	(.11)	(.085)
Log of medical expenses	-.01665**	-.01413**
in year 1	(.0078)	(.006)
Self-employed in year 1	-.07561	-.07787
	(.068)	(.052)
Married	.04136***	.02854**
	(.015)	(.013)
Family size	.01793	.01925
	(.021)	(.018)
Very good health	-.04062	-.02987
	(.063)	(.047)
Good health	.03566	.0152
	(.071)	(.054)
Fair health	-.0814	.01255
	(.092)	(.071)
Poor health	.2836***	.0706
	(.1)	(.16)
N	685	1,157

Marginal effects; Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix B

SUPPLEMENTARY INFORMATION FOR CHAPTER 3

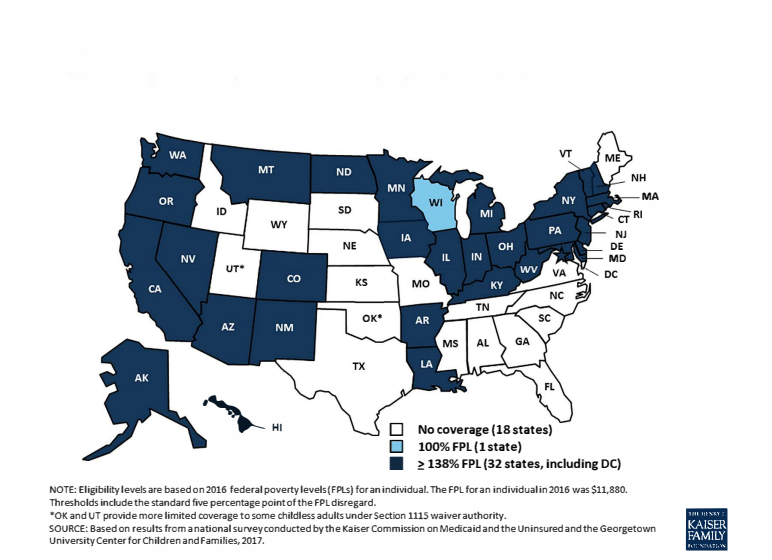


Figure B.1: Map of the non-expansion states by January, 2017

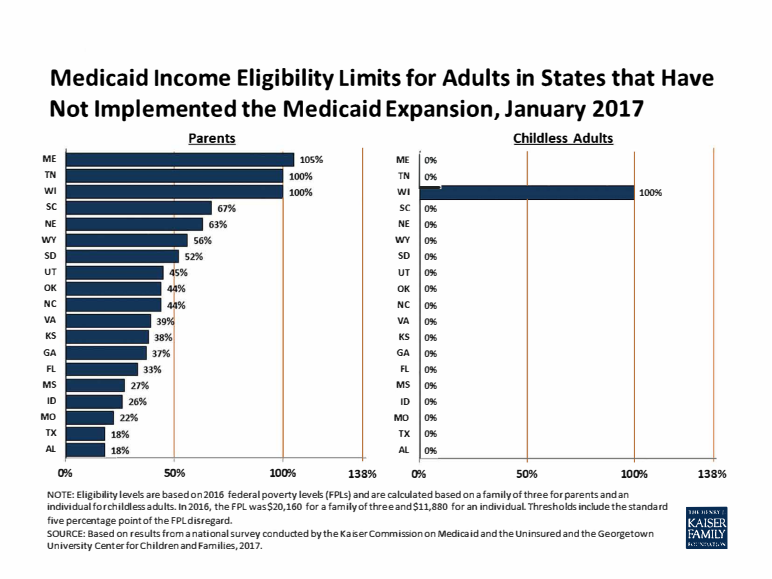


Figure B.2: Medicaid Eligibility Thresholds in Non Expansion states

Figure B.3: Percentage of nonelderly population enrolled in ESI coverage, 1998-2018

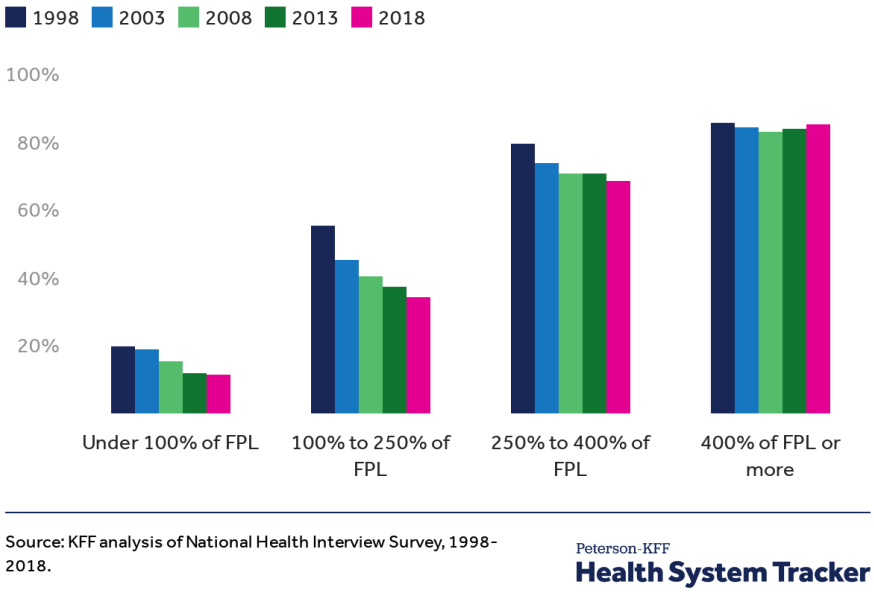


Table B.1: Income Growth for the Never Expanders (70-150% Sample)

	All	Childless	With Children
Below 100% × Post-ACA	-.0169	-.0339	.0225
	(0.027)	(0.034)	(0.044)
Below 100%	.0985***	.1***	.0968**
	(0.021)	(0.026)	(0.039)
Age	-.0017	-.00423	.00617
	(0.003)	(0.003)	(0.006)
Age^2	.0000508	.0000794**	-.0000415
	(0.000)	(0.000)	(0.000)
Female	-.077***	-.0678***	-.103***
	(0.017)	(0.019)	(0.036)

High School	.0593***	.051***	.0647**
	(0.016)	(0.018)	(0.029)
Some College	.0816***	.0681***	.0846***
	(0.021)	(0.024)	(0.032)
College Grad	.14***	.0961***	.243***
	(0.029)	(0.035)	(0.043)
Black	-.0209	-.043**	.0257
	(0.017)	(0.022)	(0.030)
Other Race	-.0491	-.00659	-.107**
	(0.037)	(0.048)	(0.053)
Self-Employed Y1	-.0305	-.0654	.0178
	(0.033)	(0.050)	(0.038)
Employed Y1	.162***	.189***	.083**
	(0.021)	(0.027)	(0.036)
Unemployed Y1	-.0406	-.0452	-.0728
	(0.036)	(0.042)	(0.070)
Log Med spend Y1	.00476	.00309	.00516
	(0.003)	(0.004)	(0.005)
Married	.11***	.125***	.0756**
	(0.023)	(0.039)	(0.038)
Family Size	-.0468***	-.0491	-.0362***
	(0.008)	(0.033)	(0.011)
Very good health Y1	-.00817	.0203	-.0501
	(0.022)	(0.031)	(0.034)
Good health Y1	-.0335	-.0103	-.0631*

	(0.023)	(0.031)	(0.033)
Fair health Y1	-.0213	-.0073	-.0178
	(0.027)	(0.037)	(0.045)
Poor health Y1	-.0336	-.00659	-.0927
	(0.031)	(0.038)	(0.068)
Very good health Y2	.00995	.0101	.00297
	(0.021)	(0.030)	(0.033)
Good health Y2	-.00868	.00409	-.0417
	(0.023)	(0.032)	(0.035)
Fair health Y2	-.0835***	-.0757**	-.107**
	(0.029)	(0.039)	(0.042)
Poor health Y2	-.123***	-.127***	-.0791
	(0.032)	(0.038)	(0.071)
Unemployment Rate	.00465	.00925*	-.00678
	(0.005)	(0.005)	(0.009)
N	5,307	3,372	1,935
R-sq	.199	.225	.168

Standard errors in parentheses
Year fixed effects are included.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.2: Changes in Usual hours at the Household Level in Never Expander States (70-150% Sample)

	All	Childless	With Children

Below 100% FPL × Post-ACA	0.551	1.671*	-0.902
	(0.844)	(0.994)	(1.699)
Below 100% FPL	-1.309**	-1.665**	-1.405
	(0.655)	(0.777)	(1.331)
Age	0.258**	0.256**	0.381
	(0.106)	(0.120)	(0.234)
Age^2	-0.004***	-0.004***	-0.006*
	(0.001)	(0.001)	(0.003)
Female	0.177	0.218	0.352
	(0.488)	(0.522)	(1.301)
High School	-0.113	-1.050	1.622
	(0.579)	(0.667)	(1.059)
Some College	0.521	0.362	0.527
	(0.608)	(0.690)	(1.159)
College Grad	-0.382	-0.591	-0.873
	(0.975)	(1.188)	(1.966)
Black	0.478	1.004	-0.463
	(0.521)	(0.720)	(0.940)
Other Race	-0.780	1.348	-3.838*
	(1.110)	(1.170)	(2.136)
Self-Employed Y1	0.404	2.004	-1.887
	(1.540)	(1.791)	(2.533)
Employed Y1	-6.201***	-5.316***	-9.504***
	(0.697)	(0.857)	(1.292)
Unemployed Y1	13.955***	12.234***	14.692***

	(1.326)	(1.737)	(2.329)
Log Med Spend Y1	0.016	0.031	-0.122
	(0.091)	(0.104)	(0.199)
Married	1.239	1.796	-0.189
	(0.775)	(1.255)	(1.404)
Family size	-0.366	-0.279	0.590
	(0.300)	(1.040)	(0.542)
Very good health Y1	0.878	2.073*	-0.689
	(0.855)	(1.148)	(1.275)
Good health Y1	1.190	1.836*	0.310
	(0.808)	(1.116)	(1.217)
Fair health Y1	0.524	1.148	-0.016
	(0.934)	(1.164)	(2.140)
Poor health Y1	0.848	1.837	-2.869
	(1.046)	(1.273)	(2.547)
Very good health Y2	-0.532	-0.363	-1.083
	(0.845)	(1.090)	(1.158)
Good health Y2	-2.483***	-3.187***	-1.777
	(0.810)	(1.133)	(1.214)
Fair health Y2	-4.200***	-4.361***	-4.321***
	(0.906)	(1.157)	(1.534)
Poor health Y2	-4.716***	-4.676***	-8.242***
	(0.934)	(1.169)	(1.967)
Unemployment Rate	0.206	0.192	-0.026
	(0.162)	(0.173)	(0.340)

N	5,307	3,372	1,935
R-sq	0.103	0.098	0.143

Notes: Standard errors in parentheses, year fixed effects included. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.3: Probability of Changes in Employment in the Never Expanders (70-150% Sample)

	Enter	Start Work	PT to FT
Below 100% FPL × Post-ACA (d)	.00121	-.00398	-.0137
	(0.011)	(0.008)	(0.013)
Below 100% FPL (d)	.0102	.00418	.0162
	(0.009)	(0.006)	(0.011)
Post-ACA (d)	-.0124	-.012*	-.00501
	(0.008)	(0.006)	(0.010)
Age	-.0055***	-.00418***	-.00468***
	(0.001)	(0.001)	(0.001)
Age^2	.0000584***	.0000434***	.0000306**
	(0.000)	(0.000)	(0.000)
Female (d)	-.0133**	-.0126***	.00314
	(0.006)	(0.005)	(0.008)
High School (d)	-.00215	-.00567	.00161
	(0.007)	(0.005)	(0.010)
Some College (d)	.00556	-.00751	.0121
	(0.008)	(0.005)	(0.011)

College (d)	.00253	-.00806	-.00228
	(0.011)	(0.007)	(0.014)
Black (d)	.00147	.00963	.00477
	(0.007)	(0.006)	(0.009)
Other Race (d)	-.0117	.00143	.00823
	(0.010)	(0.010)	(0.016)
Self-Employed Y1 (d)	-.0173**	.000555	.0674***
	(0.008)	(0.009)	(0.020)
Log Med Spend Y1	-.00258**	-.00198**	-.00142
	(0.001)	(0.001)	(0.001)
Married (d)	.0581***	.00657	.0219*
	(0.012)	(0.007)	(0.012)
Family Size	.00155	.000946	-.00897***
	(0.002)	(0.002)	(0.003)
Very Good health Y1 (d)	.000702	.000434	-.0159*
	(0.008)	(0.006)	(0.009)
Good health Y1 (d)	-.00636	.00459	-.00306
	(0.008)	(0.006)	(0.010)
Fair health Y1 (d)	.0252*	-.0103	-.0185
	(0.013)	(0.007)	(0.012)
Poor health Y1 (d)	.0237	-.0141*	-.0438***
	(0.019)	(0.009)	(0.013)
Very Good health Y2 (d)	-.00335	.00672	.0156
	(0.007)	(0.007)	(0.010)
Good health Y2 (d)	-.00979	.00617	-.0147

	(0.007)	(0.007)	(0.010)
Fair health Y2 (d)	-.0266***	-.00178	-.043***
	(0.007)	(0.008)	(0.010)
Poor health Y2 (d)	-.042***	-.0296***	-.0443***
	(0.005)	(0.005)	(0.012)
Unemployment Rate	-.00242	.000186	-.00304
	(0.002)	(0.001)	(0.002)
N	5,307	5,307	5,307

Notes: Standard errors in parentheses. Regressions include survey weights, and year fixed effects. The control variables are head of household's age, race, level of education, marital status, family size, self-reported health status, and medical expenses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.4: Marginal Effects of Changes in Employment Status (Below FPL Sample)

	Enter	Start Work	PT to FT
Non-Exp × Post-ACA (d)	.00539	.000811	-.00216
	(0.008)	(0.007)	(0.009)
Post-ACA (d)	.0114	-.00604	.00554
	(0.009)	(0.007)	(0.010)
Age	-.00237	-.00113	.00357
	(0.002)	(0.001)	(0.002)
Age2	.0000221	7.40e-06	-.0000607**
	(0.000)	(0.000)	(0.000)
Female (d)	-.0143**	-.00101	.00943

	(0.006)	(0.004)	(0.007)
High School (d)	.0122	.00303	-.00169
	(0.009)	(0.006)	(0.010)
Some College (d)	.0335***	.000923	.0113
	(0.011)	(0.006)	(0.012)
College (d)	.0204	.00497	.0271
	(0.017)	(0.011)	(0.020)
Black (d)	-.00645	.00484	.001
	(0.007)	(0.006)	(0.009)
Other race (d)	.0285	.00237	.0153
	(0.022)	(0.008)	(0.016)
Self-Employed Y1 (d)	-.0343***	-.00697	.0536**
	(0.005)	(0.006)	(0.021)
Log Med Spend Y1	.0000642	-.000762	-.000326
	(0.001)	(0.001)	(0.001)
Married (d)	.0381***	.0134	.027
	(0.014)	(0.010)	(0.017)
Family Size	.00178	.00241	-.00299
	(0.003)	(0.002)	(0.004)
Very good health Y1 (d)	.00678	-.00449	-.0118
	(0.010)	(0.006)	(0.010)
Good health Y1 (d)	.0154	.00439	-.026***
	(0.012)	(0.007)	(0.010)
Fair health Y1 (d)	.0515**	-.00218	-.0326***
	(0.021)	(0.008)	(0.011)

Poor health Y1 (d)	.0353	-.0157**	-.0407***
	(0.025)	(0.006)	(0.011)
Very Good health Y2 (d)	-.00114	-.00515	.0235*
	(0.009)	(0.005)	(0.013)
Good health Y2 (d)	-.0105	-.00998**	-.00773
	(0.008)	(0.005)	(0.010)
Fair health Y2 (d)	-.032***	-.015***	-.0463***
	(0.009)	(0.006)	(0.011)
Poor health Y2 (d)	-.0479***	-.0266***	-.0456***
	(0.006)	(0.005)	(0.009)
Unemployment Rate	.00412**	.00141	.0021
	(0.002)	(0.001)	(0.002)
N	5897	5897	5897

Notes: Marginal effects; Standard errors in parentheses. State and year fixed effects are not included. Survey weights discrete change of dummy variable from 0 to 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.5: The impacts of the Coverage Gap on changes in usual hours of work (70-150% Sample)

	All	Childless	With Children
Below 100 % × Non-Exp × Post-ACA	1.06	2.26*	-1.31
	(0.955)	(1.168)	(2.122)
Below 100 % × Post-ACA	-1.07	-1.16	-.72
	(0.663)	(0.833)	(1.481)
Below 100 % × Non-Exp	-1.3*	-1.55*	-1.31

	(0.757)	(0.926)	(1.542)
Non-Exp × Post-ACA	-.659	-1.74*	1.44
	(0.757)	(0.923)	(1.464)
Below 100 %	.301	.113	.994
	(0.540)	(0.672)	(1.090)
Age	.173**	.199**	.102
	(0.078)	(0.082)	(0.171)
Age^2	-.00297***	-.00329***	-.00154
	(0.001)	(0.001)	(0.002)
Female	.292	.458	-.532
	(0.326)	(0.363)	(0.940)
High School	-.0255	-.242	.725
	(0.359)	(0.432)	(0.707)
Some College	.753**	.746*	1.03
	(0.357)	(0.410)	(0.738)
College Grad	.878	.84	1.19
	(0.555)	(0.679)	(0.994)
Black	-.155	.238	-.741
	(0.389)	(0.484)	(0.753)
Other Race	.73	1.79***	-.941
	(0.541)	(0.639)	(0.951)
Self-Employed Y1	.0429	-.305	.542
	(0.843)	(1.111)	(1.399)
Employed Y1	-6.83***	-6.19***	-9.49***
	(0.434)	(0.529)	(0.709)

Unemployed Y1	12.1***	11.5***	11.4***
	(0.846)	(1.055)	(1.435)
Log Med Spend Y1	.148***	.16***	.0922
	(0.054)	(0.060)	(0.114)
Married	-.0492	-.0167	-.618
	(0.498)	(0.926)	(0.978)
Family Size	.183	-.0373	.231
	(0.173)	(0.786)	(0.294)
Very good health Y1	.339	.698	-.345
	(0.576)	(0.677)	(0.865)
Good health Y1	.536	.525	.646
	(0.498)	(0.639)	(0.794)
Fair health Y1	.743	.547	1.45
	(0.555)	(0.706)	(1.024)
Poor health Y1	.904	.972	-.389
	(0.604)	(0.717)	(1.339)
Very good health Y2	-.331	-.101	-.715
	(0.540)	(0.631)	(0.753)
Good health Y2	-2.17***	-2.53***	-1.53*
	(0.521)	(0.626)	(0.832)
Fair health Y2	-4.32***	-3.9***	-5.67***
	(0.566)	(0.667)	(0.981)
Poor health Y2	-5.19***	-4.66***	-9.22***
	(0.589)	(0.675)	(1.191)
Unemployment Rate	.268*	.325**	.246

	(0.146)	(0.165)	(0.284)
N	15,261	9,806	5,455
R-sq	.108	.106	.131

Standard errors in parentheses. Survey weights, and state and year fixed effects are included.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix C

SUPPLEMENTARY INFORMATION FOR CHAPTER 4

Figure C.1: Opioid Overdose Deaths by Gender

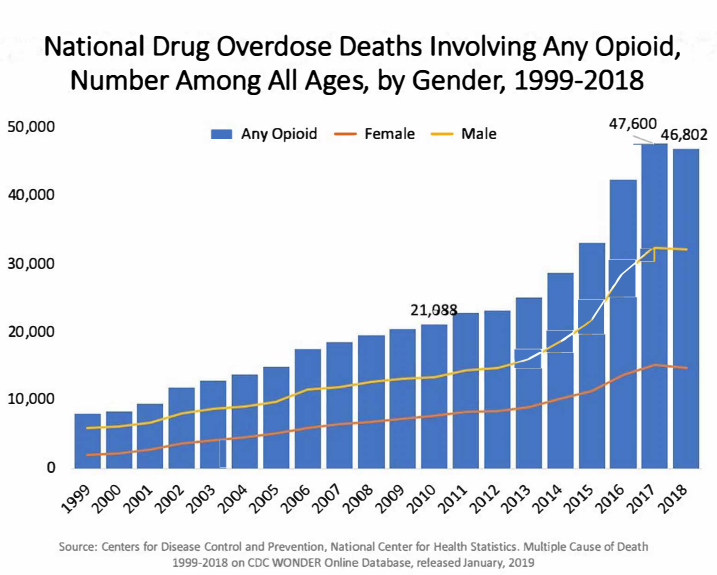
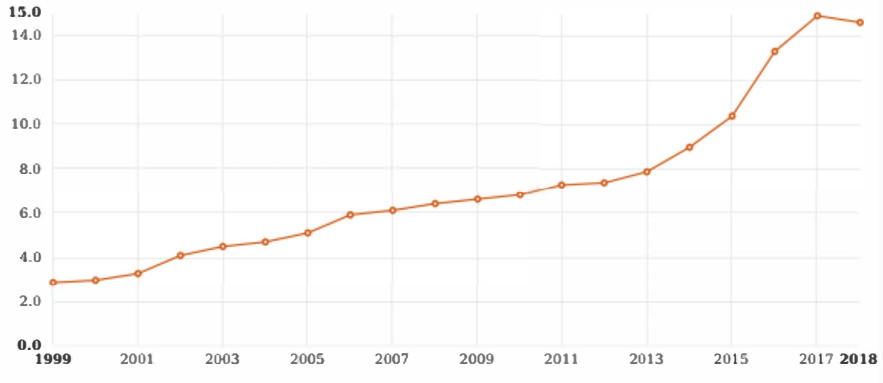


Figure C.2: Opioid Overdose Death Rates per 100,000 Population (Age-Adjusted)



Source: Kaiser Family Foundation's State Health Facts

Table C.1: Medical Marijuana Laws and Medicaid Expansion Status Timeline

State	Medical MJ Effective Year	Active Medical MJ Dispensary	Medicaid Expansion Year
Alaska	1999		2015
Arizona	2011	2012	2014
Arkansas	2016	2016	2014
California	1997	2004	2011
Colorado	2001	2005	2014
Connecticut	2013	2014	2010
Delaware	2012	2014	2014
District Of Columbia	2011	2013	2010
Florida	2017		
Hawaii	2001	2015	2014
Illinois	2014	2014	2014
Indiana			2015
Iowa			2014
Kentucky			2014
Louisiana			2016
Maine	2000	2011	2019
Maryland*	2003	2017	2014
Massachusetts	2013	2018	2014
Michigan	2009	2009	2014
Minnesota	2014	2015	2011
Montana	2005	2009	2016
Nevada	2002	2009	2014
New Hampshire	2013	2014	2014
New Jersey	2011	2013	2011
New Mexico	2008	2009	2014
New York	2014	2016	2014
North Dakota	2016	2019	2014
Ohio	2016	2019	2014
Oregon	1999	2009	2014
Pennsylvania	2016	2018	2015
Rhode Island	2006	2013	2014
Vermont	2005	2013	2014
Virginia			2019
Washington	1999	2009	2011
West Virginia			2014
Wisconsin			2014

Notes: * Maryland first passed a law in 2003 reducing penalties for individuals possessing marijuana for medical necessity. However, the comprehensive medical MJ law in Maryland used as reference by previous literature, was approved in 2014.

Table C.2: Log Opioid-Related Mortality Rate

	(1)	(2)	(3)	(4)	(5)	(6)
	1999-2010			1999-2013		
MML	-0.145	-0.033	-0.034	0.016	0.130	0.129
	(0.151)	(0.168)	(0.168)	(0.140)	(0.138)	(0.122)
White	3.824	1.612	1.759	1.181	-0.804	-2.361
	(10.256)	(10.289)	(10.275)	(7.880)	(7.874)	(7.739)
Male	5.777	-4.998	-5.656	18.928	8.746	6.353
	(30.836)	(26.905)	(26.998)	(24.788)	(21.027)	(18.572)
18-64	-10.588	-2.968	-2.584	-14.279	-7.007	-3.740
	(16.675)	(13.690)	(13.742)	(15.591)	(13.096)	(12.124)
65+	11.276	24.953	24.853	-10.114	3.355	4.877
	(21.477)	(19.292)	(19.288)	(15.400)	(14.063)	(13.487)
Unempl. Rate	0.036	0.039	0.039	0.033	0.032	0.037
	(0.037)	(0.034)	(0.034)	(0.033)	(0.031)	(0.030)
Beer Tax	-0.803**	-0.547	-0.559	-0.108	-0.038	0.020
	(0.338)	(0.396)	(0.399)	(0.350)	(0.262)	(0.276)
PDMP	-0.263*	-0.231	-0.231	0.124	0.144	0.126
	(0.134)	(0.145)	(0.146)	(0.213)	(0.201)	(0.202)
Pill Mill	-0.068	-0.078	-0.084	-0.183	-0.203	-0.236*
	(0.182)	(0.162)	(0.163)	(0.136)	(0.130)	(0.132)
Disp.		-0.334***	-0.336***		-0.343***	-0.326***
		(0.115)	(0.114)		(0.118)	(0.109)
Exp			-0.467***			-0.263**
			(0.108)			(0.117)
N	553	553	553	703	703	703
R-sq	0.960	0.961	0.961	0.964	0.966	0.967

Standard errors in parentheses

Regressions include state and year fixed effects. No constant. Missing

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.3: Log Opioid-Related Mortality Rate

	(1)	(2)	(3)	(4)	(5)	(6)
	1999-2010			1999-2013		
MML	0.013	0.131	0.130	0.097	0.221	0.221*
	(0.177)	(0.175)	(0.174)	(0.139)	(0.133)	(0.116)
White	6.026	3.883	4.045	4.422	2.595	1.486
	(8.593)	(8.658)	(8.690)	(6.749)	(6.891)	(6.976)
Male	0.214	-10.457	-11.009	10.418	0.044	-2.501
	(27.349)	(23.378)	(23.513)	(22.777)	(19.051)	(17.123)
18-64	-8.914	-1.263	-0.988	-11.640	-4.350	-1.362
	(15.513)	(12.134)	(12.178)	(14.888)	(12.026)	(11.112)
65+	11.358	24.823	24.718	-9.541	3.638	4.560
	(20.509)	(17.563)	(17.579)	(15.093)	(13.447)	(12.943)
Unempl. Rate	0.015	0.020	0.020	0.013	0.013	0.019
	(0.035)	(0.033)	(0.033)	(0.033)	(0.030)	(0.030)
Beer Tax	-0.729**	-0.568	-0.575	-0.041	0.024	0.079
	(0.293)	(0.377)	(0.379)	(0.334)	(0.226)	(0.234)
PDMP	-0.279*	-0.240	-0.240	0.115	0.137	0.121
	(0.155)	(0.175)	(0.177)	(0.217)	(0.207)	(0.206)
Pill Mill	-0.124	-0.140	-0.144	-0.204	-0.230*	-0.262**
	(0.175)	(0.155)	(0.156)	(0.135)	(0.128)	(0.130)
Disp.		-0.375***	-0.376***		-0.377***	-0.359***
		(0.102)	(0.102)		(0.109)	(0.101)
Exp			-0.314			-0.249**
			(0.284)			(0.111)
N	612	612	612	765	765	765
R-sq	0.949	0.951	0.951	0.956	0.958	0.959

Notes: State clustered standard errors in parentheses. The regressions include population weight, state and year fixed effects. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.4: Log Opioid-Related Mortality Rate (Missing very low death counts)

	(1) 1999-2014	(2) 1999-2018
MML	0.319***	0.620***
	(0.112)	(0.127)
Disp.	-0.332***	-0.246**
	(0.112)	(0.095)
Exp	-0.084	0.137
	(0.110)	(0.102)
Pill Mill	-0.211	-0.087
	(0.135)	(0.143)
Beer Tax	0.205	0.388***
	(0.196)	(0.139)
PDMP	0.220	0.103
	(0.227)	(0.131)
Male	6.426	13.583
	(18.987)	(19.226)
Unempl. Rate	0.035	0.023
	(0.029)	(0.024)
White	0.374	4.359
	(7.473)	(6.404)
18-64	-6.437	-15.469
	(12.123)	(11.571)
65+	0.872	-5.614
	(12.699)	(10.961)
N	754	958
R-sq	0.967	0.970

Standard errors in parentheses.

These regressions include population weight, state and year fixed effects.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.5: Log Opioid-Related Mortality Rate (Imputed very low death counts)

	(1)	(2)
	1999-2014	1999-2018
MML	0.375***	0.642***
	(0.111)	(0.127)
Disp.	-0.351***	-0.256***
	(0.102)	(0.093)
Exp	-0.084	0.140
	(0.107)	(0.102)
Pill Mill	-0.240*	-0.118
	(0.135)	(0.143)
Beer Tax	0.240	0.402***
	(0.169)	(0.140)
PDMP	0.213	0.098
	(0.231)	(0.130)
Male	1.480	13.819
	(17.181)	(17.101)
Unempl. Rate	0.019	0.016
	(0.029)	(0.025)
White	3.285	5.949
	(6.395)	(5.393)
18-64	-5.691	-17.025
	(11.150)	(10.766)
65+	-0.780	-8.202
	(11.817)	(9.862)
N	816	1020
R-sq	0.961	0.967

Standard errors in parentheses.

These regressions include population weight, state and year fixed effects.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$