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Three Essays on Health Economics and Policy Evaluation

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Doctor of Philosophy in
Economics

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Abstract

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This dissertation consists of three essays on U.S. Health care policy. Each paragraph below refers to the three abstracts for the three chapters in this dissertation, respectively.

I provide quantitative evidence on how much Prescription Drug Monitoring Programs (PDMPs) affects the retail opioid prescribing behaviors. Using the American Community Survey (ACS), I retrieve county-level high dimensional panel data set from 2010 to 2017. I employ three separate identification strategies: difference-in-difference, double selection post-LASSO, and spatial difference-in-difference. I compare how the retail opioid prescribing behaviors of counties, that are mandatory for prescribers to check the PDMP before prescribing controlled substances (must-access PDMPs), vary from the counties where such a PDMP check is voluntary. I find must-access PDMP reduces about seven retail opioid prescriptions dispensed per 100 persons per year in each county. But, when I compare must-access PDMPs counties with bordering counties without such law, I find a reduction of three retail opioid prescriptions dispensed per 100 persons per year suggesting the possibility of spillovers of retail opioid prescribing behaviors.

As of 2019, all U.S. states, except Missouri, have enacted voluntary Prescription Drug Monitoring Programs (PDMPs). In response to the relatively low uptake of voluntary access, several states have strengthened their PDPMs by requiring providers to access information regarding prescription drug use under certain circumstances. These "must-access" PDPMs require states to view a patient's prescription history to facilitate the detection of suspicious prescription and utilization behaviors. This paper develops causal evidence of the effectiveness of "must-access" PDPM laws in reducing prescription opioid overdose death rates relative to voluntary PDMP states. I find that PDMPs are ineffective in reducing prescription opioid overdose deaths overall, but the effects are heterogeneous across states with "must-access" PDMP states. I find that marijuana and naloxone access laws, poverty level, income, and education confound the impact of must-access PDMPs on prescription opioid overdose deaths.

The optional provision of Medicaid expansion, through the Affordable Care Act (ACA), has triggered a national debate among diverse stakeholders regarding the impacts of Medicaid coverage on various dimensions of public health, costs, and benefits. Randomized experiments like the Rand Health Insurance Experiment and the Oregon Health Insurance Experiment have generated some credible estimates of the average treatment effects of insurance access. However, identical policy interventions can have heterogeneous effects on different subpopulations. This paper uses data from the Oregon Health Insurance Experiment to estimate the heterogeneous treatment

effects of access to Medicaid on health care utilization, preventive care utilization, financial strain, and self-reported physical and mental health. I detect heterogeneous treatment effects using a cluster-robust generalized random forest, a causal machine learning approach. I find that the impact of Medicaid is more pronounced among relatively older non-elderly and poorer households, consistent with standard adverse selection theory. Furthermore, I implement the "efficient policy learning," another machine learning strategy, to identify policy changes that prioritize providing Medicaid coverage to the subgroups that are likely to benefit the most. On average, the proposed reforms would improve the average probability of outpatient visits, preventive care use, overall health outcomes, having a personal doctor and clinic, and happiness by a range of 2% to 9% over a random assignment baseline. These findings help design Medicaid Section 1115 waiver.

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Dedication

I dedicate this dissertation to my wife Gita, son Soham, and my family in Nepal. Thank you for letting me chase my dreams.

Acknowledgements

I want to thank my advisors, Dr. Jane E. Ruseski and Dr. Joshua C. Hall, for the continued support throughout these years. I would also like to thank my committee members, Dr. Feng Yao and Dr. Bradley S. Price, for their invaluable feedback. I appreciate Prof. Bob Reed; without his support, this journey of PhD. was never a possibility for me. I also acknowledge Mr. Ashutosh Mani Dixit; he persistently motivated and helped me to pursue doctoral education. My sincere thanks go to Dr. Randall Jackson; he is my rock. The support that I have received from the Regional Research Institute family is beyond the words of expressions. My forever listening ears are Alexandre Scarcioffolo, Sultan Aziz, and Eduardo Minuci. Thank you for being there in good and in bad times. Dr. Amir Neto and Dr. Shree Baba Pokherel were my guide-posts who smoothen the rugged terrain of a Ph.D. by always timely guiding me on what to expect next and how to handle each Ph.D. requirements. My sincere gratitude goes to Dr. Alicia Morgan Plemmons, who continuously mentored me in the transition phases of my Ph.D. and job market seasons.

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Chapter 1

County Level Assessment of Prescription Drug Monitoring Program and Opioid Prescription Rate

1.1 Introduction

Deaths related to overdoses of opioids drugs, including both prescription opioid drugs and illicit opioids such as heroin and illicitly manufactured fentanyl, are rising in the United States, especially after 2010. On average, 130 Americans die every day from an opioid overdose (CDC, 2019). Compared to 1999, prescription-drug sales have quadrupled in the United States (CDC, 2019), leading to a 40 percent increase in prescription drug overdose deaths.

Abuse of prescription opioids drugs is highest compared to other variants of prescription drugs. National Center on Addiction and Substance Abuse (2014) estimates one in five Americans above 12-year ages misused prescription opioid drugs in their lifetime, and more than one in four new initiates of illicit drug users started with prescription opioid drug abuse. National Center on Addiction and Substance Abuse (2015) estimates 119 million Americans aged 12 or older used prescription

psychotherapeutic drugs in the past year, representing 44.5 percent of the population and 18.9 million people aged 12 or older (7.1 percent) misused prescription psychotherapeutic drugs in the past year. National Center on Addiction and Substance Abuse (2015) highlights several contributing factors to the prescription opioid drug epidemic, namely the advancement of new drug therapies, prescribing practices, internet pharmacies, expansion of insurance coverage, pharmaceutical advertisement, increased availability, medication and prescription pad theft, employee pilferage.

Opioid-dependent abusers steal, street purchase from a friend or relative, and doctor-shop to obtain prescription opioid drugs for non–medical use. Physicians represent the primary source for prescription opioid opioids for those who obtain prescription opioids through their own prescriptions (Jones et al., 2014). In contrast, pharmacists and physicians claim doctor shopping as the leading source for opioid abusers to get prescription opioid opioids (National Center on Addiction and Substance Abuse, 2015) and is an indirect channel of supply source for street dealers (Inciardi et al., 2009).

As policy responses to the escalating rates of opioid abuse and overdose death rates, the US policymakers have tried a variety of state-level policies like quantitative prescription limits, patient identification requirements, doctor-shopping restrictions, Prescription Drug Monitoring Program (henceforth PDMP or PDMPs), provisions related to tamper-resistant prescription forms, and pain-clinic regulations (Meara et al., 2016). The CDC has been promoting PDMPs as the best defense against the current impending crisis (Birk and Waddell, 2017).

As of 2019, 49 US states, along with the District of Columbia and the US territory of Guam has implemented some form of PDMPs. Except for the state of Missouri[1], all the US states have adopted voluntary PDMP. In contrast, few other states have enacted a so-called "mandatory" or must-access PDMP. Unlike voluntary PDMP, the must-access PDMP states abide by the law to collect data on controlled substance

[1]St. Louis County that accounts for more than half of Missouri's population has implemented their unique PDMP and appeal to other counties and cities in Missouri to conjoin (PDMPTTAC, 2019).

prescriptions that doctors have written for patients. The must-access PDMP states allow authorized individuals to view a patient's prescription history to facilitate the detection of suspicious prescriptions and utilization behaviors. The PDPMs varies by state along several dimensions[2] and also evolve over time[3].

Differentiating among voluntary and must-access PDMPs is crucial to understand how these programs affect the prescribing rate. For example, when New York implemented a must-access PDMP in 2013, the number of registrants increased fourteen-fold, and the number of daily queries rose from fewer than 400 to more than 40,000 (PDMP Center of Excellence, 2016). Similarly, in Kentucky, Tennessee, and Ohio, implementing a "must access" provision increased by order of magnitude the number of providers registered and the number of queries received per day (PDMP Center of Excellence, 2016). In contrast, in the first year after a voluntary PDMP was established in Florida, a state with a well-publicized opioid misuse problem, fewer than one in ten physicians had even created a login for the system (Electronic-Florida Online Reporting of Controlled Substances Evaluation, 2014).

In this paper, I am quantifying to what extent these must-access PDMPs change the opioid prescribing behavior. This research question is a crucial policy-relevant issue because the risk of an opioid use disorder, overdose, and death from prescription opioids are susceptible to the opioid prescribing rate.

Several papers relate the reduction of an opioid prescription to heroin crime (Alpert et al., 2017; Evans et al., 2018b; Kilby, 2015; Lankenau et al., 2012; Mallatt, 2018; Meinhofer, 2018b). While another strand of literature relates must-access PDMP to overdosages and overdosages death rates (Buchmueller and Carey, 2018; Meara et al., 2016; Meinhofer, 2018b). However, in this paper, I provide several

[2]States can differ in who may access the database (e.g., prescribers, dispensers, law enforcement), in the agency that administers the PDMP (e.g., department of health, pharmacy boards), in the controlled substances (CS) that are reported (e.g., some do not monitor CS-V), in the timeliness of data reporting (e.g., daily, weekly), in how to identify and investigate cases of potential doctor shoppers (e.g., reactive, proactive), and on whether prescribers are required to query the database (Meinhofer, 2018a).

[3]Initially, several states implemented paper-based PDMPs. Still, eventually, these and others shifted to electronic-based PDMPs (Meinhofer, 2018a).

unique contributions − first, this paper study of impacts of must-access PDMPs on the retail opioid prescribing rate. Several studies exist to answer similar questions (Strickler et al., 2019; Rutkow et al., 2015; Schieber et al., 2019), but these studies are descriptive. See Ponnapalli et al. (2018) for a systematic literature review of Prescription Drug Monitoring Programs too. However, I contribute to quantifying the impacts of must-access PDMPs on the opioid prescribing rate.

Second, this paper is first to exploit the county level variations of the retail opioid prescribing rate. Several studies provide state-level analysis of PDMPs on various outcomes of interests, and this is because PDMPs are state-level law. However, the county-level analysis offers a more granular summary by capturing the county level heterogeneity on how these state-level PDMP laws change the outcome of interest.

Third, I also utilize the two-way fixed effect difference-in-difference econometric approach with two novel identification strategies using US counties-level panel data spanning from 2010 to 2017. The first approach is the double selection post-LASSO − a causal-machine learning method − to control observable characteristics. The second approach exploits spatial contiguity to control for unobservables characteristics, possibly. The PDMPs are economic policy variables that are likely not to be randomly assigned. Therefore several observable characteristics could confound the PDMPs law and opioid prescribing rate. These observable characteristics can be the social, economic, and demographic profiles of counties along with several other state-level laws like Medicaid expansion, marijuana law, good Samaritan law, Naloxone access laws. The double selection post-LASSO allows selecting observable controls that affect PDMPs and prescribing rates. However, this method is likely not to properly unobservable. I compare the prescribing rate among must-access PMDP counties, which the bordering counties without must-access PMDP.

I find that must-access PDMPs reduce seven retail opioid prescriptions dispensed per 100s persons per county per year. However, when comparing the prescribing rate among must-access PMDP counties, which the bordering counties without must-access PMDP, I find about three retail opioid prescriptions dispensed per 100s persons

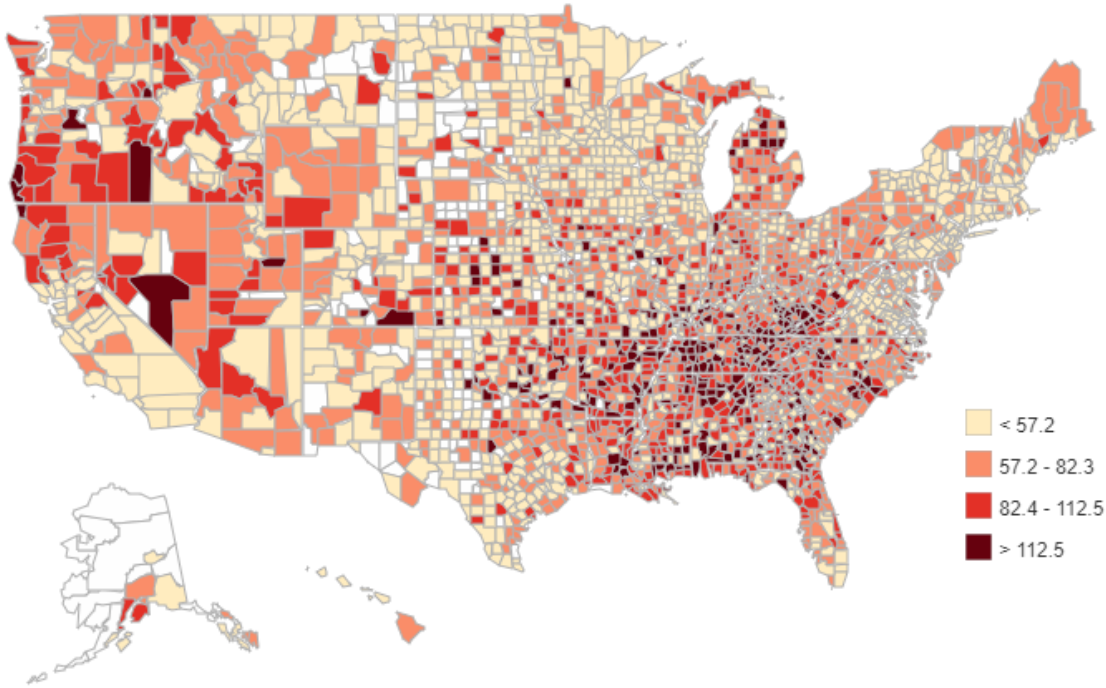
per county per year. Since the prescribing rate in boarding counties is lower than overall counties, it suggests it is likely that the prescribing rate from must-access PDMPs counties spillovers to bordering counties that do not have must-access PDMPs.

Section 1.2 explores the data. Section 1.3 layouts two-way fixed effect difference-in-difference econometric approach along with the double selection post LASSO and spatial methods. Section 1.4 provides the results and section 1.5 concludes the results.

1.2 Data

I web-scrape CDC website to acquire data of the retail opioid prescriptions dispensed per 100 persons per year[4] from 2006 to 2017. CDC estimates prescribing rates using the IQVIA Xponent data set.

Figure 1.1: Retail Opioid Dispensed per 100 Persons per Year, 2017



Source: https://www.cdc.gov/drugoverdose/maps/rxrate-maps.html

[4]Note that retail opioid prescriptions dispensed per 100 persons per year index is different from the morphine milligram equivalent (MME) per person or the number of opioids prescribed per person.

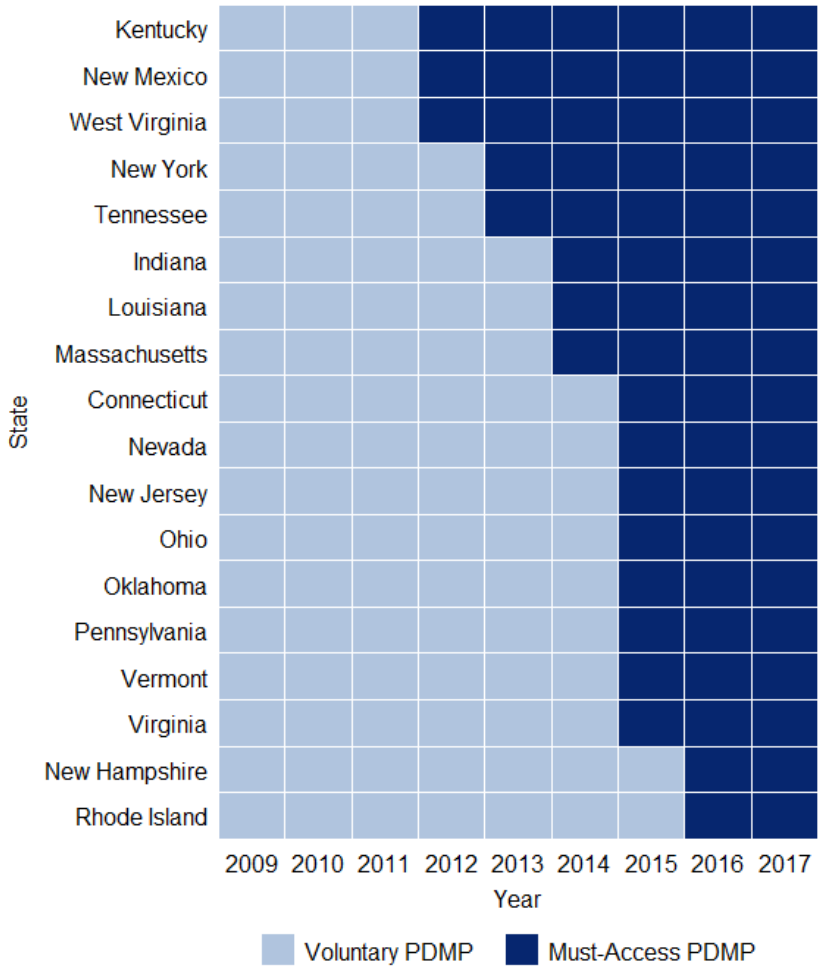
IQVIA Xponent is based on a sample of approximately 50,000 retail (non-hospital) pharmacies, which dispense nearly 90% of all retail prescriptions in the United States. For this database, a prescription is an initial or refill prescription dispensed at a retail pharmacy in the sample and paid for by commercial insurance, Medicaid, Medicare, or cash or its equivalent. This database does not include mail order pharmacy data. IQVIA Xponent data set uses the National Drug Code to identify opioid prescriptions, which include buprenorphine, codeine, fentanyl, hydrocodone, hydromorphone, methadone, morphine, oxycodone, oxymorphone, propoxyphene, tapentadol, and tramadol. However, the IQVIA Xponent data set excludes cough and cold formulations containing opioids and buprenorphine that are typically used to treat opioid use disorder. Also, methadone dispensed through methadone maintenance treatment programs is not included in the IQVIA Xponent data. A lack of available data in IQVIA Xponent may indicate that the county had no retail pharmacies, the county had no retail pharmacies sampled, or the prescription volume was erroneously attributed to an adjacent, more populous county according to the sampling rules used.

For the calculation of prescribing rates, numerators are the total number of opioid prescriptions dispensed in a county in a given year, and the denominator is the annual resident population denominator estimates obtained from the US Census Bureau. Figure 1.1 shows US opioid prescribing rate maps in 2017, where rates are classified by the Jenkse natural breaks classification method into four groups using the 12-year range of data (2006 to 2017) to determine the class breaks.

I retrieve the list of states that require prescribers to check the PDMP before prescribing controlled substances or must-access PDMP and the PDMP enactments date from the pdaps.org website. Figure 1.2 is a visual representation of state and timing of states that enacted must-access PDMP and the state with only voluntary PMDPs.

Using the Application Programming Interface of Census from the "censusapi" R package, I retrieve all the social, economic, housing, and demographic data profile of each county in the US from the five-year American Community Survey from 2010

Figure 1.2: State Requiring Prescribers to Check the PDMP Before Prescribing Controlled Substances



Source: http://pdaps.org/

to 2017. Then, I only include variables that are consistently available from 2010 to 2017. I then deleted variables that are a linear combination of each other and also remove furthermore highly correlated variables. This process, finally, retains 90 different social, economic, housing, and demographic data profiles of each county.

I also retrieve state-level laws like Good Samaritan Laws and Naloxone Access Law from pdaps.org website. I use procon.org to access the Marijuana Law (medical or/and recreational possession of Marijuana). States with the Good Samaritan Law provide immunity from prosecution for possessing a controlled substance while seeking help for himself or another person experiencing an overdose. The state with

Naloxone Access Law provides naloxone and other opioid overdose prevention services to individuals who use drugs, their families and friends, and service providers, including education about overdose risk factors, signs of overdose, appropriate response, and administration of naloxone. As of 2016, 48 states have authorized some variant of a naloxone access law, and 37 states have passed a drug overdose good samaritan law (Ayres and Jalal, 2018).

1.3 Methodology

1.3.1 Difference-in-Difference with Fixed Effects and Clustered Standard-Errors

I begin the analysis by showing if there is a significant difference in retail opioid prescriptions dispensed per 100 persons between the counties of the state that have a must-access prescription drug monitoring program (PDMPs henceforth) with the counties of the state that don't have such program. For this, I use a simple difference-in-difference model with county and year fixed effects while clustering the standard errors in-state levels.

$$Y_{it} = c + \delta D_{it} + \alpha_i + \varsigma_t + \varepsilon_{it} \tag{1.1}$$

where, Y_{it} is retail opioid prescriptions dispensed per 100 persons per year; c is the intercept, D_{it} is the treatment indicator and equals 1 after state i has been exposed to the treatment (must-access PDMP) and equals 0 otherwise; δ is the average treatment effect, α_i and ζ_t are additive individual state and year fixed effects respectively. One should expect a negative and significant value of δ, which would suggest the PDMP is successful in reducing retail opioid prescriptions dispensed. However, a positive and significant δ shows that states with PDMP have higher retail opioid prescriptions dispensed rates than in comparison states that do not have must-access PDMP.

1.3.2 High Dimensional Features and Unknown Data Generating Process

Studies that examine the impact of the must-access PDMPs on the retail opioid prescriptions dispensed are likely to suffer the endogeneity. The endogeneity leads to either over or underestimation of the effects of must-access PDMPs on the retail opioid prescriptions dispensed. The endogeneity arises because must-access PDMP enactment is a policy response to the escalating opioid-related overdose death rate and opioid prescribing behavior.

The equation 2.1 produces an incomplete picture of the relationship between retail opioid prescriptions dispensed and must-access PDMP. Since the policy/treatment variable is PDMP is a non-randomly assigned economic variable. The socio-economic and demographic profile of each county could likely affect both retail opioid prescriptions and must-access PDMP. Furthermore, literature has shown that Medicaid expansion, marijuana law, good Samaritan law, Naloxone access laws have a diverse effect on the demand for prescription opioids.

Failure to conditioning these confounders can lead to omitted variable bias. However, over-controlling leads to loss of efficiency of estimates. The actual data generating a process that explains the relationship between the must-access PDMPs and the opioid prescribing rate is unknown to the researcher. However, one can use general economic intuition to guide the variable selection that is standard in the literature. However, the actual data generating process (DGP) might comprise the various transformation of these observable confounders, for example, lags, higher-order polynomials, and interactions. Including and controlling for all these transformations may not be feasible because the covariates space can increase exponentially with high dimensional data.

Hence, the primary goal is to inference the low-dimensional parameter from the high-dimensional nuisance parameter, which comprises to solve auxiliary prediction

problem quite well. Consider the following outcomes y_i as a partially linear model:

$$\begin{aligned} y_i &= d_i\alpha_0 + g(z_i) + \xi_i, \quad E[\xi_i|z_i, d_i] = 0 \\ d_i &= m(z_i) + v_i, \quad E[v_i|z_i] = 0 \end{aligned} \tag{1.2}$$

where we have a sample of $i = 1, \ldots, n$ independent observation, d is policy/treatment variable as "must-access" PDMPs possibly non-randomly assigned an economic variable. The α_0 is the target parameter of interest, which answers the portion of variations in outcome variable due to the changes in policy variables. z_i is a high-dimensional vector of other controls or confounders. The high-dimensional vector of controls is in z_i and collected from the social, economic, housing, and demographic data profile from the American Community Survey for each county from 2010 to 2017. It is plausible to define that some of those features are a common cause for the existence of "must-access" PDMP and opioid prescription, and $m_0 \neq 0$, typically in the case of observational studies. $m_0 = 0$ would suggest that the policy variable is randomly assigned.

1.3.3 Double Selection Post LASSO

Lets consider linear combinations of control terms $x_i = P(z_i)$ to approximate $g(z_i)$ and $m(z_i)$. The list $x_i = P(z_i)$ could be composed of many transformations of elementary regressors z_i such as B-splines, dummies, polynomials, and various interactions. Having many controls poses a challenge of estimation and inference, therefore, to avoid such we assume the sparsity assumption that only a few among many variables in the z_i explains outcomes y_i.

$$\begin{aligned} y_i &= d_i\alpha_0 + \underbrace{x'_i\beta_{g0} + r_{gi}}_{g(z_i)} + \xi_i \\ d_i &= \underbrace{x'_i\beta_{m0} + r_{mi}}_{m(z_i)} + v_i \end{aligned} \tag{1.3}$$

The sparsity then relates to $x'_i\beta_{g0}$ and $x'_i\beta_{m0}$ approximate $g(z_i)$, and $m(z_i)$ that

requires only a small number of non-zero coefficients to render corresponding approximation errors r_{gi} and r_{mi}.

An appealing method to estimate the sparse parameter from a high-dimensional linear model is the Least Absolute Shrinkage and Selection Operator (LASSO) (Tibshirani, 1996). LASSO simultaneously performs model selection and coefficient estimation by minimizing the sum of squared residuals plus a penalty term. The penalty term penalizes the size of the model through the sum of absolute values of coefficients.

Let me define a feasible variable selection via LASSO for outcome variable and policy or treatment variable. Here, we change the notation as the outcome, and the policy variable takes the following form:

$$\begin{aligned} \tilde{y}_i &= \underbrace{x_i\beta_1 + r_i +}_{f(\tilde{z}_i)} \varepsilon_i \\ \tilde{d}_i &= \underbrace{x_i\beta_2 + m_i +}_{f(\tilde{z}_i)} \varepsilon_i \end{aligned} \tag{1.4}$$

moreover, LASSO estimator is defined as the solution to:

$$\begin{aligned} &\min_{\beta_1 \in \mathbb{R}^p} E_n\left[(\tilde{y}_i - \tilde{x}_i\beta_1)^2\right] + \frac{\lambda}{n}\|\beta_1\|_1 \\ &\min_{\beta_2 \in \mathbb{R}^p} E_n\left[\left(\tilde{d}_i - \tilde{x}_i\beta_2\right)^2\right] + \frac{\lambda}{n}\|\beta_2\|_1 \end{aligned} \tag{1.5}$$

where, the penalty level λ is a tuning parameter to regularize/controls the degree of penalization and to guard against overfitting. We choose λ by cross-validation in prediction. The $\|\beta\|_1 = \sum_{j=1}^{p} |\beta_j|$. The kinked nature of penalty function induces $\hat{\beta}$ to have many zeros, thus LASSO solution feasible model selection method. The estimated coefficients are biased towards 0; therefore, Belloni et al. (2013) and Belloni et al. (2014a) suggest to run an OLS on selected variables also known as post-LASSO or Gauss-LASSO estimator.

Let $\hat{I}_1 = S\left(\hat{\beta}_1\right)$ denote support or the controls selected by feasible LASSO estimator $\hat{\beta}_1$ and $\hat{I}_2 = S\left(\hat{\beta}_2\right)$ denote support or the controls selected by feasible LASSO estimator $\hat{\beta}_2$. The post-double-selection estimator $\breve{\alpha}$ of α_0 is defined as the least

squares estimator obtained by regressing y_i on d_i and the selected control terms x_{ij} with $j \in \hat{I} \supseteq \hat{I}_1 \cup \hat{I}_2$:

$$\left(\breve{\alpha}, \breve{\beta}\right) = \min_{\alpha \in \mathbb{R}, \beta \in \mathbb{R}^p} E_n\left[\left(y_i - d_i\alpha - \tilde{x}_i\beta\right)^2\right] \quad : \quad \beta_j = 0, \forall j \notin \hat{I} \tag{1.6}$$

In this equation 1.6, we can impose fixed effects and we can also cluster standard error. Belloni et al. (2013) provide theoretical results that the estimates are unbiased and consistent as:

$$\left(\left[\tilde{E}\tilde{v}_i^2\right]^{-1} E\left[\tilde{v}_i^2\tilde{\xi}_i^2\right]^{-1}\left[\tilde{E}\tilde{v}_i^2\right]^{-1}\right)^{-1/2} \sqrt{n}\left(\breve{\alpha} - \alpha_0\right) \xrightarrow{d} N\left(0, 1\right) \tag{1.7}$$

1.3.4 Managing Unobservable with Spatial Difference-in-Difference

The equation 1.6 allows us to properly select few or sparse observables from the high dimensional observables that could affect both the outcomes and policy variables. Equation 1.6 can utilize fixed effects to handle unobserved heterogeneity. However, as an additional layer of caution, I exploit the county level spatial contiguity. Rather than comparing outcomes of all the counties within the state with PDMPs and without PDMPs, in this setting, I implement equation 2.1 and 1.6 to compare outcome variables from the neighboring PDMPs county with the bordering counties without PDMPs. Figure 1.3 exhibits a map of the US that comprises the bordering treatment and comparison counties in a different color for the year 2017.

1.4 Results

Table 1.1 show the impacts of PDMP on retail opioid prescriptions dispensed with the Naïve OLS, double selection post-LASSO with pooled OLS, Naïve fixed effect, and double selection post-LASSO with fixed effect model in column (1) to (4) respectively. The dependent variable is retail opioid prescriptions dispensed per 100 persons, and the policy variable is the must-access PDMP. The standard errors are clustered at the state level to account for the intra-state level correlations.

Figure 1.3: Bordering Counties, 2017

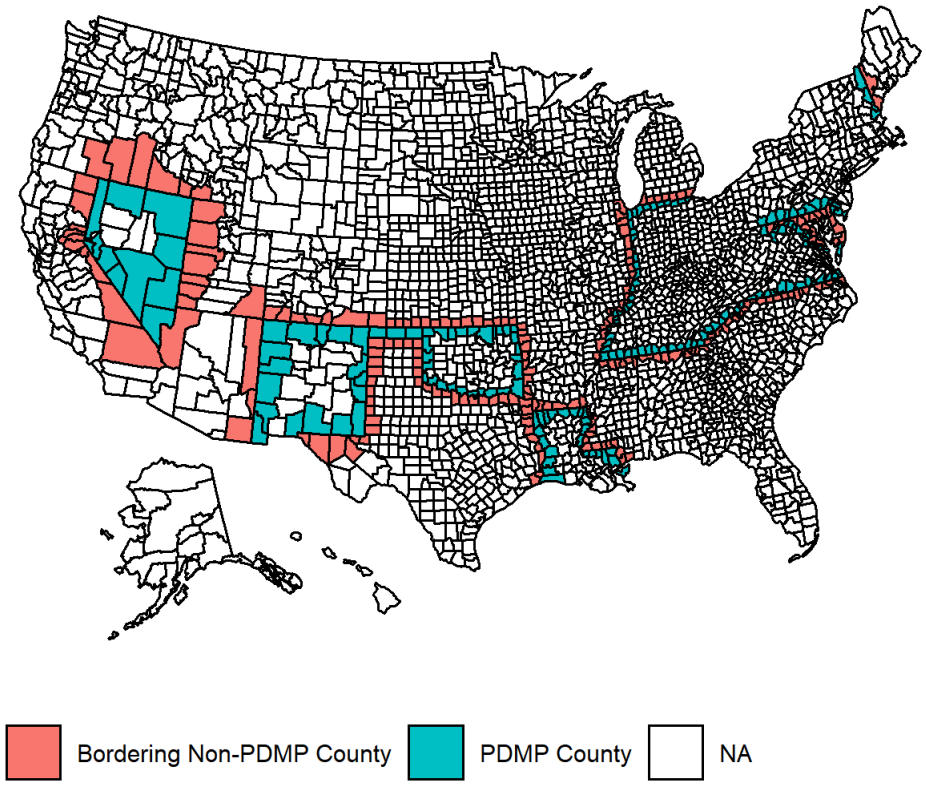


Table 1.1 column (1) and (2) are estimates of Naïve OLS and double selection post-LASSO with pooled OLS. These estimates are not signification in a 5% level of significance. However, the intercept of the Naïve OLS model holds the interpretation that, on average, in non-PDMPs counties, retail opioid prescriptions dispensed per 100 persons is 83, and counties with must-access PDMPs on average have additional six retail opioid prescriptions dispensed per 100 persons. For the remaining models in Table 1.1 column (2) to (4), the intercepts are not interpretive; therefore, I do not report them.

Table 1.1, column (3) and (4) estimate Naïve fixed effect and double selection post LASSO with fixed-effect models. Both models suggest that a reduction of 7 retail opioid prescriptions dispensed per 100 persons in the counties with must-access PDMPs compared to comparison counties. The estimates of column (3) and (4) are

Table 1.1: Impacts of Must-access PDMP on Retail Opioid Prescriptions Dispensed

	Retail opioid prescriptions dispensed per 100 persons			
	Naïve OLS	Pooled OLS	Naïve FE	DSPL FE
	(1)	(2)	(3)	(4)
PDMP	6.210	-2.622	-7.572***	-6.882***
	(7.064)	(3.282)	(2.035)	(1.521)
Intercept	83.530***			
	(3.535)			
R^2	0.002	0.258	0.929	0.931
Adj-R^2	0.002	0.257	0.919	0.920
County FE			Y	Y
Year FE			Y	Y
DSPL		Y		Y

Notes: Note: Robust standard errors clustered by the state are reported in parenthesis. *, ** and *** represent the 10%, 5% and 1% level of significance. Double selection post-LASSO (DSPL) is used for covariates selection. FE represents fixed effects.

similar; therefore, to save space, I do not report the selected variables.

Contrary to Table 1.1, in Table 1.2, I consider the must-access PDMP state's counties' retail opioid prescription rate with bordering counties from the state that have not enacted must-access PDMPs. Under the assumption that these bordering counties would be similar in their unobservables, I can test the impacts of must-access PDMPs on the retail opioid prescription rate. This will also allow checking if retail opioid prescription rate spillovers from must-access PDMPs counties to bordering counties without must-access PDMPs.

Table 1.2, column (1) presents estimates of Naïve OLS. The intercept shows that non-must-access PDMPs state counties that bordered with must-access PDMPs state counties have 95 retail opioid prescription rates per 100 persons, which is about nine retail opioid prescription rates per 100 persons higher.

Table 1.2, column (2), and (3) estimates Pooled OLS where the controls are selected using double selection post-LASSO and a Naive fixed effects estimate, respectively. Both these estimates show an insignificant effect of must-access PDMPs on the retail opioid prescription rate. However, the double selection post-LASSO with fixed effect in column (4) shows a reduction of about three retail opioid prescriptions

Table 1.2: Impacts of Must-access PDMP on Retail Opioid Prescriptions Dispensed, Spatial Contiguity

	Retail opioid prescriptions dispensed per 100 persons			
	Naïve OLS (1)	Pooled OLS (2)	Naïve FE (3)	DSPL FE (4)
PDMP	-9.184***	-2.426	-1.974	-3.158*
	(2.917)	(4.088)	(1.374)	(1.799)
Intercept	95.975***			
	(6.671)			
Good Samaritan Law				9.035***
				(2.882)
Information Industry (%)				3.811**
				(1.679)
Construction Industry (%)				1.032*
				(0.528)
Commuting Worked at Home (%)				-1.336*
				(0.665)
R^2	0.009	0.406	0.932	0.935
Adj-R^2	0.008	0.403	0.922	0.925
County FE			Y	Y
Year FE			Y	Y
" DSPL		Y		Y
Selected covariates				Y

Notes: Note: Robust standard errors clustered by the state are reported in parenthesis. *, ** and *** represent the 10%, 5% and 1% level of significance. Double selection post-LASSO (DSPL) is used for covariates selection. FE represents fixed effects.

rate per 100 persons, and this model selects several variables.

I choose and put only the significant control variables in column (4) to save space. Compared to counties without Good Samaritan Law, the counties with Good Samaritan Law has about nine more retail opioid prescription rate per 100 persons. States with the Good Samaritan Law provide immunity from prosecution for possessing a controlled substance while seeking help for himself or another person experiencing an overdose. Counties with a higher share of information and construction industry experience an additional 4 and 1 more retail opioid prescription rate per 100 persons, whereas counties with a higher share population who worked from home and did not commute have about one less retail opioid prescription rate per 100 persons.

1.5 Conclusion

This study quantifies how does the must-access PMDPs affect the retail prescription opioid prescribing rate and presents first-hand evidence at the county-level. Compare to non-must-access PDMPs counties, the must-access PDMPs counties, on average, have seven less retail opioid prescriptions dispensed per 100 persons per year. But, when I compare the bordering counties only, to control unobservables, I find must-access PDMPs counties have three less retail opioid prescriptions dispensed per 100 persons per year compared to their bordering counterpart non-must-access PDMPs counties, suggesting the possibilities of spillovers of retail opioid prescribing behaviors.

This study raises several issues. First, how much such reduction of retail opioid prescriptions dispensed per 100 persons per year translates into the decline of the prescription-related opioid death rate. Although the number of opioid-related deaths from all sources increased since 2012, the number of deaths each year associated with the use of prescription opioids alone has not increased since then (Schieber et al., 2019). Similarly, reducing retail opioid prescriptions could lead opioid abusers to switch to other cheaper and illicit substitutes. If there exists such substitution, then there could be unintended consequences of must-access PDMPs like increase crime, opioid poisoning, and deaths related to illegally manufactured Fentynal or heroine. Therefore, to solve the current opioid epidemic, both illicit street drugs and prescription opioids must become less available without compromising the need for compensating medical care related to the opioid and getting patients with opioid use disorder into treatment.

This study is subject to several limitations. CDC's IQVIA Xponent data set uses the National Drug Code to identify opioid prescriptions, which include buprenorphine, codeine, fentanyl, hydrocodone, hydromorphone, methadone, morphine, oxycodone, oxymorphone, propoxyphene, tapentadol, and tramadol. Each of these drugs is likely not equally prescribed; therefore, without administrative IQVIA Xponent data set,

it is not possible to see the heterogeneities within the retail prescription opioid prescribing rate. Furthermore, each must-access PDMPs can be different stringent on several dimensions. For example, states can differ in who may access the database (e.g., prescribers, dispensers, law enforcement), in the agency that administers the PDMP (e.g., department of health, pharmacy boards), in the controlled substances (CS) that are reported (e.g., some do not monitor CS-V), in the timeliness of data reporting (e.g., daily, weekly), in how to identify and investigate cases of potential doctor shoppers (e.g., reactive, proactive), and on whether prescribers are required to query the database (Meinhofer, 2018a). This study doesn't account for such variability of stringent PDMPs.

The analysis presented in this paper may inform states as they create laws, policies, communications, and interventions tailored to their specific problems. The magnitude, severity, and chronic nature of the opioid epidemic in the United States is of serious concern to clinicians, the government, the general public, and many others. As they review new studies and recommendations, clinicians should continue to consider how they might improve pain management, including opioid prescribing, in their practice (Schieber et al., 2019).

Chapter 2

Impact of Must-access Prescription Drug Monitoring Program on Prescription Opioid Overdose Death Rates

2.1 Introduction

The United States (U.S.) is amid an opioid drug epidemic[1]. From 1999 to 2017, over 700,000 people have died from a drug overdose, and nearly 400,000 people have died from an overdose involving prescription (Rx) opioids and illicit opioids like heroin and illicitly manufactured Fentanyl (CDC, 2019). In 2017, opioid overdoses claimed about 130 American lives each day[2]. In 2017, the number of overdose deaths involving opioids (including Rx opioids and illegal opioids) was six times higher compared to 2006 (CDC, 2019). The dramatic increase in opioid-related deaths has reversed the

[1]Opioid drugs are formulated to replicate properties of opium, mainly to soothe pain and emotions and to release the dopamine hormone to create a feeling of euphoria, and can lead users to dependence and later to the addiction. These opioid drugs include both legal painkillers like Morphine, Oxycontin, or Hydrocodone prescribed by doctors for acute or chronic pain and illegal drugs like heroin and illicitly made Fentanyl (CNN, 2019).

[2]Wide-ranging online data for epidemiologic research (WONDER). Atlanta, GA: CDC, National Center for Health Statistics; 2017. Available at http://wonder.cdc.gov.

declining midlife mortality trend for middle-aged Whites (Case and Deaton, 2015). Florence et al. (2016) estimate the total economic burden of Rx opioid overdose along with opioid abuse, dependence, loss of productivity, and criminal justice costs to be $78.5 billion annually.

In 2011, the Centers for Disease Control and Prevention (CDC) classified Rx abuse as an "epidemic". Among many policy responses, the CDC promotes the Prescription Drug Monitoring Programs (PDMPs) as the best defense against the current Rx opioid crisis (Birk and Waddell, 2017)[3]. The PDMP is a supply-side policy to restrict over-prescription and over-utilization of controlled substances while maintaining compassionate care. PDMPs collect data on prescriptions of controlled substances and allow authorized healthcare providers, law enforcement officials, PDMP administrators, and other authorized stakeholders (Meinhofer, 2018a) to identify patients who are possibly abusing Rx drugs, doctor shopping, and are at high risk of an overdose (Grecu et al., 2019)[4].

Currently, 49 U.S. states, along with the District of Columbia and the U.S. territory of Guam has implemented some form of PDMPs. The only state without a PDPM is Missouri[5] The stringency of the PDPMs varies by state along several dimensions[6] and also evolve over time[7]. As of 2018, 18 different states have enacted "must-access" or mandatory PDMP, while the remaining states have so-called "volun-

[3]Various state-level policy responses have been pursued to address the escalating rate of opioid abuse and overdose, including quantitative prescription limits, patient identification requirements, doctor-shopping restrictions, Prescription Drug Monitoring Programs (PDMPs), provisions related to tamper-resistant prescription forms, and pain-clinic regulations (Meara et al., 2016).

[4]The data collected generally includes the names and contact information of the patient, prescriber, and dispenser, the name and dosage of the drug, the quantity supplied, the number of authorized refills, and the method of payment (Meinhofer, 2018a).

[5]St. Louis County that accounts for more than half of the population of Missouri have implemented their own PDMP and appeal to other counties and cities in Missouri to conjoin (PDMPTTAC, 2019).

[6]States can differ in who may access the database (e.g., prescribers, dispensers, law enforcement), in the agency that administers the PDMP (e.g., department of health, pharmacy boards), in the controlled substances (C.S.) that are reported (e.g., some do not monitor CS-V), in the timeliness of data reporting (e.g., daily, weekly), in how to identify and investigate cases of potential doctor shoppers (e.g., reactive, proactive), and on whether prescribers are required to query the database (Meinhofer, 2018a).

[7]For instance, initially, several states implemented paper-based PDMPs, but eventually, these and others shifted to electronic-based PDMPs (Meinhofer, 2018a).

tary" PDMPs. Authorized individuals in the states that passed must-access PDMP are required by law to check the PDMP before prescribing controlled substances (Buchmueller and Carey, 2018).

Most of the previous literature finds that PDMPs, in general, have limited, inconsistent, or no effect on mortality and abuse (Meara et al., 2016; Brady et al., 2014; Reifler et al., 2012; Haegerich et al., 2014). These inconsistencies in results may be caused by not differentiating among voluntary and must-access PDMPs because, when provider access not mandatory, only a small share of providers create PDMP logins and request patient histories (PDMP Center of Excellence, 2014; Buchmueller and Carey, 2018)[8]. Therefore, previous studies that do not differentiate between voluntary and mandatory PDPMs are likely to consider lower provider utilization of PDMPs when estimating the possible impacts.

A few recent studies differentiate between "must-access" and voluntary PDMPs in the research design. Buchmueller and Carey (2018) find must-access PDMPs reduce indicators of opioid abuse while voluntary PDMPs have no effects among elderly and disabled participants between 2007 and 2013. Ali et al. (2017) find limited impact based on self-reported measures of Rx drug abuse. Grecu et al. (2019) find a reduction in opioid abuse among young adults (ages 18 to 24) and substitution toward other illicit drugs and a corresponding decrease in admissions related to cocaine and marijuana abuse.

This paper contributes to this limited literature in several aspects. First, I examine the effect of the must-access PDMPs and develop some of the first evidence of state-level heterogeneous effects of must-access PDMPs. Second, I control for observable confounders using a high dimensional panel data from 1999 to 2017, im-

[8]For example, when New York implemented a must-access PDMP in 2013, the number of registrants increased fourteen-fold, and the number of daily queries rose from fewer than 400 to more than 40,000 (PDMP Center of Excellence, 2016). Similarly, in Kentucky, Tennessee, and Ohio, implementing a "must access" provision increased by order of magnitude the number of providers registered and the number of queries received per day (PDMP Center of Excellence, 2016). In contrast, in the first year after a voluntary PDMP was established in Florida, a state with a well-publicized opioid misuse problem, fewer than one in ten physicians had even created a login for the system (Electronic-Florida Online Reporting of Controlled Substances Evaluation, 2014).

plementing the double-selection post-LASSO method (Belloni et al., 2013). Most previous studies exploit variation PDMP policies as an exogenous shock to examine the effect of PDMPs on some outcome variables like opioid abuse, poisoning, and overdose death. However, state-specific political, socioeconomic, and demographic features could affect both PDMP enactment and opioid-related outcome variables. Therefore, for inference, the state's political, socioeconomic, and demographic characteristics must be adequately controlled. The double-selection post-LASSO method helps with causal inference by utilizing the strengths of machine learning methods to select adequate observables and instruments.

Third, I examine Rx opioid overdose deaths in a state setting and contribute to the literature of program evaluation in a regional context. The synthetic control method only allows estimating the policy effect on one treatment unit or state (Abadie and Gardeazabal, 2003; Abadie et al., 2010, 2015); however, this study evaluates the impact of "must-access" PDMPs by implementing a generalized synthetic control method which allows multiple intervention units (Xu, 2017). This approach also allows modeling the unobserved time-varying heterogeneity by explicitly implementing the interactive fixed effects (IFE) model of Bai (2009), while the previous studies model the unobserved time-varying heterogeneity using unit-specific linear or quadratic time trends in a conventional two-way fixed effects models (Grecu et al., 2019; Mallatt, 2018). Fourth, to generalize the results to the national context, I implement weighted regressions, which allows for investigating the impact of "must-access" PDMP provisions across states.[9]

My findings show that "must-access" PDMPs states do not reduce the Rx opioid overdose deaths while these effects are heterogeneous across "must-access" PDMP states. I find evidence that marijuana and naloxone access laws, poverty level, income, education confound the impact of must-access PDMPs on the Rx opioid overdose deaths. I show that the unobserved time-varying heterogeneity possibly relates

[9]Grecu et al. (2019) suggest utilizing weighted regressions because unweighted regressions impose the constraint of a similar effect for the entire population which may mask heterogeneity and policy effects.

to the illicit Fentanyl overdose death rate, which cannot be identified from the overall Rx opioid overdose death rate. Furthermore, this paper explains why the existing literature does not reach a consensus regarding the effect of PDMPs. I show evidence that the definition of the Rx opioid death rate provided by the CDC can lead to inconclusive results. Among the deaths with drug overdose as the underlying cause, the CDC reports the Rx opioid deaths following ICD-10 multiple cause-of-death codes: natural and semisynthetic opioids (T40.2); methadone (T40.3); and synthetic opioids, other than methadone (T40.4). Deaths from illegally-made Fentanyl cannot be distinguished from pharmaceutical Fentanyl in the data. For this reason, deaths from both legally prescribed and illegally produced Fentanyl are included in these data.

Section 2.2 comprises a background of Rx opioids and PDMPs. Section 2.3 provides a literature review. Section 2.4 layouts detailed empirical strategies. Section 2.5 explains the data. Section 2.6 reports the results. Section 2.7 concludes the study.

2.2 Prescription Drug Epidemic

In the 1840s, opium and Morphine were sold as miracle cures and syrup. Diverse users[10] triggered the first U.S. opium and morphine epidemic that lasted until the 1910s. The 1960's heroin epidemic, the 1980's cocaine/crack epidemic, and 2000s methamphetamine epidemic are evidence that the United States has a persistent insatiable demand for intoxicating substances, legal and illegal (Pacula and Powell, 2018), and the U.S. is always on the war on drugs.

The root causes of the present U.S. opioid epidemic dates back to the 1980s. Portenoy and Foley (1986)'s conclusion that long-term usages of opioid pain relievers are safe (based on the sample size of 38 chronic pain patients) was widely cited to support the use of opioid pain relievers for chronic non-cancer pain. The prac-

[10]Mothers dosed themselves and their children with opium tinctures and patent medicines. Soldiers used opium and Morphine to treat diarrhea and painful injuries. Drinkers alleviated hangovers with opioids. Chinese immigrants smoked opium, a practice that spread to the white underworld. But the primary source of the epidemic was iatrogenic morphine addiction, which coincided with the spread of hypodermic medication during 1870–1895. The model opioid-addicted individual was a native-born white woman with a painful disorder, often of a chronic nature (Kolodny et al., 2015).

tice to prescribe opioid pain relievers for chronic non-cancer pain gradually rose and accelerated rapidly after 1995 when Purdue Pharma introduced "OxyContin" as an extended-release formulation of oxycodone with aggressive marketing and promotion strategies[11]. This extend-release formulation contained a much higher concentration of oxycodone (Singer, 2018), which slowly releases into the bloodstream and can be taken less frequent intervals (every 12 hours for control of chronic pain) than other immediate-release counterparts products (Soni, 2018). Bootleggers diverted a considerable amount of OxyContin to the illegal market for non-medical use, and abusers crush OxyContin into a fine powder to snort (intranasal), or dissolve the powder in water to inject into intravenous, or chew (Singer, 2018).

Around the same time, in 1995, the president of the American Pain Society's campaign, "Pain is the Fifth Vital Sign" encourage healthcare professionals to assess pain (Kolodny et al., 2015) along with other four vitals: body temperature, blood pressure, heart rate, and respiratory rate. By 2001, the Veteran Affairs health system and the Joint Commission on Accreditation of Healthcare Organizations (JCAHO) – which accredits hospitals and other health care organizations, with the introduction of new pain management standards – made a formal recommendation to include pain as the fifth vital sign in the physician checklist (Pacula and Powell, 2018).

In 2005, Medicare introduced Hospital Consumer Assessment of Healthcare Providers and Systems (HCAHPS)[12] linked inpatient reimbursement payments with patients'

[11]As per The United States, General Accounting Office (2003), between 1996 to 2002, Purdue Pharma funded direct sponsorship or financial grants for more than 20,000 pain-related educational programs to promoted long-term use of OxyContin for chronic non-cancer pain. Purdue Pharma also provided financial support to the American Pain Society, the American Academy of Pain Medicine, the Federation of State Medical Boards, the Joint Commission, pain patient groups, and other organizations Fauber (2012).

[12]The HCAHPS Survey has three intents. The first is to produce comparable data on the patient's perspective on the care that allows objective and meaningful comparisons between hospitals on domains that are important to consumers. Second is to incentives for hospitals to improve their quality of care by linking Medicare reimbursement. The third is to enhance public accountability in health care by increasing the transparency of the quality of hospital care provided in return for the public investment. The HCAHPS survey contains 21 patient perspectives on care and patient rating items that encompass nine key topics: communication with doctors, communication with nurses, the responsiveness of hospital staff, pain management, communication about medicines, discharge information, cleanliness of the hospital environment, the quietness of the hospital environment, and transition of care. The survey includes four screener questions and seven demographic items, which

perspectives on hospital care – one of the measures is "pain management." When the Affordable Care Act passed, value-based incentive payments to hospitals were tied to the value of these patient experience performance measures, which included pain management scores as a core component Pacula and Powell (2018).

Kolodny et al. (2015) define increasing overdose deaths involving prescription opioids (natural and semisynthetic opioids and methadone) since at least 1999 to 2010 as the "first wave". The "second wave" began in 2010 with rapid increases in overdose deaths involving heroin (CDC, 2019) and is contemporaneous with the 2010 abuse-deterrent formulations (ADF) or reformulation[13] of OxyContin (Evans et al., 2018a), pill mill crackdown, prescription drug monitoring programs (PDMPs) (Meinhofer, 2016). The "third wave" began in 2013, with significant increases in overdose deaths involving synthetic opioids — particularly illicitly-manufactured Fentanyl (IMF) (CDC, 2019) and its analogs adulterated with counterfeit pills and heroin which are highly potent, less bulky and – that are sourced primarily from China, Mexican drug trafficking organizations and disseminate using crypto-currencies through internet (Beletsky and Davis, 2017).

2.3 Literature Review

Existing studies associate PDMPs with opioid prescription and opioid-related overdose deaths and poisoning, while another strand of literature exploits PDMPs as an exogenous source of variation to investigate the heroin-related crime.

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are used to adjust the mix of patients across hospitals and for analytical purposes. The survey is 32 questions in length. See: https://www.hcahpsonline.org/

[13]By the early 2000s, opioid overdoses and deaths, especially related to OxyContin spiked. In 2007, Prude Pharma pleaded guilty to misbranding OxyContin, a felony under the Food, Drug, and Cosmetic Act, and agreed to pay more than $600 million in fines (Van Zee, 2009). In April 2010, the Food and Drug Administration approved Purdue Pharma's ADF of the original OxyContin formulation. With no public notice, on the 5th August 2010, Purdue Pharma stopped manufacturing the unique formulation of OxyContin and only manufactured and sold the reformulated version from 9th August 2010 (Butler et al., 2013) without any change in the price (Coplan et al., 2016). The ADF OxyContin is resistant to crushing, forms a gel not quickly injected when dissolved in solutions, and resists extraction with solvents (Singer, 2018).

Simeone and Holland (2006) study the effect of PDMPs on the supply using Automation of Reports and Consolidated Orders System (ARCOS) and abuse of Rx drugs using Treatment Episode Data Set (TEDS) dataset. They find states with PDMP reduces per capita supply of Rx pain relievers and stimulants while the probability of abuse is higher among nonPDMPs states compared to PDMPs states. Reisman et al. (2009) also, find similar results that PDMP decreases the number of oxycodone shipments and the Rx opioid admission rate for states with these programs. Reifler et al. (2012) implement repeated measures negative binomial regression on quarterly RADARS® System Poison Center and Opioid Treatment surveillance data (from 2003 to mid-2009) to estimate and compare opioid abuse and misuse trends. They find compared to nonPDMPs, PDMPs states reduce Poison Center intentional exposures by 1.9% per quarter, exposures opioid intentional exposures by 0.2% per quarter. In contrast, opioid treatment admissions increase, on average, 4.9% per quarter in states without a PDMP vs. 2.6% in states with a PDMP. These findings suggest the effectiveness of PDMPs. Simoni-Wastila and Qian (2012) retrieve 2.2 million records from Coordination of Benefits (COB) MarketScan administrative claims data of Medicare-eligible and their dependents to study analgesic utilization by an insured retiree population among the different types of PDMPs and nonPDMPs states with cross-sectional study implementing multivariate logistic and multinomial regressions. They find reductions in the utilization of targeted Rx opioid analgesics and increases in less scrutinized, lower scheduled opioid analgesics. Contrary to these studies, Brady et al. (2014) find no significant impact on per-capita opioids dispensed among PDMP states. They covert quarterly 1999-2008 ARCOS database to morphine milligram equivalents (MMEs) for each state then implement multivariable linear regression modeling with temporal trends and demographic characteristics.

Contrary to previous studies that use simple multivariate analysis, the health economics literature deals rigorously with an identification strategy for proper estimation. For example, Kilby (2015) uses an individual-level dataset of Rx claims of 59% of the U.S. population from Truven Health Analytics and merges this dataset

with ARCOS dataset. She finds about a 10% reduction of oxycodone Rx and a 10% decrease in oxycodone shipment. Similarly, Buchmueller and Carey (2018) uses a claims-level subsample of the universe of Medicare claims, and find must-access PDMPs reduce indicators of opioid abuse. In contrast, voluntary PDMPs have no effects among elderly and disabled participants between 2007 and 2013. Ayres and Jalal (2018) implements standard difference-in-difference with fixed effect methods on the county-level panel data on all opioid Rx in the U.S. between 2006 and 2015 along with county-level demographic controls, other state-level opioid interventions such as Naloxone Access and Good Samaritan laws, Medicaid expansion, and the provision of Methadone Assistance Treatment. They find a reduction of Rx rates; however, such a decline is pronounced among urban, predominantly white counties within more affluent regions. Another recent study Rivera-Aguirre et al. (2019) explores the source of heterogeneity of PDMPs (what populations benefit the most from these programs) and opioid overdoses using county-level, spatiotemporal study design. They find lower rates of Rx opioid-related hospitalizations but see an increase in heroin-related admission.

Contrary to the effect of PDMPs on the Rx rates, the results for the impact of must-access PDMPs on outcomes like opioid overdoses and opioid-related overdoses death rates are mixed. Patrick et al. (2017) perform 1999-2013 period state-level analysis with interrupted time-series with fixed effect and a linear time trend method using Wide-Ranging Online Data for Epidemiologic Research (WONDER) database of multiple causes of death maintained by the Centers for Disease Control and Prevention (CDC). They find an average reduction of 1.12 opioid-related overdose deaths per 100,000 population in the year after PDMPs implementation.

My study is similar to Patrick et al. (2017) in which they explore the impact of PDMPs on the Rx opioid overdose. However, they utilize interrupted time-series with fixed effect and a linear time trend; my study has a more rigorous identification strategy and implements non-linear time trends using interactive fixed effect. Unlike many other studies that utilize difference-in-difference with fixed effect methods, I

perform difference-in-difference with event study framework and generalized synthetic control approach, which is similar to event study models of Grecu et al. (2019) and Mallatt (2018) and interactive fixed-effect models used by Mallatt (2018). However, these studies explore the impact of PDMPs on the Rx opioid prescription and abuse; however, this research examines the impact of PDMPs on the Rx opioid overdose death rate similar to Erfanian et al. (2019). However, Erfanian et al. (2019) study impact of Naloxone access laws on opioid overdose deaths utilizing spatial econometric methods. Several studies exhibit the heterogeneous effects of PDMPs mainly on different age groups within the state population like Grecu et al. (2019); Mallatt (2018); Ayres and Jalal (2018); Buchmueller and Carey (2018). However, I show first-hand evidence of state-level heterogeneous effects of Rx opioid overdose. As my knowledge, this paper is first to utilize the strength and innovation of machine learning and causal inference namely the double-selection post-LASSO (Belloni et al., 2013) which is a robust method for inference on the effect of a treatment variable (must-access PDMP) on the outcome variable (Rx opioid overdose death) by selecting adequate observable confounders from a list of high dimensional controls which I compile based on the literature review and economic intuition.

2.4 Empirical Strategies

2.4.1 Two-way Fixed Effect Difference-in-Differences Framework

I exploit variation in the timing of adoption of must-access PDMPs, within a variety of difference-in-differences (D.D.) frameworks, to estimate the impact on the Rx opioid overdose death rate. I begin the analysis with a two-way fixed-effect model.

$$Y_{it} = c + \delta D_{it} + \alpha_i + \varsigma_t + \varepsilon_{it} \tag{2.1}$$

where, Y_{it} is Rx opioid overdose death rates per 100,000 population (age-adjusted); c is the intercept, D_{it} is the treatment indicator and equals 1 after state i has been exposed to the treatment (must-access PDMP) and equals 0 otherwise; δ is the average treatment effect, α_i and ζ_t are additive individual state and year fixed effects respectively. One should expect a negative and significant value of δ which would suggest the must-access PDMP is successful to reduce Rx opioid overdose death rates. However, a positive and significant δ shows that state with must-access PDMP have on average higher Rx opioid overdose death rates compare to comparison state that do not have must-access PDMP.

2.4.2 Two-way Fixed Effect Difference-in-Differences Framework with LASSO

The state-specific political, socioeconomic, and demographic features could affect both must-access PDMP enactment and Rx opioid overdose death rates. Therefore, for inference, political, socioeconomic, and demographic characteristics of state or observable confounders must be adequately controlled. Failure to conditioning these confounders can lead to omitted variable bias. However, over-controlling leads to loss of efficiency of estimates. The actual data generating a process that explains the relationship between the must-access PDMPs and Rx opioid overdose death rate is unknown to the researcher. However, one can use general economic intuition to guide the variable selection that is standard in the literature. Table 2.1 in the results section displays the list of variables, their transformation, units, data sources, and summary statistics. However, the actual data generating process (DGP) might comprise the various transformation of these observable confounders, for example, lags, higher-order polynomials, and interactions. Including and controlling for all these transformations may not be feasible because the covariates space can increase exponentially with high dimensional data, and regression is infeasible when the numbers of covariates exceed the number of observations in data.

To properly select the observable confounders, I exploit the strengths and inno-

vations of machine learning method, namely the "LASSO"[14] and causal inference. Under the assumption of sparsity, I utilize the double-selection post-LASSO method Belloni et al. (2013) to select the observable confounders properly.The double-post-LASSO procedure comprises the following steps (Belloni et al., 2014a). First, run LASSO of dependent variables on a large list of potential covariates to select a set of predictors for the dependent variable. Second, run LASSO of treatment variable on a large list of potential covariates to select a set of predictors for treatment. If the treatment is truly exogenous, I should expect this second step should not select any variables. Third, run OLS regression of dependent variable on treatment variable, and the union of the sets of regressors selected in the two LASSO runs to estimate the effect of treatment on the dependent variable then correct the inference with usual heteroscedasticity robust OLS standard error. The following D.D. exhibits the estimation after the double-post-LASSO procedure.

$$Y_{it} = c + \delta D_{it} + \beta x_{it} + \alpha_i + \varsigma_t + \varepsilon_{it} \tag{2.2}$$

where, x_{it} are a set of time-varying observable confounders selected by the double-selection post-LASSO.

2.4.3 Event Study Framework

The DD estimates in equation 2.1 and equation 2.2 only show the average impact of the must-access PDMPs. To obtain a more precise understanding of the impact of the must-access PDMPs, I employ an event study methodology, which takes into ac-

[14]The Least Absolute Shrinkage and Selection Operator (LASSO) is an appealing method to estimate the sparse parameter from a high-dimensional linear model is introduce by Frank and Friedman (1993) and Tibshirani (1996). LASSO simultaneously performs model selection and coefficient estimation by minimizing the sum of squared residuals plus a penalty term. The penalty term penalizes the size of the model through the sum of absolute values of coefficients. Consider a following linear model $\tilde{y}_i = \Theta_i\beta_1 + \varepsilon_i$, where Θ is high-dimensional covariates, the LASSO estimator is defined as the solution to $\min_{\beta_1 \in \mathbb{R}^p} E_n\left[\left(\tilde{y}_i - {}_i\Theta\beta_1\right)^2\right] + \frac{\lambda}{n}\|\beta_1\|_1$, the penalty level λ is a tuning parameter to regularize/controls the degree of penalization and to guard against overfitting. The cross-validation technique chooses the best λ in prediction models and $\|\beta\|_1 = \sum_{j=1}^{p} |\beta_j|$. The kinked nature of penalty function induces $\hat{\beta}$ to have many zeros; thus LASSO solution feasible for model selection.

count the possible dynamic response of must-access PDMP on the Rx opioid overdose death rate. The event study D.D. framework with policy lags and leads to provide visual evidence of the policies' effect. Furthermore, the event study D.D. framework is visually appealing to detect the parallel trend – in the absence of the treatment, the average outcomes of treated and comparison states would have followed parallel paths – which is the key identifying assumption for D.D.

$$Y_{it} = c + \sum_{p=-12}^{6} \delta_p D_{i,t+p} + \beta x_{it} + \alpha_i + \varsigma_t + \varepsilon_{it} \tag{2.3}$$

where $D_{i,t+p}$ is an indicator equal to one if the must-access PDMP started in state i in the time $t+p$ and equal zero in all other time periods. The coefficient δ_p capture the measured effect of the must-access PDMP at p periods after the enactment. The negative value of p correspond to "leads," which captures the effect of the policy before it is implemented and should be zero under the "parallel trend" assumption (the average outcomes of the treated and control units follow parallel paths in pretreatment periods is required to maintain for causal inference) of the DD framework. The data starts from 1999 to 2017 which is 19 period. In this data sample, Kentucky, New Mexico and West Virginia are the earliest states that enacted must-access PDMP in 2012. From 2012 until 2016 there are 6 periods and prior 2012 there are 13 periods. Therefore, I index p from -12 to 6 periods. See Figure 2.2.

2.4.4 Generalized Synthetic Control

The detection of the average outcomes of the treated and comparison states follow parallel paths in pretreatment periods is not sufficient but provides more confidence in the validity of the parallel posttreatment period. But, in many cases, if the parallel pretreatment trends are not supported by data, it's likely to fail in the posttreatment period. The "parallel trend" assumption is not directly testable; however, literature provides two broad directions. First is the synthetic control (S.C.) method proposed by Abadie, Diamond, and Hainmueller (2010, 2015). It matches both pretreatment

covariates and outcomes between a treated unit and a set of control units and uses pretreatment periods as criteria for suitable matches. Specifically, it constructs a "synthetic control unit" as the counterfactual for the treated unit by reweighting the control units. It provides explicit weights for the control units, making a comparison between the treated and synthetic control units transparent. However, it only applies to the case of one treated unit, and the uncertainty estimates it offers are not easily interpretable. The synthetic control method is not appropriate in this study because must-access PDMPs were enacted in several states at different periods.

The second approach is to model the unobserved time-varying heterogeneities explicitly. A widely used strategy is to add in unit-specific linear or quadratic time trends to conventional two-way fixed effects models. For example, Grecu et al. (2019) imposes a quadratic time trend to their two-way fixed effect model to examine the impact of opioid abuse among young adults; and Mallatt (2018) implements linear, quadratic and cubic time trends to estimate the effect of PDMP on heroin incidents.

An alternative way is to model unobserved time-varying confounders semi parametrically. For example, Bai (2009) proposes an interactive fixed effects (IFE) model, which incorporates the unit-specific intercepts interacted with time-varying coefficients. The time-varying coefficients are also referred to as (latent) factors, while the unit-specific intercepts are labeled as factor loadings. Unlike explicitly imposing a linear or a quadratic time trend to a model, the IFE allows for additional non-linear time trends that affect areas to varying degrees. The factor captures nationwide time trends in Rx opioid-related overdose deaths to which different states are either more or less susceptible, depending on the unobservable characteristics of those states. The factor loading exhibits the intensity or severity of such nationwide time trends for each state. For example, Mallatt (2018) implements the D.D. framework with the IFE model to identify the effect of PDMPs on opioid painkiller Rx filled and on rates of heroin crimes.

I implement Xu (2017) generalized synthetic control (GSC) method that unifies the synthetic control method with linear fixed-effects models. This method provides

several advantages over the S.C. method and IFE. First, it generalizes the synthetic control method to cases of multiple treated units and/or variable treatment periods and can estimate confidence intervals for counterfactual. Therefore, this method is suited for the causal inference for program evaluation in a regional context. Second, it embeds a cross-validation scheme to select the number of factors of the IFE model. This is crucial because, in practice, researchers may have limited knowledge of the exact number of factors to be included in the model. The GSC estimate is presented as:

$$Y_{it} = \delta_{it} D_{it} + x'_{it}\beta + z_i \theta_t + \lambda'_i f_t + \alpha_i + \zeta_t + \varepsilon_{it} \tag{2.4}$$

where, f_t comprise of r different factor and λ_i are factor loadings. The main quantity of interest is the average treatment effect on the treated (ATT) at the time t when $t > T_0$ and given as:

$$ATT_{t,t>T_0} = N_{tr}^{-1} \sum_{i \in \tau} \left[Y_{it}(1) - Y_{it}(0) \right] = N_{tr}^{-1} \sum_{i \in \tau} \delta_{it}$$

where, $Y_{it}(1)$ is the observed for treated units in the posttreatment period, and $Y_{it}(0)$is the counterfactual for the treated unit in the posttreatment period. The total number of states is $N = N_{tr} + N_{co}$, where N_{tr} and N_{co} are the numbers of treated and control states. The $T_{0,i}$ is the number of pretreatment period for state i and state are first exposed to the treatment at the time $(T_{0,i} + 1)$ and observed for $T - T_{0,i}$ periods. States in the control group remain unexposed to the treatment in the observed period. Under several assumptions[15], Xu (2017) GSC estimator is a three-step process. First, the GSC method estimates the interactive fixed-effect model using only the control group. Second, GSC estimates factor loadings for each treated unit by minimizing the mean squared error of the predicted treated outcome in pretreatment periods. Third, GSC estimates counterfactuals with a cross-validation

[15]Under the assumption of strict exogeneity (unconfoundedness), decomposable time-varying confounders, weak serial dependence of the error term, some regularity conditions and cross-sectionally independent and homoscedastic error terms.

procedure to select the number of factors to be included in the model.

All of the regression presented in this section have clustered standard error at the state level to allow for an arbitrary autocorrelation process within states (Bertrand et al., 2004) and are weighted by the population of the relevant state (Angrist and Pischke, 2009).

2.5 Data

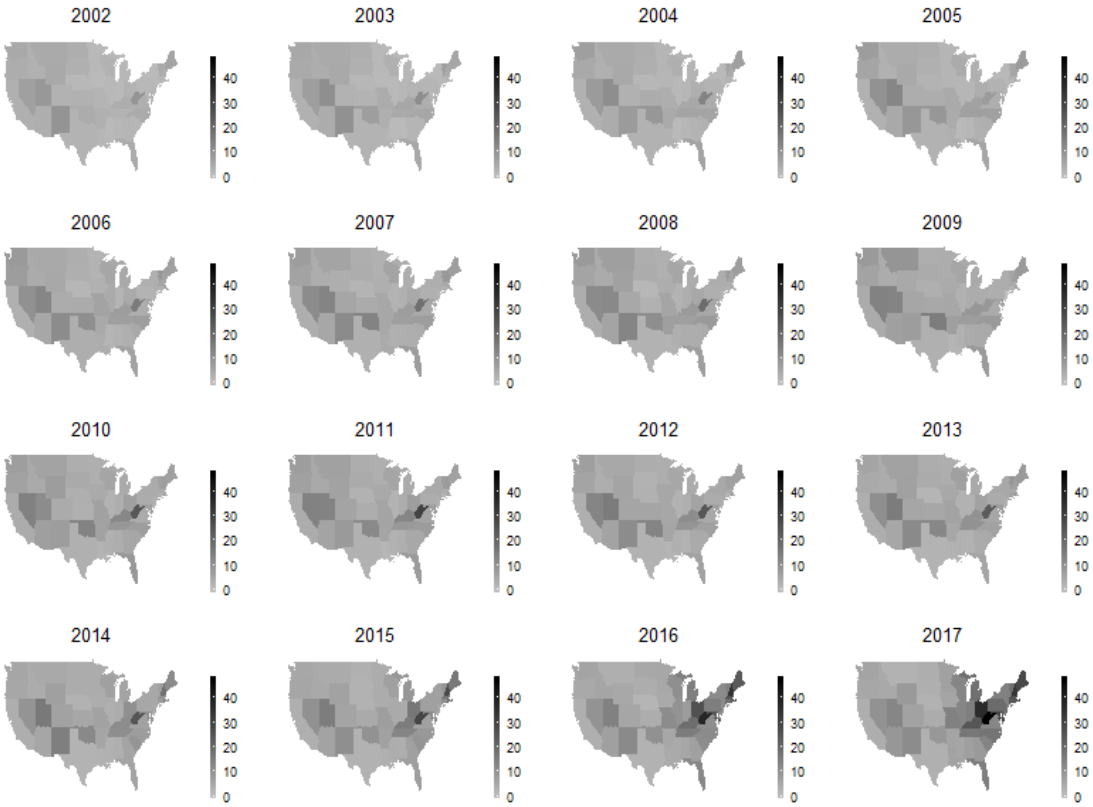
This study merges several panel data (from 1999 to 2017) from various sources. The dependent variable is Rx opioid overdose deaths per 100,000 (age-adjusted)[16] and retrieved from the National Vital Statistics System multiple cause-of-death mortality files published by the CDC. Figure 2.1 displays geographical heat maps of Rx opioid overdose death rate per 100,000 populations from 2002 to 2017.

I retrieve the list of states that require prescribers to check the PDMP before prescribing controlled substances or must-access PDMP and the PDMP enactments date from the pdaps.org website. Figure 2.2 is a visual representation of state and timing of states that enacted must-access PDMP and the state with only voluntary PMDPs.

The supply of opioids along with a health care system – that incentivizes opioid

[16]As per kff.org, the National Vital Statistics System multiple cause-of-death mortality files were used to identify drug overdose deaths. Drug overdose deaths were classified using the International Classification of Disease, Tenth Revision (ICD-10), based on the ICD-10 underlying cause-of-death codes X40–44 (unintentional), X60–64 (suicide), X85 (homicide), or Y10–Y14 (undetermined intent). Among the deaths with drug overdose as the underlying cause, Rx opioid deaths are indicated by the following ICD-10 multiple cause-of-death codes: natural and semisynthetic opioids (T40.2); methadone (T40.3); and synthetic opioids, other than methadone (T40.4). Deaths from illegally-made Fentanyl cannot be distinguished from pharmaceutical Fentanyl in the data source. For this reason, deaths from both legally prescribed and illegally produced Fentanyl are included in these data. Rates represent age-adjusted rates per 100,000 population. Natural and Semisynthetic Opioids are a category of Rx opioids that provides for natural opioid analgesics (e.g., morphine and codeine) and semisynthetic opioid analgesics (e.g., drugs such as oxycodone, hydrocodone, hydromorphone, and oxymorphone). Synthetic Opioids, other than Methadone, are a category of opioids, including drugs such as tramadol and Fentanyl. Synthetic opioids are commonly available by prescription. Fentanyl is legally made as a pharmaceutical drug to treat pain, or illegally made as a non-prescription drug and is increasingly used to intensify the effects (or "high") of other medications, such as heroin. Methadone is a synthetic opioid prescribed to treat moderate to severe pain or to reduce withdrawal symptoms in people addicted to heroin or other narcotic drugs.

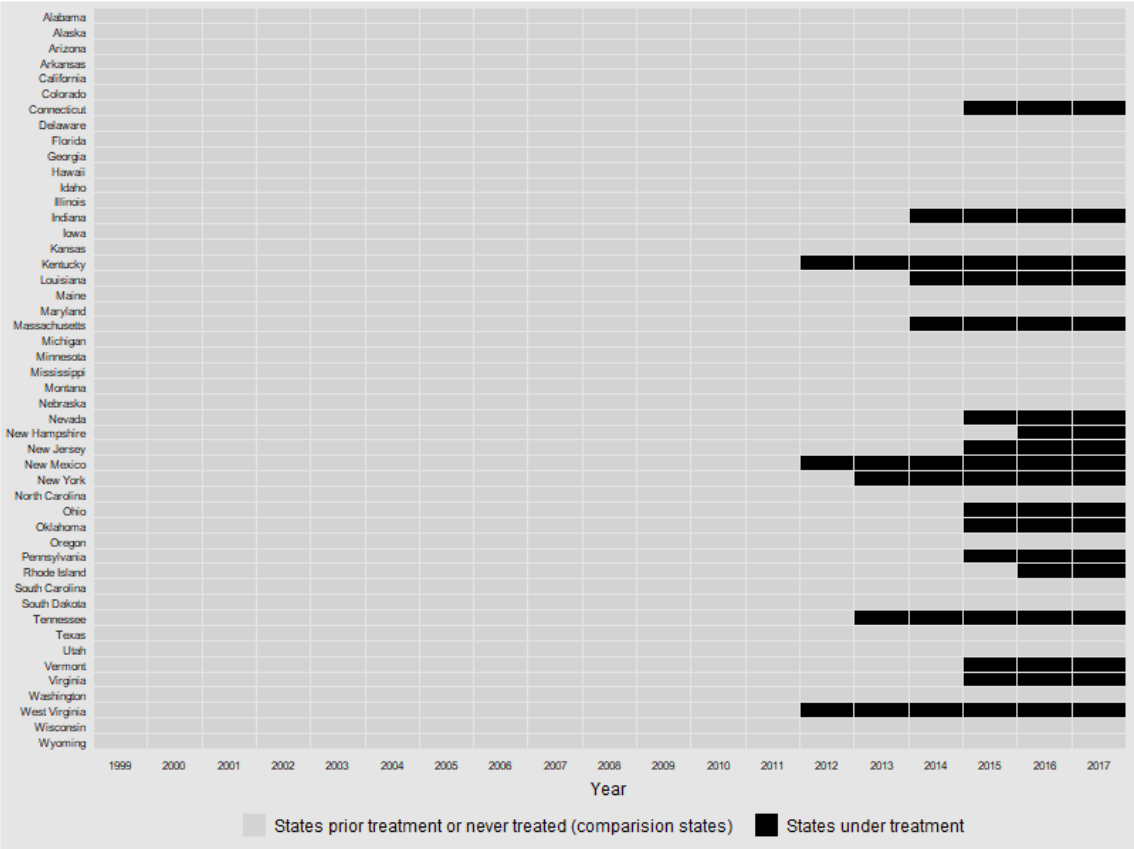
Figure 2.1: Rx Opioid-related Overdose Death per 100,000 Population



Notes: Darker intensity represents a higher Rx opioid-related overdose death rate. The intensity is fixed between 0 and 50 deaths per 100,000. This allows comparison of each state with others over the period. I exclude Alaska for scaling purpose.

prescription as a quick-fix to complex physical and mental health needs – fuels the U.S. opioid crisis, therefore, I include Morphine mg equivalents of prescribed opioids per 100,000 population in the control variable list. These quantities are available from the Drug Enforcement Administration's Automation of Reports and Consolidated Orders System (ARCOS). ARCOS reports the legal flow of control substances from the manufacturer to retails sales in the zip level and quarterly frequency. Reliance on opioid medication for physical pain, psychological trauma, concentrated disadvantages, isolation, and hopelessness – that are caused by economic and social upheaval – complicates the etiology of the U.S. opioid crisis (Dasgupta et al., 2018). Therefore controlling social and economic confounders (common causes of Rx opioid deaths and PDMPs enactment) is crucial for estimation. I retrieve several socioeconomic vari-

Figure 2.2: State Requiring Prescribers to Check the PDMP Before Prescribing Controlled Substances.



Notes: I exclude the state of Missouri because it has not enacted any form of PDMPs. Comparison states have enacted only a voluntary PDMPs. I also exclude the state of North Dakota due to the missing data. These data are retrieved from pdaps.org website.

ables from the University of Kentucky Center for Poverty Research (2019) database; Annual State-Level Measures of Human Capital Attainment database (Frank, 2009); Measures of Income Inequality database (Frank, 2014); Top Income Shares by the State of Frank, State level employment database constructed by Barry and David was created in 2002 and is updated annually. Monnat (2016) briefs opioid crisis also intertwined with political supports, so I also control some political variables like a fraction of statehouse and Senate house that is democrats.

I also use Good Samaritan Laws, Marijuana Law (medical or/and recreational possession of Marijuana), and Naloxone Access Law as indicator variables. States with the Good Samaritan Law provide immunity from prosecution for possessing a

Table 2.1: Descriptive Statistics (Pooled Across the State from 1999 to 2017)

Variables	Min	Max	Mean	Std Dev	Source
Prescription Drugs Monitoring Programs	0.00	1.00	-	-	PDAPS
Prescription opioid overdose death rate per 100,000 population	0.30	47.20	5.96	5.27	NVSS
Unemployment rate	2.30	13.70	5.65	1.97	UKCPR
Poverty rate	4.50	23.10	12.56	3.37	UKCPR
The fraction of state house that is the democrat	0.00	92.00	49.60	17.77	UKCPR
The fraction of state senate that is the democrat	0.00	100.00	48.52	19.02	UKCPR
State minimum wage (in 2014 $)	2.20	10.62	7.39	1.05	UKCPR
Employment to population (percentage)	38.51	56.12	47.66	3.47	UKCPR
High school completion (percentage)	52.63	74.84	63.95	3.93	Frank (2009)
College level completion (percentage)	10.71	30.56	18.96	4.15	Frank (2009)
Atkinson inequality coefficient	21.60	41.08	28.25	3.62	Frank (2014)
Gini inequality coefficient	52.18	71.14	59.81	3.68	Frank (2014)
Thiel inequality coefficient	0.44	1.50	0.82	0.20	Frank (2014)
Fraction of top 1% income population	0.08	20.07	2.05	3.19	Frank (2014)
Fraction of millionaires population	0.11	18.27	2.05	3.05	Frank (2014)
Log of per capita Gross Domestic Product (in thousands, 2014 $)	10.34	11.39	10.81	0.19	UKCPR
Log of per capita income (in thousands, 2014 $)	10.37	11.39	10.82	0.19	UKCPR
Share of private construction industry (percentage)	2.89	11.99	5.59	1.22	unionstats
Share of private manufacturing industry (percentage)	1.61	27.23	11.97	4.87	unionstats
Share of total public industry (percentage)	10.78	31.87	17.08	3.60	unionstats
Morphine mg equivalents of prescribed opioids per 100,000 population	0.15	52.29	11.82	9.62	ARCOS
Marijuana law (either Medical and/or recreational)	0.00	1.00	-	-	PDAPS
Naloxone access law	0.00	1.00	-	-	Procon.org
Good samaritan law	0.00	1.00	-	-	PDAPS

Notes: The state of Nebraska don't have state upper and lower house. Missing value imputation were based on the weighted moving average and performed separately for each state.

controlled substance while seeking help for himself or another person experiencing an overdose. The state with Naloxone Access Law provides naloxone and other opioid overdose prevention services to individuals who use drugs, their families and friends, and service providers, including education about overdose risk factors, signs of overdose, appropriate response, and administration of naloxone. As of 2016, 48 states have authorized some variant of a naloxone access law, and 37 states have passed a drug overdose good samaritan law (Ayres and Jalal, 2018).

Table 2.1 displays the list of variables, their transformation, units, data sources, and summary statistics. The summary statistics comprise the minimum, maximum, mean, and standard deviation for each variable. Each variable is pooled across time and state.

2.6 Results

2.6.1 Main Results

Table 2.2 presents the estimated impact of the PDMP on the age-adjusted Rx opioid overdose death rate per 100,000 population utilizing difference-in-difference (DID), event study and generalized synthetic control with interactive fixed-effect (GSC) methods. When applicable, each of these methods use the double-selection post-LASSO method to select the confounders.

Table 2.2: Impact of Must-access PDMPs on Age-adjusted Rx Opioid Overdose Death Rate per 100,000 Population

VariablesVariablesVariablespt¡ -Variablespt¿	DiD		Event study		GSC	
	(1)	(2)	(3)	(4)	(5)	(5)
ATT	5.45***(1.58)	4.31***(1.42)				
ATT (Average)			6.05***(1.65)	4.95***(1.78)	-0.91(2.39)	-0.82(2.92)
High school		0.29***(0.11)		0.12(0.11)		-0.05(0.05)
Marijuana law		2.01***(0.94)		1.77*(1.14)		-0.27(0.43)
Medicare expansion		1.31(1.09)		-0.94(1.13)		-0.72(0.53)
Fraction of millionaires		-0.18(0.63)		-0.19(0.59)		-0.15(0.36)
Naloxone access law		1.12(1.11)		2.33***(1.07)		-0.57(0.40)
Private manufacturing industry		-0.04(0.18)		-0.23(0.13)		-0.03(0.07)
Morphine		0.07(0.08)		0(0.08)		0.01(0.03)
Intercept	5.5***(0.41)	-13.93(8.19)				
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
DSPL	-	22	-	22	-	22
Factor	-	-	-	-	1	1
Observation	950	950	950	950	950	950
Treated states	18	18	18	18	18	18
Control States	30	30	30	30	30	30

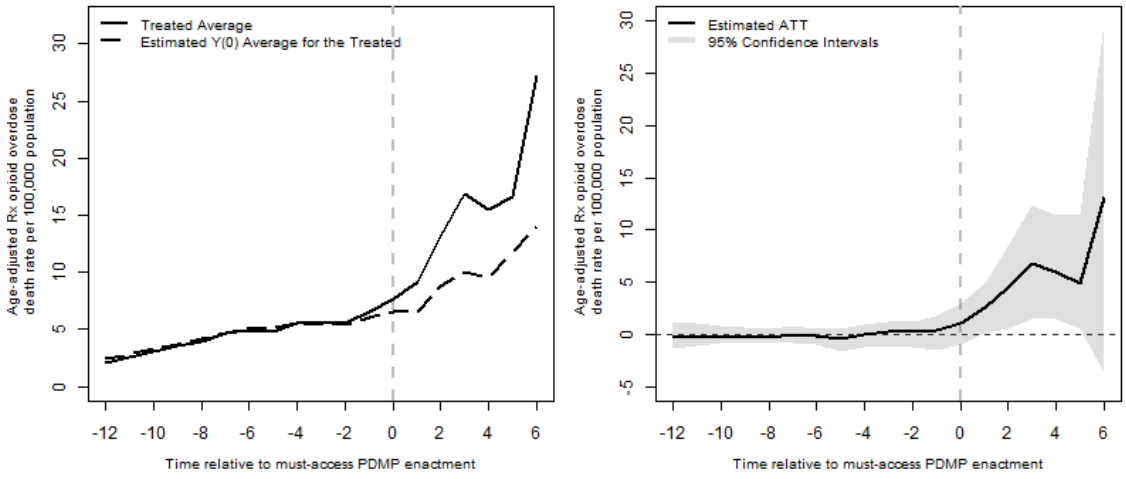
Notes: All the model comprises of state and year fixed effects. Standard errors are based on non-parametric bootstraps (blocked at the state level) of 2,000 times. Controls are selected, implementing the double post-LASSO selection method. Standard errors are enclosed in parenthesis. The 1%, 5%, and 10% level of significance are given as ***, **, and *, respectively. DSPL shows the number of variables that are feed to the double-selection post-LASSO method. In this table, the double-selection post-LASSO method was performed using 24 different contemporaneous covariates. Variable Morphine represents Morphine mg equivalents of prescribed opioids per 100,000 population. The event study and generalized synthetic control regressions are weighted based on the relevant state population.

Estimates presented in columns (1) and (2) are the standard two-way fixed effect model, also known as DID in the literature. The estimates in column (1) only include the indicator of must-access PDMP, while column (2) contains additional controls, these controls are selected utilizing the double-selection post-LASSO method. On average, PMDP enacting states have 5.45 and 4.31 additional age-adjusted Rx opioid

overdose death rates per 100,000 population, compared to comparison states with only voluntary PDMP.

The identification strategy of DID is the "parallel trend", I relax this assumption. In other words, I estimate the effect of must-access PDMP on Rx opioid overdose death in the posttreatment period by subtracting the time intercepts estimated from the control group and the unit intercepts based on the pretreatment data. The predict Rx opioid overdose death for state i in year t, therefore, is the summation of unit intercept i and time intercept t, plus the impact of the time-varying covariates. The column (3) and (4) exhibits the average of ATT and Figure 2.3 provides a visualization. These regressions are weighted based on the population size of relevant states.

Figure 2.3: Estimated Impact of Must-access PDMP on Age-adjusted Rx Opioid Overdose Death Rate per 100,000 Population for Years Before, During, and After Adoption, (Based on Event Study)



The left panel of Figure 2.3 shows the average actual age-adjusted Rx opioid overdose death rate per 100,000 population (solid line) and average predicted age-adjusted Rx opioid overdose death rate per 100,000 population in the absence of must-access PDMP laws (dashed line); both averages are taken based on the number of terms since (or before) must-access PDMP laws first took effect. The right panel of Figure 2.3 shows the gap between the two lines or the estimated ATT. The confidence

intervals are derived from the standard errors, which are based on non-parametric bootstraps (blocked at the state level) of 2,000 times. It is clear from both figures that the "parallel trends" assumption is not likely to hold since the average predicted Rx opioid overdose death deviates from the average actual Rx opioid overdose death in the pretreatment periods.

These estimates presented in column (1) to column (4) may be contaminated by measurement error, mainly in the dependent variable. As per the CDC, among the deaths with drug overdose as the underlying cause, prescription opioid deaths are indicated by the following ICD-10 multiple cause-of-death codes: natural and semisynthetic opioids (T40.2); methadone (T40.3); and synthetic opioids, other than methadone (T40.4). Deaths from illegally-made Fentanyl cannot be distinguished from pharmaceutical Fentanyl in the data source. For this reason, deaths from both legally prescribed and illegally produced Fentanyl are included in these data.

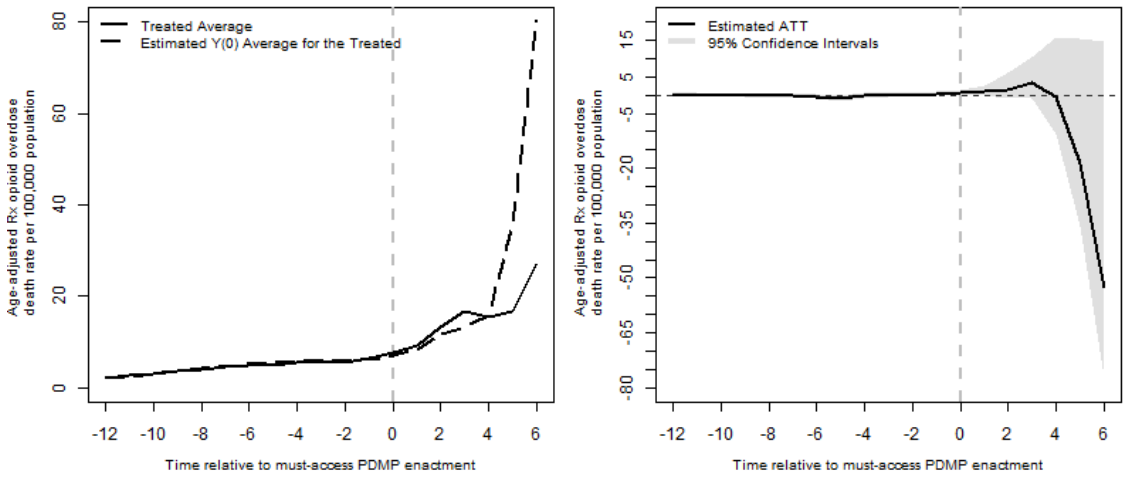
Literature establishes that PDMPs being a supply-side policy, opioid abusers, and dependent users may substitute the Rx opioid with cheap substitutes like illegally manufactured Fentanyl or illegal heroin. (CDC, 2019) reports such substitution led to another nationwide crisis known as the third wave of the opioid crisis and illicit manufactured Fentanyl are adulterated with counterfeit pills and heroin which are highly potent, less bulky and – that are sourced primarily from China, Mexican drug trafficking organizations and disseminate using cryptocurrencies through internet (Beletsky and Davis, 2017).

The estimates presented in Table 2.2 cannot distinguish deaths from legally prescribed and illegally produced Fentanyl. Therefore, the estimates possibly incorporate the total effect of must-access PDMPs on intended Rx opioid overdose deaths as well as the third wave of the opioid crisis (an unintended substitution effect). The positive significant coefficient suggests possibly unintended substitution effect surpasses the intended impact of PDMPs on the Rx opioid overdose death rate. One way to deal with such a situation is to implement an interactive fixed-effect model, which can help to control for the underlying nationwide time trends in Rx opioid death rate (if

such pattern exists). The next section explores such a possibility.

Next, I apply the GSC method, which is similar to the synthetic control method but allows multiple treatment units and also allows the possibility of interactive fixed effects. Table 2.2 columns (5) and (6) reports the estimation from the GSC method. Again, both specifications impose additive state and year fixed effects. In column (5), no covariates are included, while in column (6), controls are selected, implementing the double-selection post-LASSO method. These regressions are weighted based on the population size of relevant states. The cross-validation scheme finds one unobserved factor to be important and ater conditioning on both the factors and additive fixed-effects. The estimated ATT is -0.91 and -0.82 and insignificant. These estimates suggest that must-access PDMP state laws are not associated with an increase in Rx opioid overdose death. Figure 2.4 provides a visualization.

Figure 2.4: Estimated Impact of Must-access PDMP on Age-adjusted Rx Opioid Overdose Death Rate per 100,000 Population for Years Before, During, and After Adoption, (Based on Generalized Synthetic Control Study)



The left panel of Figure 2.4 shows averages taken ater the actual and predicted Rx opioid overdose death rates are realigned to the timing of the must-access PDMP enactment. With the GSC method, the average actual Rx opioid overdose death and average predicted Rx opioid overdose death match well in pretreatment periods, and diverged ater must-access PDMP laws took effect. The right panel of Figure 2.4

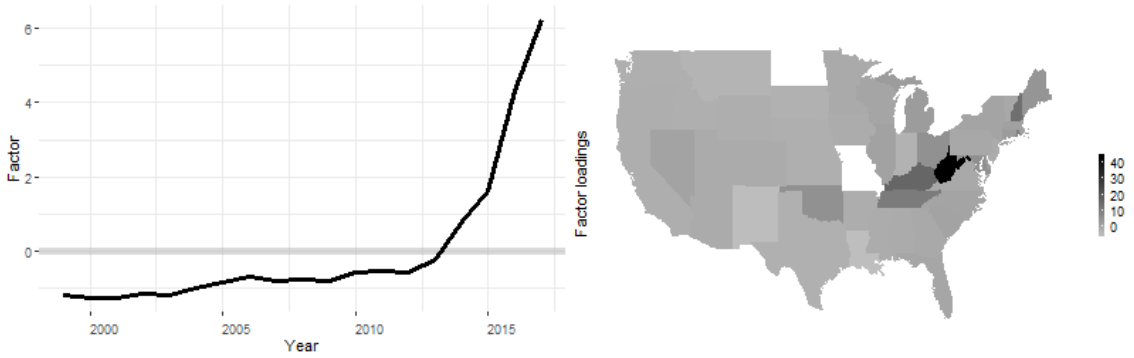
shows that the gaps between the two lines are flat in pretreatment periods, and the results shoot downward right ater few years of the adoption of "must-access" PDMP. Yet, such a relationship is not statistically significant.

2.6.2 A Nationwide Time Trends in Rx Opioid

The estimates presented in 2.2 columns (5) and (6) requires an in-depth interpretation because the GSC includes the factor. Figure 2.5 shows factor in left panel. The x-axis is a year, and the y-axis is the magnitude of factors (re-scaled by the square root of their corresponding eigenvalues to demonstrate their relative importance). Bearing in mind the caveat that estimated factors might not be directly interpretable because they are, at best, linear transformations of the true factors, I find that the estimated factors shown in this figure are meaningful as this factor correlates with the third wave of the opioid crisis known as the synthetic opioid crisis. In simplest, the factor can be thought of as nationwide time trends in Rx opioid, in which different states are either more or less susceptible, depending on the unobservable characteristics of those states. A widely used strategy is to add in unit-specific linear or quadratic time trends to conventional two-way fixed effects models. For example, Grecu et al. (2019) imposes a quadratic time trend to their two-way fixed effect model to examine the impact of opioid abuse among young adults; and Mallatt (2018) implements linear, quadratic and cubic time trends to estimate the effect of PDMP on heroin incidents. The basic difference-in-difference model accounts for national non-linear patterns in Rx opioid overdose deaths, and the GSC factor model extends this by accounting for additional non-linear time trends that affect areas to varying degrees. This factor gradually increases in Rx opioid overdose deaths from 1999-2012, which then increases exponentially from 2013-2015. States experience the non-linear increase in Rx opioid overdose deaths to differing degrees, which is accounted for in each states' factor loading. In the case of Rx opioid overdose deaths, a state's factor correlated with the third wave of the opioid crisis known as synthetic opioid crisis (CDC, 2019), implying that Rx opioid overdose deaths-dense states are more sensitive to the third wave of

the opioid crisis that was triggered particularly those involving illicitly-manufactured Fentanyl. This is consistent with the hypothesis that restricting Rx opioids causes opioid abusers toward another illicit opioid, in this case, that could be illicit Fentanyl.

Figure 2.5: Factor and Factor Loadings



Notes: I exclude the state of Missouri, because it has not enacted any form of PDMPs. Comparison states have enacted only a voluntary PDMPs. I also exclude the state of North Dakota due to the missing data. Factor loading are enclosed in parenthesis with descending order as: West Virginia (43), Kentucky (16.2), New Hampshire (13.9), Tennessee (10.9), Rhode Island (10.1), Maryland (6.1), Oklahoma (5.6), Ohio (5.5), Maine (4.7), Connecticut (3.1), Delaware (3.1), Michigan (3), Massachusetts (1.8), Illinois (1.5), Nevada (1.4), South Carolina (1.2), North Carolina (1.2), New York (1), Wisconsin (0.9), Florida (0.7), Vermont (0.2), Pennsylvania (0.1), Georgia (0), Virginia (-0.1), Minnesota (-0.2), New Jersey (-0.4), Arizona (-0.4), Alaska (-0.5), Utah (-0.5), Alabama (-0.6), Colorado (-0.6), Iowa (-0.7), Mississippi (-0.8), Arkansas (-0.9), Oregon (-1.2), Wyoming (-1.3), California (-1.3), Idaho (-1.3), Nebraska (-1.5), Washington (-1.5), Kansas (-1.5), Hawaii (-1.6), Texas (-1.6), South Dakota (-1.8), Indiana (-2), Montana (-2.6), New Mexico (-4.7), Louisiana (-4.9)

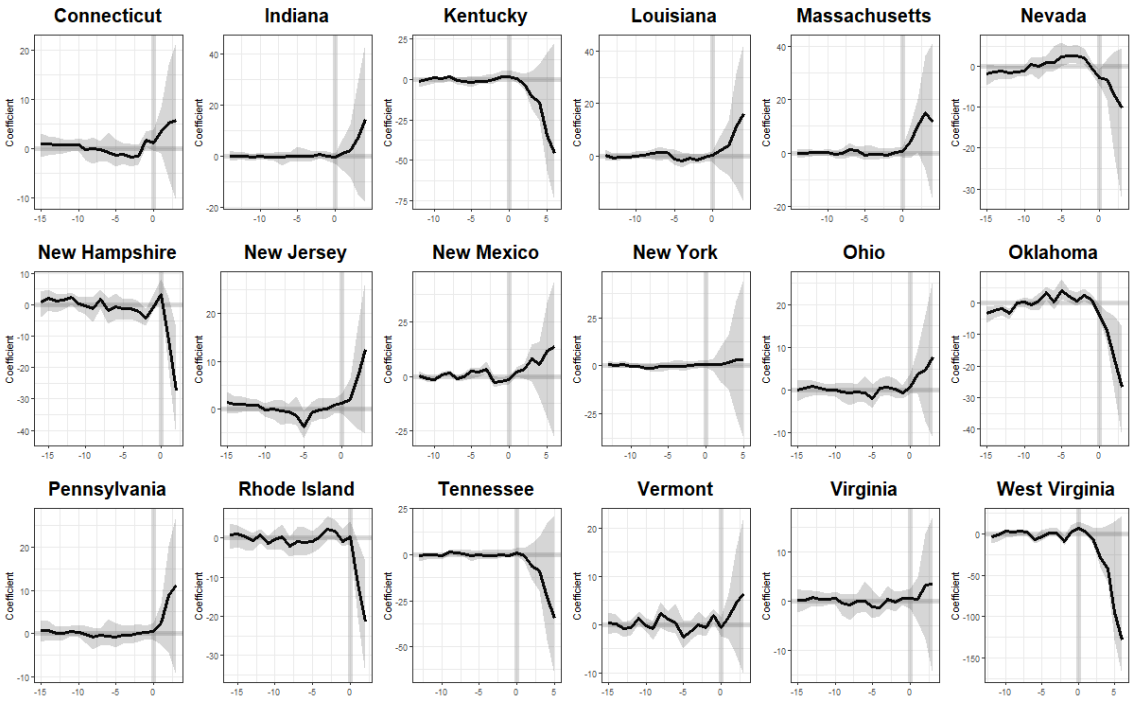
Figure 2.5 shows factor loadings in right panel. Factor loading exhibits severity of the state's experience of the non-linear increase in Rx opioid deaths (or the factor which correlates with synthetic opioid crisis) to differing degrees, which is accounted for in each state. The states with darker colors are more susceptible to the factor.

2.6.3 State-level Impact of Must-access PDMPs

Next, Figure 2.5 shows state-level impact of must-access PDMPs. Note to interpret these plots; we should keep track of the factor loading. The PDMPs seems to reduce the Rx opioid overdose deaths among the state of West Virginia, Kentucky, New Hampshire, Tennessee, Rhode Island, Oklahoma but these states also have higher factor loading – suggesting that possibly these states suffer higher unintended conse-

quences where Rx opioid overdose deaths are substituted by the deaths from the third wave of the opioid crisis. The rest of the states presented in Figure 2.5 shows that PDMPs are ineffective in reducing Rx opioid deaths, however, the state of Nevada, despite having lower factor loading, seems to reduce the Rx opioid death successfully.

Figure 2.6: State-level Impact of Must-access PDMPs



Notes: To interpret these plot, we should keep track of the factor loading for each PDMP law abiding state.

2.6.4 Validity: Consistency using High Dimensional Covariates

One potential question arises regarding the control variables. Causal interpretation relies on the belief that there are no higher-order terms of the control variables, no interaction terms, and no additional excluded variables that associate with the PDMPs and Rx opioid overdose deaths. Thus, controlling a large set of variables seems desirable to make this assumption plausible. However, naively controlling redundant variables reduces the ability to distinguish the impact of interest variables

and, consequently, produces less precise estimates. Further, literature considers utilizing lagged control rather than contemporaneous control mainly to avoid the potential reverse causality.

Table 2.3: Impact of Must-access PDMPs on Age-adjusted Rx Opioid Overdose Death Rate per 100,000 Population (Variables Selection on High Dimensional Covariates)

	DiD		Event study		GSC	
	(1)	(2)	(3)	(4)	(5)	(6)
ATT	5.35***(1.55)	3.78***(1.27)	-	-	-	-
ATT (Average)	-	-	5.91***(1.63)	4.7(2.64)	-1.23(2.26)	-2.42(3.07)
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
DSPL	-	324	-	324	-	324
Factor	-	-	-	-	1	1
Observation	900	900	900	900	900	900
Treated states	18	18	18	18	18	18
Control States	30	30	30	30	30	30

Notes: All the model comprises of state and year fixed effects. Standard errors are based on non-parametric bootstraps (blocked at the state level) of 2,000 times. Controls are selected, implementing the double post-LASSO selection method. Standard errors are enclosed in parenthesis. The 1%, 5%, and 10% level of significance are given as ***, **, and *, respectively. DSPL shows the number of variables that are feed to the double-selection post-LASSO method. In this table, the double-selection post-LASSO method was performed using the first lag, the second-order polynomial of first lag, all the possible interaction between first lag variables, in total, there are 24+24+24*23/2=324 different possible covariates. The regressions (2), (3), (4), and (5) are weighted based on the relevant state population.

In this regard, I have 24 contemporaneous covariates; instead of these contemporaneous covariates, I took the first lag of these variables. Next, I allow second-order polynomial or quadratic of these lagged variables to account for the possible non-linear relationship. Further, to allow the possible interaction of controls, I took all the feasible controls. Then to select the adequate controls, I implement the double-selection post-LASSO method using the first lag, the second-order polynomial of first lag, all the possible interaction between first lag variables. In total, there are 24+24+24*23/2=324 different potential covariates. Table 2.3 shows the estimates which are very similar to the estimate I presented in the main results in Table 2.2 therefore provides validity of effect as the results have consistency across high dimensional covariates.

2.7 Discussion and Conclusion

The results in the previous section are consistent and have relevance in the policy analysis in the regional settings. The GSC approach unifies the synthetic control method with interactive linear fixed-effects models under a simple framework, of which DID is a particular case (Xu, 2017). In short, this paper concludes that, on average, the effect of "must-access" PMDPs to reduce Rx opioid overdose deaths are heterogeneous. States, where the "must-access" PMDPs seem to reduce Rx opioid overdose deaths (mainly West Virginia, Kentucky, New Hampshire, Tennessee, Rhode Island, Oklahoma), is heavily affected by additional non-linear time trends that correlate with the third wave of the opioid crisis. The rest of the states, except for Nevada, the "must-access" PMDPs, seem unsuccessful, and these sates are mildly affected by additional non-linear time trends. In aggregate, the PDMPs do not save lives, mainly due to the third wave of the opioid crisis.

We present some discussions on some of the obvious questions that the reader may have. First is why we choose to discuss the Rx opioid-related overdose deaths and not the Rx rates or other overdose deaths and what are some caveats of the dependent variable. Several papers discuss the impact of PDMPs on the Rx rate. We think that the effect of PDMPs on the prescription rate is evident that the PDMPs leads to a reduction of prescription rates. However, there may be some heterogeneity (Ayres and Jalal, 2018).

Literature finds the opioid Rx rate declines after PDMPs, but the trend of Rx opioid overdoses is rising. This phenomenon could represent either that the Americans are reporting more pain (which is not the case), or the opioid user is using more of other opioid drugs (possible heroin/Fentanyl), and the overdose occurred due to Rx opioid. As per the CDC, among the deaths with drug overdose as the underlying cause, prescription opioid deaths are indicated by the following ICD-10 multiple cause-of-death codes: natural and semisynthetic opioids (T40.2); methadone (T40.3); and synthetic opioids, other than methadone (T40.4). Deaths from illegally-made

Fentanyl cannot be distinguished from pharmaceutical Fentanyl in the data source. But simple economic intuition suggests that a reduction of prescription opioids would lead to higher demand for substitutes like heroin and Fentanyl.

This paper shows evidence regarding the unintended consequence of the PDMPs using the interactive fixed-effect model. Readers might get concerned about endogeneity as the PDMPs were policy responses to the prescription-related opioid overdose deaths. I argue that the states with high opioid-related overdose death might enact "must-access" PDMPs, but once the PDMPs are passed, the feedback of high opioid-related overdose death to reenact "must-access" PDMPs is not possible. However, I also provide an additional analysis with a lagged variable and high dimensional list of covariates to control two different sources of endogeneity: reverse causality and omitted variables biases. The results hold validity.

Secondly, I discuss why this paper finds evidence of the ineffectiveness of PDMP in generalize synthetic control and not in a simple Difference-In-Difference framework. The DID framework assumes "parallel trend" or the average outcomes of the treated and control units follow parallel paths in pretreatment periods. Due to the unobserved time-varying confounding effect, the parallel trend assumption is not directly testable, and visual detection of the parallel trend is also most likely not to hold. GSC method captures unobserved time-varying confounding effects. At the same time, GSC allows the interactive fixed effect to potentially capture the unobserved heterogeneity. I argue that GSC absorbs the third wave of the opioid crisis, mainly the switching of prescription opioids to the illicit Fentanyl, which is an unintended consequence of PDMPs. DID exhibits the estimates with both intended and unintended consequences of PDMPs, while GSC estimates teasel out an intentional and unintentional effect of PDMPs.

Third, I discuss the meaning of the unobserved time-varying confounding effect or the factor. The factor captures nationwide time trends in prescription opioid-related overdose deaths to which different states are either more or less susceptible, depending on the unobservable characteristics of those states. The factor correlates with

the third wave of the opioid. Therefore, this factor potentially captures a nation-wide trend of prescription opioid switching toward illicit Fentanyl as the unintended consequence. Even thou, we don't know the source of switching behavior, but the literature suggests Oxycotin reformulation or other supply-side policy that restricts the prescription opioid, or drug lords are moving toward the suburb. Finally, to conclude, the Rx opioid deaths from illegally-made Fentanyl cannot be distinguished from pharmaceutical Fentanyl in the data source, therefore to study the impact of PDMPs on Rx opioids is obscure. However, there is clear evidence that abusers possible switch to cheaper opioid alternatives like Fentanyl.

Chapter 3

Heterogeneous Treatment Effects of Medicaid and Efficient Policies

3.1 Introduction

As of September 20, 2019, 37 states and the District of Columbia have expanded Medicaid coverage for low-income adults to 138%[1] of the Federal Poverty Level through the Affordable Care Act (ACA). This optional[2] provision to expand the Medicaid program through the ACA has triggered a substantial nationwide debate among policymakers and diverse stakeholders about what effects, if any, Medicaid has on the various dimensions of health (Baicker, 2019).

Finkelstein et al. (2012) use random assignment of Medicaid, employing the Oregon Health Insurance Experiment (OHIE) dataset, and found mixed-bag effects[3] of

[1]Medicaid income eligibility limits for adults as a percent of the Federal Poverty Level, indeed, are different from states to states. Kaiser Family Foundation (2019a) provides a table for the state by state Medicaid income eligibility levels for adults.

[2]Following the June 2012 Supreme Court decision, states face a decision about whether to adopt the Medicaid expansion. But, as per the Centers for Medicare and Medicaid Services (CMS) guidance, there is no deadline for states to implement the Medicaid expansion (Kaiser Family Foundation, 2019b).

[3]Finkelstein et al. (2012) use OHIE data set, and found that, in the year following the random assignment of lottery Medicaid, the treatment group had higher health care use, lower out-of-pocket medical expenditures and medical debt, and better self-reported physical and mental health than the control group, but did not reflect any detectable improvements in physical health conditions like high blood pressure.

Medicaid which have presented policymakers with tough choices in balancing the costs and benefits of Medicaid (Baicker, 2019). Meanwhile, states like Florida, Minnesota, and North Carolina are analyzing their Medicaid programs to find potential savings, some of which could be redirected to improve access and the quality of care to patients served by the Medicaid program (Rueben, 2019). Furthermore, another significant reform in Medicaid is the "Medicaid work requirements[4]," which take away Medicaid coverage from people not engaging in work or work-related activities for a specified number of hours each month (Katch et al., 2018).

The mixed-bag effect of Medicaid and policymakers' quest for Medicaid reforms are the main motivations of my research. In this paper, I provide answers to the questions of "Why previous literature finds the mixed-bag effect of Medicaid?", and "How to think about Medicaid reforms while improving the effectiveness of Medicaid?" To answer these research questions, I use the Oregon Health Insurance Experiment (OHIE) public-use data. This data set comprises the lottery assignment of Medicaid in Oregon, thus creates a randomized controlled study setting and allows causal analysis by comparing various outcomes of the lucky Oregonians who received Medicaid to those who did not (Klein, 2013). The primary rationale to use OHIE data is that random assignment of Medicaid allows circumventing the challenges of endogeneity. Endogeneity arises because it is difficult to control[5] for observed and unobserved confounding[6] variables among the insured and uninsured population (Levy and Meltzer, 2008).

Answers of the above research questions can contribute to two primary domains that are relevant for policy development. First, unlike the series of papers[7] that

[4]Centers for Medicare Medicaid Services (CMS) guidance for state Medicaid waiver proposals, issued on January 2018, allows states, for the first time, to impose work requirements in Medicaid as a condition of eligibility. As a result, several states have received approval for or are pursuing these waivers. See Garfield et al. (2019) for details.

[5]For example, a comparison of the health between those with and without health insurance,(say the Medicaid) can reveal that Medicaidinsurance is detrimental for one's health because people with poor health are more likely to get insurance compared to healthy people (Baicker and Finkelstein, 2011).

[6]Confounding variables are common causes that explain both treatment and outcome variables.

[7]See Allen et al. (2010); Baicker et al. (2013, 2017, 2014); Baicker and Finkelstein (2011); Finkel-

have evaluated the average treatment effects of Medicaid, I contribute by estimating the heterogeneous treatment effect of lottery Medicaid, employing the Athey et al. (2019) cluster-robust generalized random forest, on several outcome variables. These outcomes variables are health care utilization, preventive care utilization, financial strain, self-reported physical and mental health, and several variables of potential mechanism to improve health. The primary rationale to understand the heterogeneous treatment effects is that the identical policy intervention can often distinctly affect different individuals and subpopulations in different ways. Along with average treatment effects, policymakers are usually interested in how the effects of intervention vary across subpopulations. Identifying the heterogeneous treatment effects accommodate the discovery of underlying mechanisms that drive the results and allow for efficient design and reform of policy.

Second, I contribute insights regarding how to target health insurance interventions for effective policymaking using the Athey and Wager (2019a) strategies of "efficient policy learning." Understanding "who should be treated" with intervention is ubiquitous in policymaking. It can be unfair, unethical, and sometimes illegal to target policy at only a particular subpopulation. Moreover, intervening everybody in the population (a blanket policy) is welfare-maximizing but can be costly.[8] The main logic of efficient policy learning is to identify treatment assignment policies based on easily observable individual characteristics. The treatment assignment, in this paper, represents Medicaid assignment.

To investigate the heterogeneous treatment effects, one can stratify the data in mutually exclusive groups or include interactions within a regression (Athey and Imbens, 2017a). However, for large-scale investigations of effect heterogeneity, $p-$values

stein et al. (2012); Grossman et al. (2016); Taubman et al. (2014); Zhou et al. (2017).

[8]For example, a provision of the Affordable Care Act (ACA) was that the federal government would pay the full cost of coverage expansion through 2016. Moreover, it would reimburse at least 90% of the cost of covering the newly-insured population (Norris, 2018). Oregon responded to this incentive by expanding Medicaid in January 2014 and ensured insurance to everyone with incomes up to 133% of the federal poverty line. When the federal government gradually reduced their payments, the state budget of Oregon (nearly $74 billion for 2017-2019) suffered about $1 billion budget hole due to the cost of health care (Foden-Vencil, 2018).

of standard "single" hypothesis tests are no longer valid because of the multiple hypothesis testing[9] problems (Lan et al., 2016; List et al., 2019). Moreover, performing ad-hoc searches or $p-$hacking[10] to detect the responsive subgroups may lead to false discoveries or may mistake noise for an actual treatment effect (Davis and Heller, 2017). To avoid many of the issues associated with data mining or $p-$hacking, researchers can commit in advance to study only a subgroup by a preregistered analysis plan.[11] However, this may also prevent discovering unanticipated results and developing new hypotheses (Athey and Imbens, 2016).

I implement the cluster-robust generalized random forest methods, developed by the Athey et al. (2019), on the OHIE dataset to explore the heterogeneous treatment effects of Medicaid. The Athey et al. (2019) method re-engineers the strengths and innovations of the Breiman (2001) random forest[12], a predictive machine learning method for causal inference. The Athey et al. (2019) modifications[13] allow for a systematic investigation of the heterogeneous treatment effects that are not prone to data mining and $p-$hacking, and useful when research includes high-dimensional

[9]The "multiple hypothesis testing problems" leads to the so-called "ex-post selection problem," which is widely recognized in the program evaluation literature. For example, for fifty single hypotheses tests, the probability that at least one test falsely rejects the null hypotheses at the 5% significance level (assuming independent test statistics as an extreme case) is $1 - 0.95^{50} = 0.92$ or 92%.

[10]The $p-$hacking is an exhaustive search for statistically significant relations from combinations of variables or combinations of interactions of variables or subgroups. The $p-$hacking could lead to discovering the statistically significant relationship, when, in fact, there could have no real underlying effect.

[11]A preregistered analysis plan is sets of analyses plans released in the public domain by the researchers in advance prior they collect the data and learn about outcomes. For example, The American Economic Association's registry for randomized controlled trials is a reputable platform for conducting a preregistered analysis plan.

[12]The Breiman (2001) random forest ensembles or bootstrap and aggregate many classifications and regression tree (CART) of the Breiman et al. (1984), and report the average. The CART recursively filters and partitions the large dataset into binary sub-groups (nodes) such that the samples within each subset become more homogeneous in their fit of the response variable, thus resulting in a tree-like format.

[13]The modifications are based on the "causal tree" (Athey and Imbens, 2016), "causal forest" (Wager and Athey, 2018) and the "generalized random forest" (Athey et al., 2019) methods. The "causal tree" approach re-engineers the Breiman et al. (1984) classification and regression tree (CART), a machine learning algorithms for causal inference. The remaining methods extend the "causal tree" approach utilizing the Breiman (2001) random forest machine learning algorithm for causal inference.

covariates.[14] Furthermore, OHIE provides individual-level data set, but the Medicaid lottery intervention occurred at household-level; therefore, the outcome variable may be arbitrarily correlated within a household. The Athey et al. (2019) allows a conservative approach of cluster-robust analysis to account for potential correlations within each household cluster.

Along with the heterogeneous treatment effects, the question of: "Who should get treatment?" is also a widespread issue in policy design. For example, who should get in youth employment programs (Davis and Heller, 2017), who should get Medicare funding for hip or knee replacement surgery (Kleinberg et al., 2015), who should get a job training, job searching support, and other assistance (Kitagawa and Tetenov, 2018). My paper implements the "efficient policy learning" strategies of Athey and Wager (2019a) to answer how to set eligibility criteria to intervene with Medicaid coverage. The Athey and Wager (2019a) approach allows identifying policy changes/reforms that prioritize providing Medicaid coverage to the subgroups that are likely to benefit the most.

I show efficient policy rules considering two rationales − first, I exclude observable covariates like race, gender, and residence. Excluding these covariates are essential to allow ethical, legislative, and political considerations of policy design. Second, I follow the Kitagawa and Tetenov (2018) approach to design policy from an "intent-to-treat" perspective. This approach is crucial because the policy maker's problem is only a choice of the eligibility criteria and not the take-up[15] rate.

I find the heterogeneous treatment effect of Medicaid on health care utilization, preventive care utilization, financial strain, self-reported physical and mental health, and several variables of potential mechanism to improve health. For each of these outcome variables, I display the causal thresholds for distinct subpopulations where

[14]The nearest-neighbor matching, kernel methods, and series estimation are classical approaches for nonparametric estimation of heterogeneous treatment effects (Crump et al., 2008; Lee, 2009; Willke et al., 2012), and performs well with a smaller set of covariates. However, these classic approaches break down quickly when covariates are large in numbers (Athey and Wager, 2019a).

[15]The take-up rate in our study is the percentage of eligible people who accept Medicaid benefits. Individuals decide the take-up rate for various reasons unknown to the policymakers.

the impacts of Medicaid intensify and subdue. These realms have not been explored earlier, and my results are some unique contributions to the literature. My findings, therefore, provide a holistic perspective toward the large, and at times contradictory research exploring the effects of Medicaid on health. I find that the heterogeneous impacts of Medicaid are more pronounced among poorer and older non-elderly households. These impoverished families may need more medical services, and when Medicaid provides an opportunity, these households utilize more health care compared to those who are uninsured, just as standard adverse selection theory would predict.

Furthermore, I find efficient policies or reforms for several selected outcome variables. I quantify the cost of estimated policy rules in comparison to the random assignment of Medicaid. On average, the proposed reforms would improve the average probability of outpatient visits, preventive care use, overall health outcomes, having a personal doctor and clinic, and happiness by a range of 2% to 9% over a random assignment baseline, and these improvements are likely to support a causal interpretation.

In summary, I use the Oregon Health Insurance Experiment public-use data and contribute to examining the sources of treatment heterogeneity on Medicaid programs and offering efficient policy rules or reforms that prioritize Medicaid allotments to subgroups that are likely to benefit the most. The findings of this paper are useful for analysts, policymakers, and insurance designers to discover the underlying mechanisms that drive the health outcome results and to design or reform policy. For example, the proposed reforms can help Oregon to develop a priority list against current blanket Medicaid policy which can help to reduce the state budget-deficits[16] without hampering the current Medicaid welfare.

Section 3.2 summarizes the institutional background of the Oregon Health Insurance Experiment. Section 3.3 summarizes approaches to study health insurance and health outcomes and explains how causal machine learning can help to analyze

[16]The federal government started to defund Oregon's Medicaid Expansion from 2016 which has led to a budget deficit and Oregon Measure 101 − a two-year budget fix to close state budget deficit by taxing hospital and insurance agencies − is nearing to end in 2020.

different research questions. Section 3.4 lays out identification strategy and empirical methods for the cluster-robust random forest for heterogeneous treatment estimation along with efficient policy learning strategies. Section 3.5 displays the results. Section 3.6 provides discussions on findings and concludes the study.

3.2 Oregon Health Insurance Experiment

Oregon's Medicaid program, the Oregon Health Plan (OHP), created by one of the first federal waivers of traditional Medicaid rules, has two separate parts. First is the "OHP Plus." It serves low-income children, pregnant women, welfare recipients, and poor elderly and disabled populations groups who are categorically eligible Medicaid populations in Oregon (Office for Oregon Health Policy and Research, 2009). Second is the "OHP Standard." It servers poor adults who are financially but not categorically eligible for the Plus program. Eligibility for the Standard plan is limited to adults ages 19−64 who are Oregon residents and U.S. citizens or legal immigrants, and have incomes below the 100% federal poverty level and/or who have been without health insurance for at least six months, and/or have less than $2,000 in assets (Office for Oregon Health Policy and Research, 2009; Allen et al., 2010).

Except for vision and non-emergency dental services, the OHP Standard provides relatively comprehensive benefits with no consumer cost-sharing. The OHP Standard coverage includes physician services, prescription drugs, all significant hospital benefits, behavioral health, and chemical dependency services (including outpatient services), hospice care, and some durable medical equipment (Finkelstein et al., 2012; Baicker and Finkelstein, 2011). In 2001−2004, the average annual Medicaid expenditures for an individual on the OHP Standard were about $3,000, with monthly premiums that ranged below $20 depending upon income and was $0 for those below 10% of the federal poverty level (Wallace et al., 2008).

In early 2002, OHP Standard covered nearly 110,00 people, but in 2004, a budgetary shortfall halted new enrollment in the OHP Standard; and by early 2008,

attrition had reduced enrollment to about 19,000. However, in early 2008, the state of Oregon had the budget to enroll an additional 10,000 adults. Despite this newfound budget, the demand for the program among eligible individuals would far exceed the 10,000 available slots. Therefore, Oregon's Department of Human Services applied for and received permission from the Centers for Medicare and Medicaid Services to add new members through random lottery draws from a new reservation list (Finkelstein et al., 2012).

In early 2008, the state of Oregon campaigned an extensive public awareness program about the lottery opportunity focusing on the group that was not categorically eligible for the Plus program. Any qualified person could sign up from January 28 to February 29, 2008, by telephone, fax, in-person sign-up, mail, or online while providing very little demographic information. The sign up form required minimal demographics information such as sex, date of birth, address, telephone number, P.O. box, and preferred language of communication (either English or Spanish) along with the list of names, sex, and date of birth of anyone age nineteen and older in the household whom they wished to add to their sign up form (Allen et al., 2010).

No attempts were made to verify the information or to adequately screen for program eligibility at sign up for the lottery in order to keep the entry barrier low. During the window from January 28 to February 29, 2008, a total of 89,824 individuals signed up. Ineligible individuals for the OHP Standard were excluded before the lottery. The exclusion comprises individuals residing outside of Oregon, individuals born before 1944 or after 1989, individuals with the OHP standard plan as of January 2008, individuals with an institutional address, and individuals who sign up by an unrelated third party (Allen et al., 2010).

This exclusion leads to a sample that comprises 74,922 individuals (representing 66,385 households). After the sign-up phase, the state of Oregon conducted eight lottery drawings (occurred from March through September 2008) and randomly selected 29,834 individuals, and the remaining 45,088 individuals were kept as a control group.

Lottery selectees were sent a two-page application form[17]. Up to eight supplemental forms could accompany it (Allen et al., 2010). The selected individual was eligible to apply for OHP Standard for themselves and their family member (whether listed or not) and was required to submit the paperwork within 45 days. If they met the eligibility requirements, they could enroll in the Oregon Health Plan (OHP) Standard indefinitely. However, they had to verify their status every six months.

About 60% of the people who were selected by the lottery sent back the application. Half of those applications failed to meet the requirements. The primary reason was the requirement of income in the last quarter, corresponding to annual income below the poverty level. The federal poverty line in 2008 was $10,400 for a single person and $21,200 for a family of four (Allen et al., 2010). Therefore, about 30% of the total selected individuals successfully enrolled in the OHP Standard. Shortly after the random assignment of lottery and OHP Standard application form, an "initial survey" was conducted and followed by the "main survey" a year later. These surveys consist of data for 58,405 individuals comprising 29,589 individuals in treatment, and 28,816 individuals in the control group.

3.3 Approaches to Health Insurance & Health Outcomes

"How does health insurance affect health?" The answer seems obvious, but Levy and Meltzer (2008) review the literature and draw three conclusions. First, the problem of endogeneity makes causal claims tenuous. Second, the papers that establish causal evidence are focused on small subgroup populations. For example, public health insurance reduces mortality among infants and children (Currie and Gruber, 1996a,b; Hanratty, 1996), while for the elderly, public health insurance improves dif-

[17] "The main form asked for the names of all household members applying for coverage and inquired about their Oregon residence, U.S. citizenship, insurance coverage over the past six months, household income over the past two months, and assets. Documentation of identity and citizenship and proof of income had to be returned with the completed form" (Allen et al., 2010).

ferent outcomes but not mortality (Card and Maestas, 2008; Finkelstein and McKnight, 2008; McWilliams et al., 2007b,a). Third, the nature of these studies is not representative of the broader population, which prohibits generalizing for policy purposes. In this paper, I provide causal claims of the effects of Medicaid that qualify for subgroups and also allow results to generalize in out-of-samples.

Allen et al. (2010) point out three practical designs for insurance and health outcomes research: observational studies, quasi-experimental studies, and randomized experiments. Observational studies comprise the most substantial part of the literature. Most of these studies typically utilize "multivariate regression" approaches. When implemented correctly, these approaches control the observable confounding variables between health insurance and health outcomes. However, these approaches are less likely to address the issues of unobservable confounders between health insurance and health outcomes. Failure to control unobservable differences between the insured and the uninsured may drive the observed differences in health outcomes (Levy and Meltzer, 2004, 2008), which could lead to biased estimations.

The second set of studies exploit natural experiments to evaluate the effect of health insurance on health outcomes. These studies implement techniques like differences-in-differences estimations, regression discontinuity designs, and instrumental variables. These techniques exploit an exogenous event that results in variation within health insurance coverage − changes that are plausibly unrelated to health and other underlying determinants of health insurance coverage (Levy and Meltzer, 2008). Exploiting an exogenous event makes the variation of the health insurance coverage take-up as good as random. In other words, health insurance coverage varies in a way that is unrelated to the unobservable factor. Thus a comparison of various outcomes between insured and uninsured are likely to support a causal interpretation.

However, the results of natural experiments are valid for only specific population groups; thus, they cannot be generalized to the broader population. As explained earlier, several studies show that public health insurance reduces mortality among infants and children (Currie and Gruber, 1996a,b; Hanratty, 1996), while for the

elderly, public health insurance does not reduce mortality (Card and Maestas, 2008; Finkelstein and McKnight, 2008; McWilliams et al., 2007b,a). The "one size fits all" policy approaches are unlikely to be useful for the broader population. For example, the channels or mechanisms through which having insurance affects health outcomes may be different for infants and children than they are for elderly adults.

The third set of studies are social experiments, which are the "gold standard" for establishing causality. The RAND Health Insurance Experiment (RAND) and the Oregon Health Insurance Experiment (OHIE) are only two of such kind in the United States. Newhouse (1994) provides details on the RAND experiment while Finkelstein et al. (2012) offer details on the Oregon experiments. Using RAND experiment data, Newhouse (1994) and Brook et al. (1983) find no significant effect of insurance on the health status of an average adult. Levy and Meltzer (2008) point out a weakness of the RAND experiment that it did not randomize people to receive any health insurance. Instead, random individuals have treated with health insurance with varying degrees of generosity. Finkelstein et al. (2012) study the Oregon health insurance experiment data. They find statistically significant higher health care utilization, lower out-of-pocket medical expenditures and medical debt, and better self-reported physical and mental health among the treatment group.

The observational studies, quasi-experimental studies, and randomized experiments often focus on causal inference and have been dominant in empirical policy research in health economics as well as economics in general. Recently, due to the availability of big-data and computing powers, machine learning approaches are gaining momentum among researchers and policymakers. Several scholars like Varian (2014), Mullainathan and Spiess (2017), and Athey (2018) have promoted the value of the big-data and machine learning method in the field of economics. Within the domain of machine learning in economics, two strands of literature are gaining momentum: machine learning for policy prediction problems and machine learning for causal inference problems.

The machine learning algorithms behave well for out-of-sample prediction as it uti-

lizes flexible model selection, model ensembles, high dimensional data environment, and cross-validations. Therefore these algorithms are useful in many policy applications[18] where the causal inference is not central or potentially unnecessary. However, machine learning algorithms are not well suited for causal inference. Rather than just correctly predicting out-of-sample, establishing causal effect relates to understanding the counterfactual − what would happen with and without a policy (Athey, 2018). However, some slight modifications of "off-the-shelf" or readily-available machine learning algorithms can utilize the strengths and innovations of machine learning algorithms for causal inference. The predictive machine learning algorithms are readily available with the open-source routines for statistical software like Python and R.

The approaches that use machine learning methods for causal inference focus on estimating the average treatment effect, heterogeneous treatment effects, and optimal policies (Athey, 2018). In Appendix A, I provide a summary of these approaches. This paper implements a causal machine learning approach, the "generalized random forest" of Athey et al. (2019), to explore the heterogeneous treatment effects of expanding access to public health insurance on various dimensions of healthcare utilization, personal finance, health, and wellbeing. Then, I utilize efficient policy learning strategies of Athey and Wager (2019a) to explore some strategies that can help to reform access to public health insurance programs.

[18]For example, Kleinberg et al. (2015) consider a resource allocation problem in health policy in which a policymaker needs to decide which otherwise-eligible patients should not be given hip replacement surgery through Medicare. They predict the probability that a candidate for a joint replacement would die within a year from other causes. They then identify patients who are at particularly high risk and should not receive joint replacement surgery. Similarly, Henderson et al. (2012) use satellite data on lights at night to predict economic growth, and Glaeser et al. (2018) use Google Street View images to predict income in New York City. Glaeser et al. (2016) develop a system for allocating health inspectors to restaurants in Boston, and Naik et al. (2016) quantify the "urban appearance" from street-level imagery for 19 American cities and establish an empirical connection between the physical appearance of a city and the behavior and health of its inhabitants.

3.4 Empirical Strategy

3.4.1 Identification

Finkelstein et al. (2012) provides the most detailed explanations and analyses of the Oregon Health Insurance Experiment. They give the Intent-to-Treat (ITT) and the Local Average Treatment Effect (LATE) estimates for various outcome variables using the data from the "main survey" along with several other data sources. Shortly after the lottery assignment, − that allowed lucky Oregonians to apply for the OHP Standard Medicaid, and an "initial survey" was conducted to collect information from those that participate in the application. A year later, a follow-up survey or the "main survey" was performed. Therefore, the "initial survey" is pre-treatment, and the "main survey" is a post-treatment survey. These surveys consist of data of 58,405 individuals comprising 29,589 individuals in treatment, and 28,816 individuals in the control group.

Analyses in this paper consider similar outcome variables as Finkelstein et al. (2012). However, the interpretations are very distinct compared to the approach of Finkelstein et al. (2012). This paper contemplates a situation where analysts know their outcome variable (Y) at the post-treatment and have data of observables (X) at the pre-treatment period. Therefore, the sample in this study may not be independent because the covariates are all drawn from the "initial sample" and merged to the outcome variables that are from the "main survey" sample. For this reason, this paper analyzes the data as an observational, rather than a genuinely randomized study. This paper assumes unconfoundedness to identify causal effects. Unconfoundedness means that treatment assignment is as good as random conditional on observable covariates (Rosenbaum and Rubin, 1983).

Consider $i \in \{1, \ldots, N\}$ observations where the potential outcomes for each unit is either $\{Y_i(0), Y_i(1)\}$. Following Rosenbaum and Rubin (1983), the unit level causal effect is the difference in potential outcomes $\tau_i = Y_i(1) - Y_i(0)$, where, $W_i \in \{0, 1\}$ is a binary indicator for the treatment with $W_i = 0$ indicating that unit i did not

received the treatment and $W_i = 1$ indicating that unit i received the treatment. X_i is a k-component vector of features or covariates unaffected by the treatment. The data consist of triple $\left(Y_i^{obs}, W_i, X_i\right),\ \forall\ i = 1, \ldots, N$which are $i.i.d$ samples drawn from a large population. The realized outcome for unit i is the potential outcomes corresponding to the treatment i.e. Y_i^{obs} is

$$Y_i^{obs} = Y_i(W_i) = \begin{cases} Y_i(0)\ if\ W_i = 0, \\ Y_i(1)\ if\ W_i = 1. \end{cases}$$

then, unconfoundedness can be formalized as:

$$\{Y_i\left(0\right), Y_i\left(1\right)\} \perp W_i | X_i.$$

3.4.2 Mean Comparison of Demographics

In this study, the outcome variables are health care utilization, preventive care utilization, financial strain, and health after a year of the OHP Standard or Medicaid experience. The treatment variable is lottery selection, and observable covariates comprise pre-treatment demographics. This paper begins the analyses by comparing the mean of control and treatment group demographics.

$$\tilde{x}_{i,h} = \gamma_0 + \gamma_1 W_{i,h} + \eta_{ih} \tag{3.1}$$

where $\tilde{x}$ is an observable demographic variable in the pre-treatment period, γ_0 is the mean of the control group and, γ_1 is the mean difference between the control and treatment group. One should expect γ_1 to be statistically zero for comparable control and treatment groups. The selected individuals were eligible to apply for OHP Standard for themselves and their family member (whether listed or not); therefore, standard errors are household-level clustered and heteroscedasticity-consistent.

3.4.3 Intent to Treat Effect of Lottery

Secondly, this paper estimates the "intent-to-treat" (ITT) effect of winning the lottery (i.e., the difference between treatment and controls). The ITT provides a causal assessment of the net impact of expanding access to public health insurance.

This paper utilizes the double-selection post-LASSO approach introduced by (Belloni et al., 2014b). This method is based on the"LASSO."[19] Under the assumption of sparsity[20], the double-selection post-LASSO approach select the observable confounders and covariates properly. Confounders are common-cause variables that affect both outcomes and treatments. Covariates are variables that might affect results but are not associated with anything else.

The double-selection post-LASSO procedure is comprised of the following steps (Belloni et al., 2014a). First, run LASSO of dependent variables on a large inventory of potential covariates to select a set of predictors for the dependent variable. Second, run LASSO of treatment variable (lottery) on an extensive list of potential covariates to choose a set of predictors for treatment. If the treatment is genuinely exogenous, one should expect this second step should not select any variables. Third, perform OLS regression of dependent variable on treatment variable, and the union of the sets of regressors chosen in the two LASSO implementations to estimate the effect of treatment on the dependent variable then correct the inference with usual heteroscedasticity robust OLS standard error.

[19]The Least Absolute Shrinkage and Selection Operator (LASSO) is an appealing method to estimate the sparse parameter from a high-dimensional linear model is introduce by Frank and Friedman (1993) and Tibshirani (1996). LASSO simultaneously performs model selection and coefficient estimation by minimizing the sum of squared residuals plus a penalty term. The penalty term penalizes the size of the model through the sum of absolute values of coefficients. Consider a following linear model $\tilde{y}_i = \Theta_i\beta_1 + \varepsilon_i$, where Θ is high-dimensional covariates, the LASSO estimator is defined as the solution to $\min_{\beta_1 \in \mathbb{R}^p} E_n\left[\left(\tilde{y}_i - {}_i\Theta\beta_1\right)^2\right] + \frac{\lambda}{n}\|\beta_1\|_1$, the penalty level λ is a tuning parameter to regularize/controls the degree of penalization and to guard against overfitting. The cross-validation technique chooses the best λ in prediction models and $\|\beta\|_1 = \sum_{j=1}^{p}|\beta_j|$. The kinked nature of penalty function induces $\hat{\beta}$ to have many zeros; thus LASSO solution feasible for model selection.

[20]The "sparse" outcome model means a model with a few meaningful covariates affect the average outcome.

$$Y_{i,h} = \beta_0 + \beta_1 W_{i,h} + x_{ih}\beta_2 + \varepsilon_{it} \tag{3.2}$$

where, β_1 is the main coefficient of interest and gives the average difference in (adjusted) means between the treatment group (the lottery winners) and the control group (those not selected by the lottery). β_1 is the impact of being able to apply for OHP Standard through the Oregon lottery (Finkelstein et al., 2012). The x_{ih} are selected from X_{it}, implementing the double-selection post-LASSO. x_{ih} includes the set of confounding variables that correlate with treatment probability (and potentially with the outcome) along with covariates that explain treatment and outcome. Therefore controlling these covariates helps to estimate the "unbiased" relationship between winning the lottery and the outcome.

3.4.4 Local Average Treatment Effect of Lottery

The ITT estimates from equation 3.2 provide the causal effect of winning the lottery to apply for the OHP Standard. Another interesting causal parameter would be the impact of actual OHP Standard Medicaid insurance coverage rather than just the impact of winning the lottery to be eligible for the OHP Standard (ITT). In other words, policymakers are interested in the causal effect of compliance to the lottery and not just winning the lottery. The "complier" is the subset[21] of individuals who obtain insurance from winning the lottery and who would not obtain insurance through the lottery selection. One way to retrieve this parameter is to utilize lottery selection as an instrument and perform a two-stage least square (2SLS). equation 3.3 represents

[21]Imbens and Angrist (1994) point out that there exist four possible groups of individuals based upon the compliance types: complier, always-taker, never-taker, and defier. The "complier" is the subset of individuals who obtain insurance by winning the lottery and who would not obtain insurance without winning the lottery. Never takers are a subset of individuals who never get insurance even after winning the lottery. Always takers will get insurance regardless of the lottery. The defier insured themselves when they are in the control group, and don't take insurance when they are in the treatment group. So, always taker and defier have insurance though they are in the control group. The never taker and defier won't take insurance though they win the lottery.

the first stage equation and the second stage equation respectively.

$$\begin{aligned} Z_{i,h} &= \delta_0 + \delta_1 W_{i,h} + x_{ih}\delta_2 + \mu_{it} \\ Y_{i,h} &= \phi_0 + \phi_1 \widehat{Z}_{i,h} + x_{ih}\phi_2 + \nu_{it} \end{aligned} \tag{3.3}$$

where, $W_{i,h}$ is an instrumental variable of lottery assignment; $Z_{i,h}$ is an endogenous binary variable that takes a value of 1 if an individual is "ever in Medicaid" during the study period (from initial notification period until September 2009), or 0 otherwise. The first stage equation provides $\widehat{Z}_{i,h}$, which is the predicted value of "ever in Medicaid." The main coefficient of interest is ϕ_1 and is interpreted as a local average treatment effect (LATE) of Medicaid insurance (Imbens and Angrist, 1994) and identifies the causal impact of insurance among the "compliers." For just identified model, the LATE estimates, ϕ_1, is the ratio of ITT estimates from equation 3.2 and the first-stage coefficient on winning the lottery from equation 3.3 or $\phi_1 = \frac{\beta_1}{\delta_1}$ (Finkelstein et al., 2012). Relative to the study population, "compliers" are somewhat older, more likely white, in worse health, and in lower socioeconomic status (Finkelstein et al., 2012).

3.4.5 Heterogeneous Treatment Effects

Numerous studies examine the population average treatment effect of having an insurance. This effect can be formalize using a potential outcome framework as $\tau = E\left[Y_i(1) - Y_i(0)\right]$. However, this paper's main contribution is examining the heterogeneous treatment effect of Medicaid on several health and personal finance related outcomes. The treatment heterogeneity can be expressed as the conditional average treatment effect (CATE) and can be formalized as $\tau(x) \equiv E\left[Y_i(1) - Y_i(0)|X_i = x\right]$.

This paper employs the cluster-robust random forest approach of Athey and Wager (2019b) to access the treatment heterogeneity. This approach is based on the "causal tree" (Athey and Imbens, 2016), "causal forest" (Wager and Athey, 2018) and the

"generalized random forest" (Athey et al., 2019) methods. The "causal tree" approach re-engineers the Breiman et al. (1984) classification and regression tree (CART)[22], a machine learning algorithms for causal inference. The remaining methods extend the "causal tree" approach utilizing the Breiman (2001) random forest[23] machine learning algorithm for causal inference.

In essence, CART recursively filters and partitions the large data-set into binary sub-groups (nodes) such that the samples within each subset become more homogeneous in their fit of the response variable, thus resulting in a tree-like format. Figure 3.1 shows an example of features of the Titanic survivors using the CART method, as demonstrated by Varian (2014).

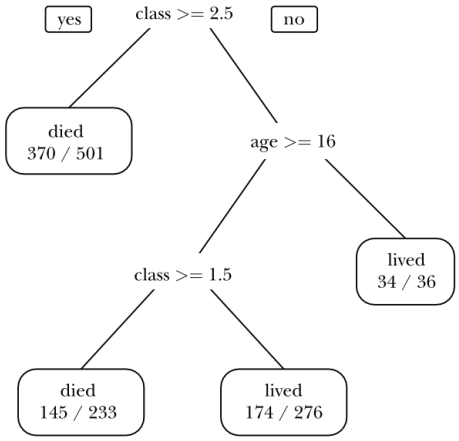
CART minimizes the mean-squared error of the prediction of outcomes to capture heterogeneity in outcomes. However, the "causal tree" minimizes the mean-squared error of treatment effects to capture treatment effect heterogeneity. The approach to estimate the "causal tree" is similar to the Imai and Ratkovic (2013) method. A sample is split into two halves. One half is used to determine the optimal partition of covariates space. The other half is used to estimate treatment effects based on the optimal partition of covariates selected from the first partition (Athey and Imbens, 2016). This sample-splitting approach is known as an "honest" estimation because model training and model estimation are independent. This approach leads to loss of precision, as only half of the data is used to estimate the effect. However, this approach generates a treatment effect and a confidence interval for each subgroup that is valid no matter how many covariates are used in estimation. This paper employs the Chernozhukov et al. (2018a) cross-fitting approach, which will be covered later in this section, to prevent the loss of precision.

One caveat of the causal tree is that it does not provide personalized estimates.

[22]In simplest, the CART algorithm chooses a variable and split that variable above or below a certain level (which forms two mutually exclusive subgroups or leaves) such that the sum of squared residuals is minimized. This splitting process is repeated for each leave until the reduction in the sum of squared residuals is below a certain level (as defined by users), thus resulting in a tree format (Athey and Imbens, 2017b).

[23]The Breiman (2001) random forest ensembles or bootstrap and aggregate many CART and report the average.

Figure 3.1: A Classification Tree for Survivors of the Titanic



Source: Varian (2014).
Interpretation: The leftmost terminal node can be interpreted as, if the class of travel is more than 2.5 (a third-class accommodation), 370 out of 501 died. The rightmost terminal node can be interpreted as, out of 36 people of the age-cohort 16 or below who were in the first and second-class accommodation, 34 survived. Those who were age-cohort more than 16, if they were in second-class accommodation, 145 died out of 233 (second from the leftmost terminal node), while 174 out of 276 died if they were in the first-class accommodation (second from the rightmost terminal node). These rules fit the data reasonably well, misclassifying about 30 percent of the observations in the testing set.

Wager and Athey (2018) utilize the "random forest" machine learning approach and propose a "causal forest" method, where many different causal trees are generated and averaged, which can provide personalized estimates. This method offers causal effects that change more smoothly with covariates and provides distinct individualized estimates and confidence intervals. Wager and Athey (2018) also provide an essential finding that the predictions from causal forests are asymptotically normal and centered on the true conditional average treatment effect for each individual. Athey et al. (2016) extend the approach to other models for causal effects, such as instrumental variables, or other models that can be estimated using the generalized method of moments (GMM). In each case, the goal is to evaluate how a causal parameter of interest varies with covariates.

3.4.6 Cluster-robust Random Forest

Random Forest

Essentially, the Breiman (2001) random forest approach makes prediction from an average of b CARTs or trees, as follow: (1) for each tree $b = 1, \ldots, B$, draw a subsample $S_b \subseteq \{1, \ldots, n\}$; (2) grow a tree via recursive partitioning on each such subsample of the data; and (3) make a prediction by averaging the prediction made by individual tree as:

$$\hat{\mu}(x) = \frac{1}{B} \sum_{b=1}^{B} \sum_{n=1}^{n} \frac{Y_i \mathbf{1}\left(\{X_i \in L_b(x), i \in S_b\}\right)}{|\{i : X_i \in L_b(x), i \in S_b\}|} \tag{3.4}$$

where, $L_b(x)$ denotes the leaf of the b^{th} tree containing the training sample x. For out-of-bag prediction, one can estimate the average as $\hat{\mu}^{(-i)}(x)$ by only considering those trees b for which $i \notin S_b$. $(-i)$ superscript denote "out-of-bag" or "out-of-fold" prediction

$R-$Learner Objective Function

Nie and Wager (2017) showed that "$R-$learner" objective function for heterogeneous treatment effect estimation as

$$\hat{\tau}(\cdot) = \arg\min_{\tau} \left\{ \sum_{i=1}^{n} \left(\left(Y_i - \hat{m}^{(-i)}(X_i)\right) - \tau(X_i)\left(W_i - \hat{e}^{(-i)}(X_i)\right)\right)^2 + \lambda_n\left(\tau(\cdot)\right) \right\} \tag{3.5}$$

where, $\lambda_n(\tau(\cdot))$ is a "regularizer" that controls the complexity of the learned conditional average treatment effect $\hat{\tau}(\cdot)$ function. $e(x) = P[W_i | X_i = x]$ is the propensity score or probability of being treated; $m(x) = E[Y_i | X_i = x]$ is expected outcomes marginalizing over treatment; $(-i)$ superscript denote "out-of-bag" or "out-of-fold" prediction.

Causal Random Forest

As explained earlier, random forest ensembles of many trees and provides prediction as an average prediction made by many individual trees. Athey et al. (2019) show that a random forest can be equivalent as an adaptive kernel method and re-express the random forest from equation 3.4 as

$$\hat{\mu}(x) = \sum_{i=1}^{n} a_i(x) Y_i; \quad a_i(x) = \frac{1}{B} \sum_{b=1}^{B} \frac{Y_i \mathbf{1}(\{X_i \in L_b(x), i \in S_b\})}{|\{i : X_i \in L_b(x), i \in S_b\}|} \tag{3.6}$$

where, $a_i(x)$ is a data-adaptive kernel or simply weights that measure how often the i^{th} training example appears in the same leaf as the test point x. Causal forests can be seen as a forest-based method motivated by "$R-$learner". Causal forest has several tuning parameters[24] and the cross-validation on the "$R-$learner" objective function helps to select these tuning parameters. The kernel-based perspective on forests suggests a natural way to use them for treatment estimation by first growing a forest to get weights $a_i(x)$, and then set

$$\hat{\tau} = \frac{\sum_{i=1}^{n} a_i(x_i) \left(Y_i - \hat{m}^{(-i)}(X_i)\right) \left(W_i - \hat{e}^{(-i)}(X_i)\right)}{\sum_{i=1}^{n} a_i(x_i) \left(W_i - \hat{e}^{(-i)}(X_i)\right)} \tag{3.7}$$

Athey et al. (2019) discuss this approach in more detail, including how to design a splitting rule of a forest that will be used to estimate prediction via equation 3.7. At the implementation level, the causal forest starts by fitting two separate regression forests to estimate $\hat{m}(\cdot)$ and $\hat{e}(\cdot)$ and making out-of-bag predictions using these two first-stage forests. Then the model uses these out-of-bag predictions as inputs to the causal forest where cross-validation on the "$R-$learner" objective function, as given in equation 3.5, chooses the tuning parameters for the causal forest.

The random forests in this paper employs the Wager and Athey (2018) "honest" estimation, as explained earlier. Furthermore, the lottery assignment was to the household rather than to an individual. Therefore, this paper grows random forests

[24]These tuning parameters include the number of variables to try for each split, number of trees grown in the forest, a target for the minimum number of observations in each tree leaf, number of minimum node size for tree.

by drawing a subsample at household level rather than individual-level. Similarly, the out-of-bag predictions are made using the household that was not in the training sample. equation 3.8 exhibits effectiveness of intervention, or Medicaid in individual, household, and global levels.

$$\hat{\tau}_h = \frac{1}{n_h} \sum_{\{i:H_i=h\}} \hat{\Gamma}_i, \quad \hat{\tau} = \frac{1}{H} \sum_{h=1}^{H} \hat{\tau}_h, \quad \hat{\sigma}^2 \frac{1}{H\left(H-1\right)} \sum_{h=1}^{H} \left(\hat{\tau}_h - \hat{\tau}\right)^2,$$

$$\widehat{\Gamma}_i = \hat{\tau}^{(-i)}\left(X_i\right) + \frac{W_i - \hat{e}^{(-i)}\left(X_i\right)}{\hat{e}^{(-i)}\left(X_i\right)\left(1 - \hat{e}^{(-i)}\left(X_i\right)\right)} \left(Y_i - \hat{m}^{(-i)}\left(X_i\right) - \left(W_i - \hat{e}^{(-i)}\left(X_i\right)\right)\hat{\tau}^{(-i)}\left(X_i\right)\right) \tag{3.8}$$

where, for the individual with household index $A_i \in \{1, \ldots, H\}$, the individual level effectiveness of lottery intervention is $\hat{\Gamma}_i$ and estimated based on the "doubly-robust" estimator with cross-fitting (Chernozhukov et al., 2018a). The household-level effectiveness of lottery intervention or the doubly-robust Average Treatment Effect (ATE) is $\hat{\tau}_h$. The global effectiveness of lottery intervention is $\hat{\tau}$ with standard error of $\hat{\sigma}^2$. The "doubly-robust" estimator is a variant of the augmented inverse-probability weighting. The name "doubly-robust" means in the sense that estimates are consistent whenever either the propensity fit, $\hat{e}\left(\cdot\right)$, or the outcome fit, $\hat{m}\left(\cdot\right)$, is consistent, and are asymptotically efficient in a semiparametric specifications. The cross-fitting, as suggested by Chernozhukov et al. (2018a), is similar to the Athey and Imbens (2016) "honest" estimation. A sample is split into two halves. The first half (main sample) is used to determine the optimal partition of covariates space. The second half (auxiliary sample) is used to estimate treatment effects within the leave based on the optimal partition of covariates selected from the first partition. Then flip the role of the main and auxiliary samples. Each of the estimates is "honest" or the two estimators will be approximately independent, so simply averaging them offers an efficient procedure (Chernozhukov et al., 2018a). In Section 3.5, column (3) of Table 3.2, 3.3, 3.4 and 3.5 exhibits the estimates of $\hat{\tau}_j$.

Assessing Treatment Heterogeneity

A heuristic approach to gain qualitative insights about the strength of heterogeneity is to see how different are the doubly-robust average treatment effects for the subgroup whose out-of-bag CATE estimates are below or above median CATE (Athey and Wager, 2019b). Davis and Heller (2017) and Athey and Wager (2019b) have used this approach to test for heterogeneity.

However, another test is based on "best linear predictor" or BLP method of Chernozhukov et al. (2018b). First test if the model is calibrated or not, and second, test for the existence of treatment heterogeneity. For this Chernozhukov et al. (2018b) suggest to create three variables: $B_i = Y_i - \hat{y_i}^{(-i)}$; $C_i = \bar{\tau}W_i - \bar{\tau}\hat{e_i}^{(-i)}$; and $D_i = (\hat{\tau}^{(-i)}(X_i) - \bar{\tau})(W_i - \hat{e_i}^{(-i)})$. $\bar{\tau}$ is out-of-bag ATE, and $\hat{e_i}^{(-i)}$ is out-of-bag propensity score.

The mean forest prediction or regressing B_i and C_i, should yield $\frac{dB_i}{dC_i} = 1$. A coefficient of one for mean forest prediction (MFP) suggests that the mean forest prediction is correct. Next, the differential forest prediction (DFP), or regressing B_i and D_i, if $\frac{dB_i}{dC_i} = 1$, it suggests that the forest has captured heterogeneity in the underlying signal. The $p-$value of the DFP coefficient also acts as an omnibus test for the presence of heterogeneity: if the coefficient is significantly greater than 0, then one can reject the null of no heterogeneity. However, asymptotic results justifying such inference are not presently available.

3.4.7 Estimation of Treatment Policies

The optimal policy estimation has received greater attention in the machine learning literature[25] (Athey, 2018). The optimal policy function maps the observable characteristics of an individual to a policy or treatment assignment. In simplest, the main goal of optimal policy estimation is to answer "who should be treated?" or the optimal treatment allocation. The understanding of optimal policy is essential

[25]See Strehl et al. (2010); Dudík et al. (2011); Li et al. (2012); Dudík et al. (2014); Swaminathan and Joachims (2015); Jiang and Li (2015); Thomas and Brunskill (2016) and Kallus (2018).

in policymaking because an ad-hoc targeting of a specific subpopulation with positive interventions can be unfair, unethical, illegal, and politically incorrect to some other subpopulations while intervening everyone in the population (a blanket policy) is welfare-maximizing but can be extremely costly.

The optimal policy estimation, or optimal treatment allocation, has been recently studied in using causal machine learning in economics, mainly by Kitagawa and Tetenov (2018) and Athey and Wager (2019a). The main idea is to select a policy function that minimizes the loss from failing to use the ideal policy, referred to as the "regret" of the policy. Note that estimating conditional average treatment effect or heterogeneous treatment effect focus on the squared-error loss while the optimal policy estimation focuses on utilitarian regret (Athey and Wager, 2019a).

Once a policymaker understands the heterogeneity effect, they would like to assign the correct treatment to each individual or subpopulation. For that, I implement the Athey and Wager (2019a) strategy to find the policy function π that can map the observable characteristic of individuals, X_i, to an available set of treatment, W_i.

$$\pi : X_i \to W_i \in \{+1, -1\}$$

Note, $W_i \in \{1, 0\}$ is reindexed as $W_i \in \{+1, -1\}$ which will help to formulate an optimal policy assignment strategy later. Then an optimal treatment assignment policy can be given as π^* that maximizes expected utility, in our case, the health outcomes.

$$\pi^* \in \arg \max_{\pi \in \prod} E\left[Y_i\left(\pi\left(X_i\right)\right)\right]$$

Alternatively, any other non-optimal policy experiences the regret of $R(\pi)$, and we would like to minimize the regret function:

$$R(\pi) = E\left[Y_i\left(\pi^*\left(X_i\right)\right)\right] - E\left[Y_i\left(\pi\left(X_i\right)\right)\right] \tag{3.9}$$

Under unconfoundedness, the overlapping assumptions and binary treatment as-

signment Athey and Wager (2019a) propose a technique to estimate the regret, regret convergence, and bound of the regret. They first determine the treatment effect, $\hat{\Gamma}_i$, for each i using the double-robust estimation technique called double machine learning of Chernozhukov et al. (2018a) and given in equation 3.8.

Equation 3.8 is a doubly-robust estimator because only one of $\hat{\mu}$ or $\hat{e}$ needs to be correctly specified, and the term double machine learning is used because $\hat{\mu}$ and $\hat{e}$ can be semi- or non-parametric estimators. If the estimate is a positive treatment effect $\hat{\Gamma}_i$, I assign individual to treatment ($\pi(X_i) = 1$) and if not then I assign individual to control ($\pi(X_i) = 0$) and penalize for mismatch and maximize the following Q function to assess the effective policy:

$$\hat{Q}(\pi) = n^{-1} \sum_i \pi(X_i) |\hat{\Gamma}_i| sign\left(\hat{\Gamma}_i\right)$$

Further, Athey and Wager (2019a) show that the regret has

$$\sqrt{n}\left(\hat{R}_{DML}(\pi) - R(\pi)\right) \xrightarrow{d} N\left(0, \sigma^2(\pi)\right)$$

convergence and is bounded with the order of $\sqrt{VC(\Pi)/n}$ where $\hat{R}_{DML}(\pi)$ is the double machine learning estimates of regret. The bound provides a robust theoretical prediction that the test-error on any out-of-sample data is upper bounded with the sum of training error and $\sqrt{VC(\Pi)/n}$.

3.5 Results

The analysis presented in this paper utilizes data from the "initial survey" and the "main survey." The "initial survey" (administered shortly after random assignment of lottery and mailing of the OHP Standard application form to the lottery selectee) and the "main survey" (conducted a year after the random assignment of the lottery) collect data from very similar questionnaire from 58,405 individual comprising 29,589 individuals in treatment and 28,816 individuals in the control group. Each of these

individuals is adults of ages 19−64 who are Oregon residents, the U.S. citizens, or legal immigrants without health insurance for at least six months, and/or are below the federal poverty level and/or have assets below $2,000.

3.5.1 Pre-treatment Comparison of Demographic Characteristics

Employing equation 3.1, Table 3.1 begins the analysis by presenting how different are treatment and control groups in their demographics in the pre-treatment period. These demographics are retrieved from the lottery list data and the initial survey data. Table 3.1 illustrates the mean of the control group and the difference of means between the treatment group and the control group. Given the random assignment of insurance, one should expect that the mean of the treatment and control group should be statistically similar. Except for a few variables, the differences in the means between treatment and control group are statistically zero. There exist some anomalies where the mean difference of few demographics are statistically nonzero, but close to zero, which could be due to the large sample size. This evidence suggests that treatment or lottery was assigned randomly.

3.5.2 ITT, LATE and Heterogeneous Treatment Effects

The treatment effect often varies with individuals' observable characteristics. For example, if the treatment is costly and less accessible, then only those who are likely to benefit most will take the treatment. In this case, the availability of the treatment may reduce the average effect among the treatment recipients. While, on the other hand, if the treatment provided to the individuals who are less likely to benefit, then the availability of the treatment may increase the average effect among the treatment recipients. Therefore understanding the heterogeneity in treatment effects has important implications for policymakers, mainly to yield valuable insights about how to distribute scarce social resources in an unequal society (Xie et al., 2012) by

Table 3.1: Pre-treatment Comparison of Demographic Characteristics

Variable	Control mean	Mean diff	Variable	Control mean	Mean diff
% Female §	0.600	-0.015***	% don't currently work	0.527	-0.007
		(0.006)			(0.008)
% English preferred §	0.921	-0.009**	% work below 20 hours/week	0.096	-0.002
		(0.004)			(0.005)
% Self signup §	0.880	-0.045***	% work 20–29 hours/week	0.111	-0.003
		(0.004)			(0.005)
% Signed up on first day §	0.102	0.004	% work 30+ hrs/week	0.266	0.012*
		(0.004)			(0.007)
% PO Box address §	0.127	0.000	% income the FPL below 50%	0.436	-0.029***
		(0.005)			(0.009)
% MSA §	0.750	-0.004	% income the FPL 50–75%	0.125	0.005
		(0.006)			(0.006)
Age (as of 2008) §	42.33	-0.108	% income the FPL 75–100%	0.154	0.000
		(0.169)			(0.006)
% Race as White	0.838	-0.009	% income the FPL 100–150%	0.171	0.012*
		(0.006)			(0.007)
% Race as Black	0.031	-0.001	% income the FPL above 150%	0.114	0.011*
		(0.003)			(0.006)
% Race as Spanish/Hispanic/Latino	0.100	0.009*	% Insurance	0.293	0.145***
		(0.005)			(0.008)
% 4-year college degree or more	0.113	0.000	% OHP	0.067	0.158***
		(0.005)			(0.006)
% High school diploma or GED	0.506	-0.007	% Private insurance	0.028	-0.002
		(0.008)			(0.003)
% Less than high school	0.168	0.002	% Other insurance	0.055	0.00
		(0.006)			(0.004)
% Vocational training or 2-year degree	0.212	0.004	Household size	2.884	0.094***
		(0.007)			(0.029)

Notes: The initial survey consists of data of 58,405 individual comprising 29,589 individuals in the treatment group and 28,816 individuals in the control group. The variables collected from the lottery list for the population that appeared in the "initial survey" are marked with §. Enclosed in the parenthesis are household-level clustered heteroscedasticity-consistent standard errors. The ***, **, and * represent 1%, 5%, and 10% level of significance, respectively. the FPL represents the FPL; in 2008, it was $10,400 for a single person and $21,200 for a family of four Allen et al. (2010). The variables presented in this table are similar to Finkelstein et al. (2012) paper. However, these estimates are different from theirs. They compare the means of treatment and control group using lottery list data (marked as §) for the observation of $n = 74922$ and the "main survey" data while this table utilizes "initial survey" data.

balancing the competing policy objectives, such as reducing cost, maximizing average outcomes, and reducing variance in outcomes within a given population (Manski, 2009).

As noted earlier, this paper contemplates a situation where analysts know their outcome variable, (Y), at post-treatment and have data of observables, (X), at the pre-treatment period. For this reason, this paper analyzes the data as an observational rather than a genuinely randomized study. Therefore, treatment heterogeneity is likely because such a situation could arise if there are unobserved household-level features that are an essential treatment effect modifiers. For example, some house-

holds may have better access to care and probably implement the intervention better than others or may have the knowledge to utilize resources to benefit from the treatment.

To generalize the results outside the sample size, one needs to robustly account for the sampling variability of potentially unexplained household-level effects. This study takes a conservative approach and assumes that the outcome variables of an individual within the same household may be arbitrarily correlated within a household (or "cluster"); therefore, it utilizes the cluster-robust analysis. Furthermore, to generalize beyond the household given in the data, each household is equally weighted such that the model allows the prediction of the effect on a new individual from a new household.

Tables 3.2, 3.3, 3.4 and 3.5 comprise various estimates for health care/preventive utilization, financial strain, self-reported health and potential mechanisms, respectively. These outcome variables are taken from the "main survey" and proxy the causal effects after one year of Medicaid experiences. Each of these tables has several estimates. The estimates in Column (1) represent "intent-to-treat" effects implementing double-selection post-LASSO method. Column (2) shows local average treatment effects, which can be interpreted as the impact of Medicaid among compliers. Column (3) presents the doubly-robust average treatment effect, which presents the average effectiveness of the lottery intervention on the outcomes.

For each Table, Columns (4), (5), and (6) explore the treatment heterogeneity. Column (4) provides "heuristic", or qualitative, insights about the strength of heterogeneity, and it groups the out-of-bag CATE estimates to above or below the median CATE estimate then estimates average treatment effects in these two subgroups separately using the doubly-robust approach to test if those average treatment effects are statistically similar or not. Columns (5) and (6) provide a test calibration for causal forest or the omnibus evaluation of the quality of the random forest-based on the "best linear predictor" method of Chernozhukov et al. (2018b). It computes the best linear fit of the target "estimand" using the forest prediction (on held-out data)

as well as the mean forest prediction as to the sole two regressors. A coefficient of one for mean forest prediction (MFP) suggests that the mean forest prediction is correct, whereas a coefficient of one for differential forest prediction (DFP) additionally suggests that the forest has captured heterogeneity in the underlying signal. The $p-$value of the DFP coefficient also acts as an omnibus test for the presence of heterogeneity: If the coefficient is significantly higher than 0, then we can reject the null hypothesis of no heterogeneity. Though the treatment heterogeneity is not detected, this does not exclusively indicate the non-existence of treatment heterogeneity. Therefore, a heatmap plot is provided for a closer look at the location of heterogeneity.

The heatmap helps to characterize which subpopulations are more or less inclined to Medicaid. However, a heatmap is a partial representation of overall treatment heterogeneity. It requires caution while interpreting because it only presents two-dimensions: age in the x-axis and household income as a percentage of the FPL. Indeed, there may exist several variables that should be taken into consideration for proper interpretation of heterogeneous treatment effects. Appendix B provides a list of relevant variables to explain each of the heatmaps in this section.

Health Care Utilization

Table 3.2 Panel A describes health care utilization on extensive and intensive margins. The health care utilization extensive margin relates to if an individual is currently taking any medication, has any outpatient visits, has any emergency visits, or has any inpatient hospital admission in the last six months. While the health care utilization intensive margins quantify how many times an individual is currently taking medication, has outpatient visits, has emergency visits, has inpatient hospital admission in the last six months.

The ITT and LATE estimate in Table 3.2 Panel A shows that on both margins of the health care utilization, there are substantial and (mostly) statistically significant increases in prescription drugs and outpatient use. However, the doubly-robust ATE estimates illustrate a significant effect for the outpatient usages only. The average

Table 3.2: Health Care Utilization

Outcome variables	ITT (1)	LATE (2)	ATE (3)	Heuristic (4)	MFP (5)	DFP (6)
Panel A: Health care utilization						
Extensive margins						
Currently taking any prescription medications	0.021**	0.067**	0.007	-0.018	0.801	-0.494
	(0.009)	(0.03)	(0.009)	(0.018)	(1.015)	(0.734)
Outpatient visits last six months	0.07***	0.224***	0.062***	0.055***	1.028***	1.316***
	(0.009)	(0.027)	(0.009)	(0.017)	(0.145)	(0.312)
ER visits last six months	0.009	0.029	0.005	-0.014	0.696	-3.331
	(0.008)	(0.024)	(0.008)	(0.015)	(1.172)	(1.816)
Inpatient hospital admissions last six months	0.002	0.005	0.001	-0.006	0.272	-0.626
	(0.004)	(0.014)	(0.005)	(0.009)	(2.322)	(1.4)
Intensive margins						
Number of prescription medications currently taking	0.104*	0.342*	0.042	-0.119	0.899	-0.383
	(0.055)	(0.177)	(0.055)	(0.109)	(1.219)	(1.005)
Number of Outpatient visits last six months	0.335***	1.087***	0.304***	0.426***	1.037***	1.502***
	(0.052)	(0.166)	(0.055)	(0.11)	(0.188)	(0.373)
Number of ER visits last six months	0.006	0.018	-0.003	-0.115***	1.97	-10.89
	(0.016)	(0.053)	(0.017)	(0.035)	(14.846)	(2.98)
Number Inpatient hospital admissions last six months	0.007	0.024	0.007	0.008	0.713	-2.071
	(0.007)	(0.021)	(0.007)	(0.014)	(0.661)	(1.974)
Panel B: Preventive care utilization						
Blood cholesterol checked (ever)	0.036***	0.116***	0.035***	0.00	1.043***	1.022*
	(0.008)	(0.026)	(0.008)	(0.016)	(0.236)	(0.73)
Blood tested for high blood sugar/diabetes (ever)	0.038***	0.121***	0.035***	0.003	0.982***	-1.588
	(0.008)	(0.025)	(0.008)	(0.017)	(0.235)	(1.618)
Mammogram within last 12 months (women 40 + age)	0.078***	0.249***	0.063***	0.048*	0.992***	2.036***
	(0.013)	(0.039)	(0.014)	(0.027)	(0.213)	(0.697)
Pap test within last 12 months (women)	0.053***	0.18***	0.047***	0.037*	1.003***	2.159***
	(0.01)	(0.034)	(0.011)	(0.022)	(0.23)	(0.671)

Notes: The ***, **, and * represent 1%, 5%, and 10% level of significance, respectively. Enclosed in the parenthesis are household-level clustered heteroscedasticity-consistent standard errors. The regressions in Columns (1) and (2) include household size dummies, survey wave dummies, and survey wave interacted with household size dummies. For the LATE estimates in Column (2), the instrumental variable is lottery assignment, and the endogenous variable is "Ever in Medicaid". The ITT and LATE estimates are base on the double-selection post-LASSO.

treatment effect of winning the lottery is associated with about a 0.30 (std. err. = 0.06) increase in the number of outpatient usages. Table 3.2 Panel B depicts the preventive care utilization. The ITT and ATE estimates are similar and statistically significant, suggesting that winning the lottery increases the likelihood for preventive

cares like a blood test for cholesterol and diabetes, mammograms (for women of age 40+), or Pap tests (for women). However, these estimates are small in size and also do not shed light on the treatment heterogeneity. There is likely no effect among a particular subgroup, while another subgroup may be uniquely affected.

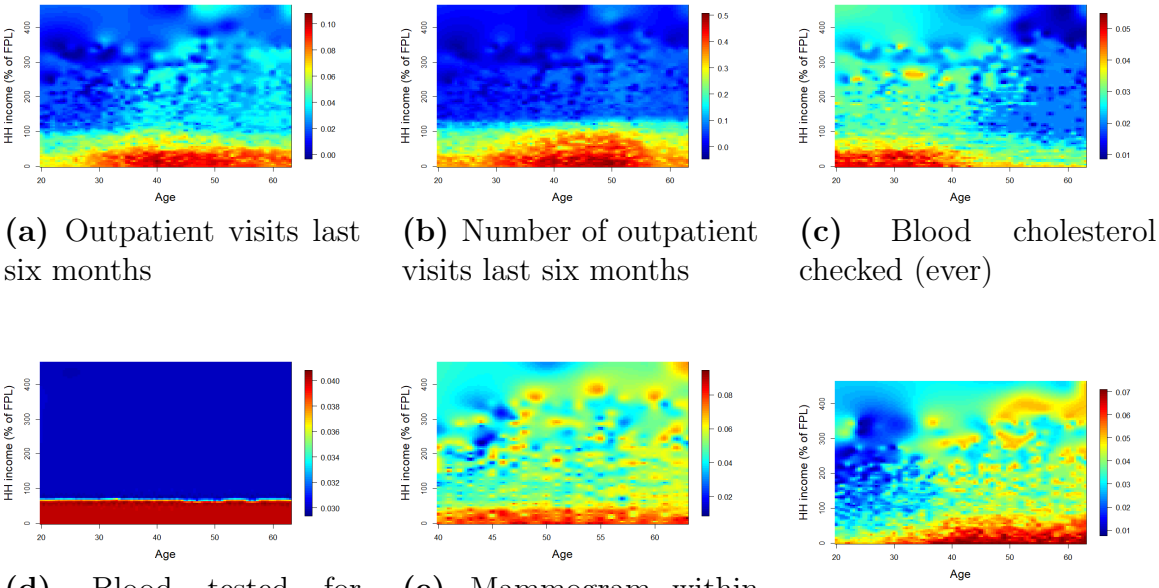
Table 3.2 Column (4) renders the heuristic approach to test the treatment heterogeneity. Evidence of treatment heterogeneity for outpatient usages and preventive care utilization is found. Table 3.2 Column (5) shows the MFP and Column (6) represents the DFP. The MFP and DFP are close to unit and statistically nonzero, suggesting treatment heterogeneity among these variables.

Note that 2000 causal trees were assembled to develop a cluster-robust random forest. Among these 2000 causal trees, the algorithm always selects the age and the household income below the federal poverty level along with the household size and other variables like education and employment. Appendix A provides the variable importance table for all of the outcomes analyzed. It lists the variables which were split (more than average) by the random forest. Only for illustration purpose of treatment heterogeneity, I develop a heatmap by grouping age and percentage of household income below the FPL and average the out-of-bag conditional average treatment. The heatmap has age on the x−axis and Household Income below the FPL (in percentage) on the y−axis.

Figure 3.2 Panel (a) to (e) renders graphical depictions that compare the treatment and control group to exhibit the treatment heterogeneity for the outpatient usages and preventive care. Figure 3.2 Panel (a) and (b) portray an insight into outpatient utilization, CATE, over Age and household income. It appears that outpatient usage CATE (in extensive margin) for lottery winners is high and positive for those who belong to a household whose income lies below 100% of the FPL, regardless of age cohorts. The findings are similar for the intensive margin of outpatient usage CATE; however, there exist some additional heterogeneity for different age-cohorts.

Figure 3.2 Panel (c) exhibits treatment heterogeneity if the blood test for cholesterol level were ever done within the study period. Mostly younger age cohorts,

Figure 3.2: Health Care and Preventive Care Utilization



(a) Outpatient visits last six months

(b) Number of outpatient visits last six months

(c) Blood cholesterol checked (ever)

(d) Blood tested for high blood sugar/diabetes (ever)

(e) Mammogram within last 12 months (women 40+)

(f) Pap test within last 12 months (women)

Notes: The heatmap helps to exhibit which subpopulations are more or less susceptible to Medicaid. For each heatmap, age is in the x-axis and household income as a percentage of the FPL is in the y-axis. For each grid of x-axis and y-axis, the color maps the intensity of individualized treatment effect. However, a heatmap is a partial representation of overall treatment heterogeneity and requires caution to interpret. Indeed there may exist several variables which should be taken into consideration for proper interpretation of heterogeneous treatment effect. Appendix B provides relevant variables list to explain each of the heatmaps in this section.

between 20 to 40, who belong to a more impoverished household, have a higher likelihood of this preventive test. Figure 3.2 Panel (e) shows the treatment subgroup who are in a household below 80% of the FPL are more likely to the blood test for diabetes. Figure 3.2 Panel (e) and (f) illuminates CATE for the Mammogram test (for women whose age is above 40) and the Pap test (for women). It appears that women aged 40 years and above who belong below 50% of the FPL households are highly likely to elect to have a Mammogram test performed. Post 50 years, women are likely to have a Mammogram test regardless of the household income is below the FPL. The heatmap of the Pap test shows, women from households close to the FPL or below 100% the FPL are likely to participate for the Pap test.

Financial Strain

Table 3.3 displays extensive margins and intensive margins of financial strains. Winning the insurance lottery is associated with lower financial strains both in extensive and intensive margins. The ITT and ATE estimates for financial strains in intensive margins quantify the results in dollar terms as the net effect of winning the lottery. The ITT and ATE ranges describe that winning the lottery relates to reductions of various types of out-of-pocket costs for the past six months. The ITT and ATE estimate ranges depicts on average $20 reductions on out-of-pocket costs for doctors visits, clinics or health centers; nearly $40 to $49 reduction on out-of-pocket costs for emergency room or overnight hospital care; about $13 to $15 reduction on out-of-pocket costs for medical care and nearly about $50 reduction on the total out-of-pocket cost for medical care. Other than these financial strains, the group that received insurance through the lottery also has nearly $450 to $500 on average reduction of their medical debts.

The "best linear prediction" (BPL) model narrates the treatment heterogeneity in the out-of-pocket expenses (last six months) only. Again, this does not necessarily mean that there is no heterogeneity because the BPL acts as an omnibus test for the presence of heterogeneity. A closer look at the heatmap in Figure 3.3 illuminates some sources of treatment heterogeneity.

The heatmap of Figure 3.3 Panel (a) shows a reduction for the extensive margin on the out-of-pocket medical expenses (last 6 months) suggesting lower financial strain for lottery winners of all age groups and all households but the effects are more pronounced for lottery winning households with income that ranges below 80% the FPL and belong to the age group of 40 years and above. Figure 3.3 Panel (b) exhibits a sharp discontinuity of owing money for medical expenses for lottery winning households with income below 100% the FPL. These differences suggest that at least within a low-income and relatively older population, individuals who select health insurance coverage are in poorer health (and therefore demand more medical care) than those who are uninsured, just as standard adverse selection theory would predict

Table 3.3: Financial Strain

Outcome variables	ITT (1)	LATE (2)	ATE (3)	Heuristic (4)	MFP (5)	DFP (6)
Extensive margins						
Any out-of-pocket medical expenses, last six months	-0.073*** (0.009)	-0.238*** (0.029)	-0.073*** (0.009)	0.028 (0.018)	1.021*** (0.125)	1.449*** (0.562)
Owe money for medical expenses currently	-0.053*** (0.009)	-0.17*** (0.027)	-0.058*** (0.009)	0.038** (0.018)	1.076*** (0.169)	0.87 (1.253)
Borrowed money or skipped other bills to pay medical bills, last six months	-0.057*** (0.009)	-0.184*** (0.028)	-0.064*** (0.009)	0.008 (0.017)	1.061*** (0.145)	0.473 (1.323)
Refused treatment because of medical debt, last six months	-0.012** (0.005)	-0.037** (0.015)	-0.013*** (0.005)	0.006 (0.009)	1.054*** (0.387)	-3.706 (2.121)
Intensive margins						
out-of-pocket costs for doctors visits, clinics or health centers, past 6 months	-19.308*** (3.46)	-61.429*** (10.919)	-20.175*** (3.594)	-8.47 (7.192)	0.999*** (0.179)	0.371 (0.664)
out-of-pocket costs for emergency room or overnight hospital care, past 6 months	-49.519** (21.611)	-157.71** (67.674)	-40.73** (18.46)	14.213 (36.89)	1.035** (0.468)	0.211 (0.689)
out-of-pocket costs for prescription medicine, past 6 months	-15.042** (6.941)	-45.756** (22.054)	-12.747** (6.012)	2.234 (12.067)	0.889** (0.403)	-1.116 (1.405)
out-of-pocket costs for other medical care, past 6 months	-3.431 (2.088)	-10.577 (6.55)	-3.052 (2.083)	-7.223* (4.188)	0.894* (0.617)	-3.693 (1.492)
Total out-of-pocket costs for medical care, last 6 months	-48.203*** (9.552)	-152.815*** (30.393)	-53.793*** (9.751)	13.3 (19.707)	1.034*** (0.188)	0.489 (0.732)
Total amount currently owed for medical expenses	-442.39*** (96.744)	-1447.906*** (318.1)	-496.084*** (105.023)	167.277 (208.674)	1.038*** (0.223)	-0.298 (1.125)

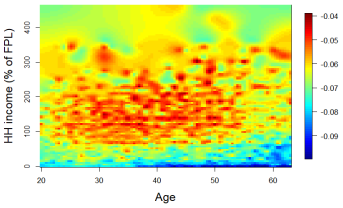
Notes: The ***, **, and * represent 1%, 5%, and 10% level of significance, respectively. Enclosed in the parenthesis are household-level clustered heteroscedasticity-consistent standard errors. The regressions in Columns (1) and (2) include household size dummies, survey wave dummies, and survey wave interacted with household size dummies. For the LATE estimates in Column (2), the instrumental variable is lottery assignment, and the endogenous variable is "Ever in Medicaid". The ITT and LATE estimates are base on the double-selection post-LASSO.

Finkelstein et al. (2012).

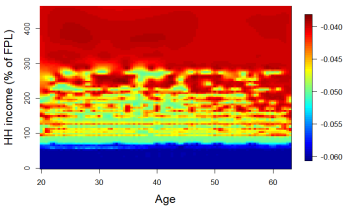
Figure 3.3 Panel (c) shows no heterogeneity of being refused for treatment because of medical debt. Privately-owned hospitals may refuse patients in a non-emergency, but public hospitals cannot turn away patients. The Emergency Medical and Treatment Labor Act (EMTLA) enacted by Congress in 1986, explicitly prohibits public hospitals from denying care to indigent or uninsured patients even if they cannot pay.

Figure 3.3 Panel (d) shows that lottery winners have an overall reduction of bor-

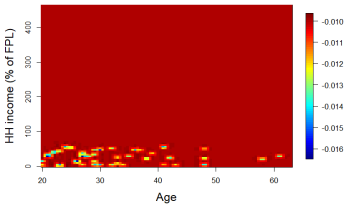
Figure 3.3: Financial Strain



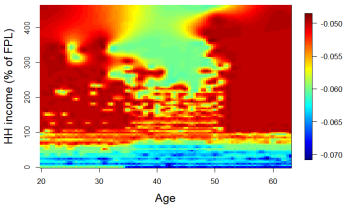
(a) Any out-of-pocket medical expenses, last 6 months



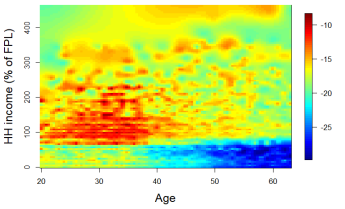
(b) Owe money for medical expenses currently



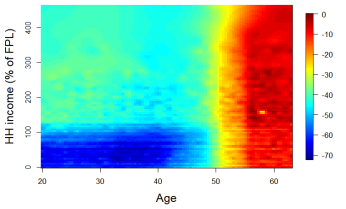
(c) Refused treatment because of medical debt, last six months



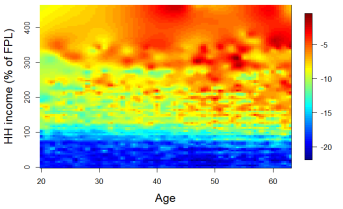
(d) Borrowed money or skipped other bills to pay medical bills, last 6 months



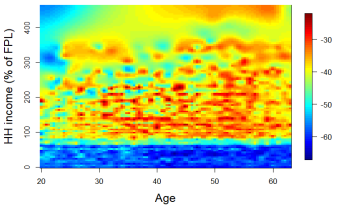
(e) out-of-pocket costs for doctors visits, clinics or health centers, past 6 months



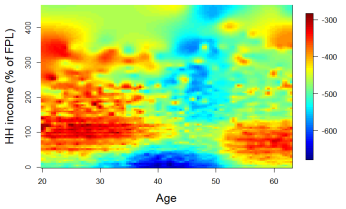
(f) out-of-pocket costs for emergency room or overnight hospital care, past 6 months



(g) out-of-pocket costs for prescription medicine, past 6 months



(h) Total out-of-pocket costs for medical care, last 6 months



(i) Total amount currently owed for medical expenses

Notes: The heatmap helps to exhibit which subpopulations are more or less susceptible to Medicaid. For each heatmap, age is on the x-axis, and household income as a percentage of the FPL is on the y-axis. For each grid of the x-axis and y-axis, the color maps the intensity of the individualized treatment effect. However, a heatmap is a partial representation of overall treatment heterogeneity and requires caution to interpret. Indeed there may exist several variables that should be taken into consideration for proper interpretation of heterogeneous treatment effect. Appendix B provides a relevant variables list to explain each of the heatmaps in this section.

rowing money or skipping other bills to pay medical costs compared to the control group. However, the effect is more pronounced for lottery winning households with

income below 100% of the FPL compared to a similar control group. These estimates are for the extensive margin only. The next figure exhibits some of the intensive margins of financial strains.

Figure 3.3 Panel (e) shows that more than $25 to $30 reductions of out-of-pocket costs (for doctors visits, clinics or health centers in past six months) for age group 50 plus who belongs to the lottery winning household with an income below 80% of the FPL compared to the control group. The below 40 age group from the lottery winning households within the range of 80% to 200% of the FPL have less than about $15 reductions of such cost compared to the similar control group. The rest of the lottery winning subgroup has roughly an average of $20 cuts of such cost, compared to the control group.

Figure 3.3 Panel (f) shows about $60 to $70 or more reduction in the out-of-pocket costs for emergency room or overnight hospital care in the past six months for the age group of 40 below of the lottery winning household with income below 100% of the FPL. The reduction of such costs is less than $20 for the 50 and above age group, regardless of their household-level income status. The remaining subgroup of these aged below 50 who belong to a household with income more than 100% of the FPL has about $30 to $50 reductions in the costs of the out-of-pocket payments for emergency room visits or overnight hospital stays.

Figure 3.3 Panel (g) exhibits that the lottery winners who belong to the household with income below 100% the FPL (regardless of their age) report more than $15 of reductions in the out-of-pocket costs for prescription medicine in past six months.

Figure 3.3 Panel (h) illuminates that the lottery winners who belong to a household with income below 100% the FPL (regardless of their age) have more than $50 of reductions in the total out-of-pocket cost for medical care in last six months.

Figure 3.3 Panel (i) exhibits the decline of the total amount currently owed for medical expenses. Compared to the control group, the treatment group with the age of 35 to 50, have medical debt reductions. Such medical debt reductions are more pronounced (more than $600) if the person belongs to a household with an income of

50% below the FPL.

As pointed out by Finkelstein et al. (2012), these results suggest that some of the financial benefits from Medicaid coverage can spillover beyond the insured. For example, the declines in out-of-pocket expenses and a reduction in the difficulty of paying non-medical bills means a reduction in the costs of unpaid care for medical providers. Furthermore, insurance can reduce extreme adverse shocks to consumption and can lead to consumption-smoothing.

Self-reported Health

Table 3.4 describes the effectiveness of the Oregon Health Insurance Experiment in the various dimensions of the perceived physical and mental health outcomes after a year. The ITT and ATE are similar and positive, suggesting lottery winners, on average, self-reported higher health in comparison with the control group. The LATE relates to the effect that is even higher for the compliance subgroup. There exists detectable treatment heterogeneity.

The survey has a self-reported health section. The responders had five options to choose (excellent, very good, good, fair, and poor) to report their health for different time frames. These are ordinal questions in nature, and there is no doubt that responders may have different perceptions of what good health represents for each individual. These options are recoded as binary for the self-reported health: good/very good/excellent to 1 and not fair or poor to 0.

Figure 3.4 Panel (a) shows, compared to the control group, the lottery winning subgroup, those aged 40 and above, from a household whose income is below 100% of the FPL are more likely to report better health. Only the small subgroup of those aged 50 reported at least not poor health, as exhibited in Figure 3.4 Panel (b).

Figure 3.4 Panel (c) depicts heterogeneity for another question regarding the responder's perceptions of better or worse health outcomes throughout the last six months. The lottery winners from households whose income is below 70% of the FPL report better health to compare to the control group. When asked to quantify the

Table 3.4: Self-reported Health

Variables	ITT (1)	LATE (2)	ATE (3)	Heuristic (4)	MFP (5)	DFP (6)
Self-reported health good/very good/excellent (not fair or poor)	0.046*** (0.009)	0.15*** (0.028)	0.046*** (0.009)	0.032* (0.017)	0.984*** (0.19)	1.485*** (0.431)
Self-reported health not poor (fair, good, very good, or excellent)	0.033*** (0.006)	0.107*** (0.019)	0.033*** (0.006)	0.044*** (0.012)	1.036*** (0.188)	1.085*** (0.316)
Health about the same or gotten better over last six months	0.035*** (0.008)	0.115*** (0.026)	0.039*** (0.008)	0.078*** (0.016)	1.086*** (0.223)	1.748*** (0.437)
Number of days physical health good, past 30 days	0.557*** (0.182)	1.796*** (0.587)	0.602*** (0.183)	0.431 (0.364)	1.037*** (0.312)	1.011*** (0.4)
Number days poor physical or mental health did not impair usual activity, past 30 days	0.432** (0.198)	1.397** (0.641)	0.454** (0.197)	1.333*** (0.392)	1.157** (0.511)	1.286*** (0.421)
Number of days mental health good, past 30 days	0.741*** (0.209)	2.479*** (0.675)	0.806*** (0.207)	0.807** (0.411)	1.041*** (0.27)	0.815*** (0.311)
Did not screen positive for depression, last two weeks	0.024*** (0.008)	0.079*** (0.027)	0.027*** (0.008)	0.023 (0.017)	1.055*** (0.338)	0.657 (0.81)

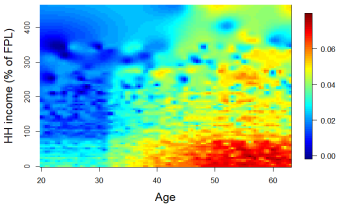
Notes: The ***, **, and * represent 1%, 5%, and 10% level of significance, respectively. Enclosed in the parenthesis are household-level clustered heteroscedasticity-consistent standard errors. The regressions in Columns (1) and (2) include household size dummies, survey wave dummies, and survey wave interacted with household size dummies. For the LATE estimates in Column (2), the instrumental variable is lottery assignment, and the endogenous variable is "Ever in Medicaid". The ITT and LATE estimates are base on the double-selection post-LASSO.

number of good physical health days in the past 30 days, lottery winning households closer to the FPL report higher numbers, as presented in Figure 3.4 Panel (d).

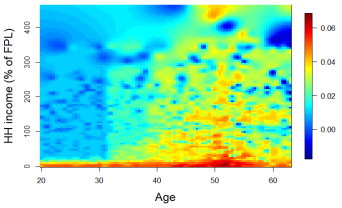
However, in Figure 3.4 Panel (e), the number of good mental health days in the past 30 days is reported to be higher for the age group above 40 from the lottery winning households closer to the FPL. The severity of mental and physical health is captured from the question to quantify the number of poor physical or mental health days did not impair the usual activity, past 30 days. Again, households closer to the FPL report higher numbers of days that were not impaired by poor physical and mental health, as plotted in Figure 3.4 Panel (f).

Figure 3.4 Panel (g) shows a group of those aged 50 and above who are from a household below 100% of the FPL are more likely to not be among those detected with depression (in the last two weeks). In all these Panels, it is repeatedly observed that lottery winning, poorer households report slightly better health outcomes compared

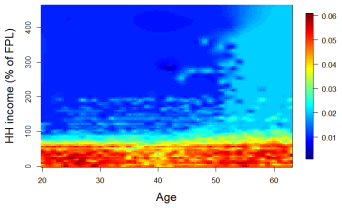
Figure 3.4: Self-reported Health



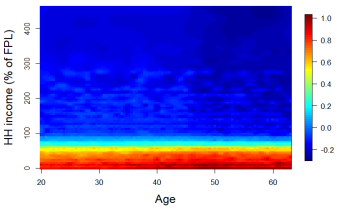
(a) Self-reported health good/very good/excellent (not fair or poor)



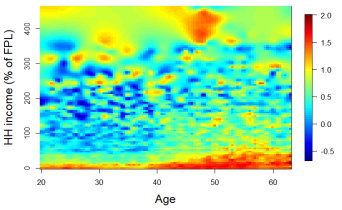
(b) Self-reported health not poor (fair, good, very good, or excellent)



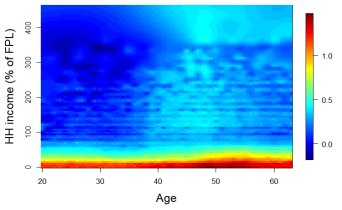
(c) Health about the same or gotten better over last six months



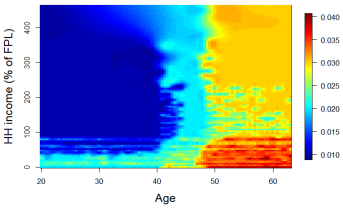
(d) Number of days physical health good, past 30 days



(e) Number of days mental health good, past 30 days



(f) Number days poor physical or mental health did not impair usual activity, past 30 days



(g) Did not screen positive for depression, last two weeks

Notes: The heatmap helps to exhibit which subpopulations are more or less susceptible to Medicaid. For each heatmap, age is on the x-axis, and household income as a percentage of the FPL is on the y-axis. For each grid of the x-axis and y-axis, the color maps the intensity of the individualized treatment effect. However, a heatmap is a partial representation of overall treatment heterogeneity and requires caution to interpret. Indeed there may exist several variables that should be taken into consideration for proper interpretation of heterogeneous treatment effect. Appendix B provides a relevant variables list to explain each of the heatmaps in this section.

to those who were not selected in the lottery. These results could arise due to adverse selection. As the theory suggests, those who are typically viewed as poorer/older

require more health services than their counterparts. When they can receive that care, they will report better health outcomes compared to the groups that are unable to acquire that care.

Potential Mechanism for Improved Health

Table 3.5 depicts some potential mechanisms by which health insurance could have improved objective physical health along the heterogeneities in these mechanisms. Table 3.5 Columns (1), (2), and (3) present statistically significant increases of self-reported access to care (Panel A), quality of care (Panel B), and happiness (Panel C). Overall, the evidence suggests that people feel better off due to insurance, but Finkelstein et al. (2012) point-out that with the current data, it is difficult to determine the fundamental drivers of this improvement. One way to look at the drivers of this improvement is to capture the treatment heterogeneities. Except for the use of ER for a non-emergence (last six months), there are treatment heterogeneities in the access to care, quality of care, and happiness detailed in Table 3.5 Columns (4), (5), and (6).

Figure 3.4 illustrates the heatmap with age in the x−axis and percentage of household income below the FPL in the y−axis. The treatment effects are plotted for every possible grid of age and percentage of household income below the FPL. Figure 3.4 Panel (a) exhibits a particular threshold that households with income below the FPL 90% are more likely to have the usual place of clinic-based care than the control subgroup of similar attributes.

Figure 3.4 Panel (b) depicts households with income above 100% of the FPL with those aged 40 years and above are less likely to have a personal doctor compared to the households with income below 100% of the FPL with those aged under 40. Most of the poorer households are likely to get all their needed medical care (Figure 3.4 Panel (c)) and medications (Figure 3.4 Panel (d)) while households with income below 50% of the FPL and aged 40 age and above are less likely to utilize the ER in instance of non-emergencies (Figure 3.4 Panel (e)). Perceived quality of care is very

Table 3.5: Potential Mechanism for Improved Health

Variables	ITT (1)	LATE (2)	ATE (3)	Heuristic (4)	MFP (5)	DFP (6)
Panel A: Access to care						
Have usual place	0.087***	0.274***	0.086***	0.041**	1.012***	2.185***
of clinic-based care	(0.009)	(0.029)	(0.009)	(0.018)	(0.109)	(0.736)
Have personal doctor	0.073***	0.235***	0.072***	0.101***	1.031***	1.329***
	(0.009)	(0.029)	(0.009)	(0.018)	(0.127)	(0.202)
Got all needed medical	0.085***	0.274***	0.085***	0.095***	1.019***	1.985***
	(0.009)	(0.028)	(0.009)	(0.017)	(0.106)	(0.332)
Got all needed drugs,	0.07***	0.227***	0.073***	0.058***	1.016***	1.733***
last six months	(0.008)	(0.026)	(0.008)	(0.016)	(0.112)	(0.416)
Didn't use ER for	0.00	0.00	0.003	-0.04***	1.163	-4.168
non emergency, last six months	(0.005)	(0.015)	(0.005)	(0.01)	(1.469)	(2.29)
Panel B: Quality of care						
Quality of care received last six months	0.049***	0.15***	0.053***	-0.312***	1.028***	-402.796
good/very good/excellent (conditional on any)	(0.01)	(0.03)	(0.01)	(0.019)	(0.179)	(19.252)
Panel C: Happiness						
Happiness, very happy or pretty	0.062***	0.202***	0.069***	0.057***	1.049***	1.551***
happy (vs. not too happy)	(0.009)	(0.029)	(0.009)	(0.017)	(0.134)	(0.379)

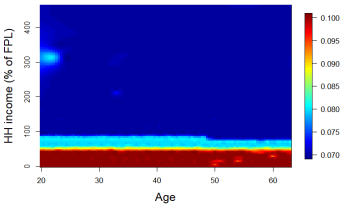
Notes: The ***, **, and * represent 1%, 5%, and 10% level of significance, respectively. Enclosed in the parenthesis are household-level clustered heteroscedasticity-consistent standard errors. The regressions in Columns (1) and (2) include household size dummies, survey wave dummies, and survey wave interacted with household size dummies. For the LATE estimates in Column (2), the instrumental variable is lottery assignment, and the endogenous variable is "Ever in Medicaid". The ITT and LATE estimates are base on the double-selection post-LASSO.

uniformly distributed among the households and all age groups (Figure 3.4 Panel (f)). However, aged 40 and above in households with income below 180% of the FPL are more likely to have perceived happiness (Figure 3.4 Panel (d)).

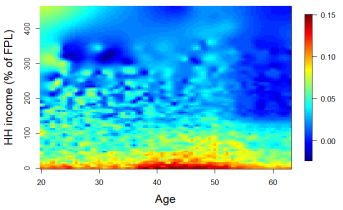
3.5.3 Efficient Policies

The previous section describes the ITT, LATE, and ATE along with the test of treatment heterogeneity. A more interesting question is whether we can find ways to prioritize treatment to some subgroups of Medicaid eligible registrants who are more likely to benefit from it. Following the out-of-bag prediction using generalized random forests of the Athey and Wager (2019b), I compute doubly-robust scores for the treatment effect as in equation 3.8, and learning policies empirical maximization

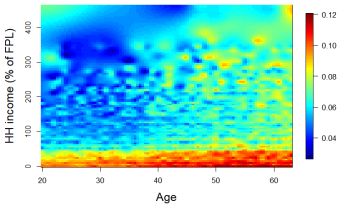
Figure 3.5: Potential Mechanism for Improved Health



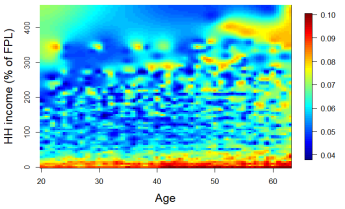
(a) Have usual place of clinic-based care



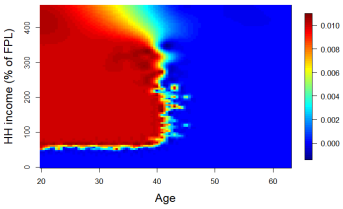
(b) Have personal doctor



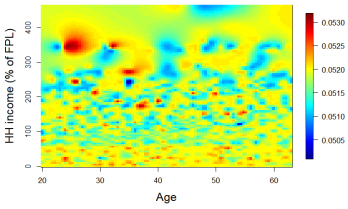
(c) Got all needed medical care, last six months



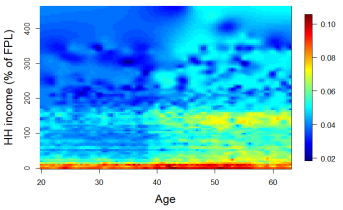
(d) Got all needed drugs, last six months



(e) Didn't use ER for non-emergency, last six months



(f) Quality of care received last six months good/very good/excellent (conditional on any)



(g) Happiness, very happy or pretty happy (vs. not too happy)

Notes: The heatmap helps to exhibit which subpopulations are more or less susceptible to Medicaid. For each heatmap, age is on the x-axis, and household income as a percentage of the FPL is on the y-axis. For each grid of the x-axis and y-axis, the color maps the intensity of the individualized treatment effect. However, a heatmap is a partial representation of overall treatment heterogeneity and requires caution to interpret. Indeed there may exist several variables that should be taken into consideration for proper interpretation of heterogeneous treatment effect. Appendix B provides a relevant variables list to explain each of the heatmaps in this section.

as in equation 3.9.

Table 3.6 Column (1) details the average outcome for each policy variable of interest under the random assignment of treatment. Table 3.6 Columns (2) to (5) present

Table 3.6: Estimate of the Utility Improvement of Various Policies Over Baseline.

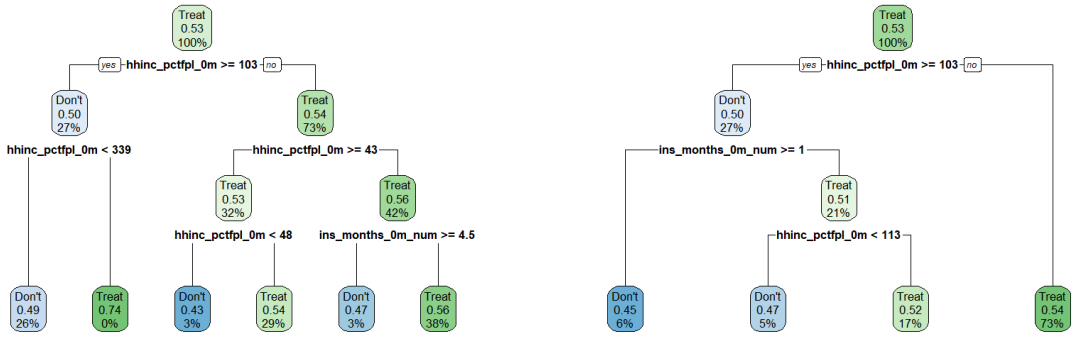
Variable	Baseline (1)	Probability rule (2)	CATE rule (3)	Shallow tree (4)	Deeper tree (5)
Panel A: Health care utilization					
Outpatient visits	0.604***	4.74***	5.119***	4.228***	2.898***
last six months	(0.002)	(0.182)	(0.17)	(0.197)	(0.177)
Panel B: Preventive care utilization					
Blood cholesterol checked (ever)	0.659***	0.575***	3.023***	1.934***	1.59***
	(0.005)	(0.176)	(0.146)	(0.166)	(0.154)
Blood tested for high blood	0.625***	1.066***	3.059***	2.665***	2.068***
sugar/diabetes (ever)	(0.003)	(0.157)	(0.124)	(0.137)	(0.178)
Mammogram within last	0.331***	7.008***	10.228***	9.26***	5.75***
12 months (women + 40)	(0.002)	(0.482)	(0.398)	(0.552)	(0.42)
Pap test within last	0.411***	3.489***	5.682***	4.955***	4.058***
12 months (women)	(0.003)	(0.286)	(0.24)	(0.315)	(0.316)
Panel C: Self-reported health					
Self-reported health good/very	0.579***	1.952***	4.186***	4.225***	2.588***
good/excellent (not fair or poor)	(0.003)	(0.174)	(0.145)	(0.201)	(0.195)
Panel D: Potential mechanism					
Have usual place	0.558***	5.462***	7.44***	7.305***	4.718***
of clinic-based care	(0.002)	(0.227)	(0.203)	(0.237)	(0.202)
Have personal doctor	0.544***	6.114***	6.432***	6.144***	4.576***
	(0.003)	(0.192)	(0.207)	(0.244)	(0.181)
Happiness, very happy or pretty	0.629***	2.137***	4.883***	5.042***	3.306***
happy (vs. not too happy)	(0.002)	(0.196)	(0.174)	(0.218)	(0.166)

Notes: The ***, **, and * represent 1%, 5%, and 10% level of significance, respectively. Enclosed in the parenthesis are standard errors. The estimates in Columns (1) represents the averages of each variable based on the random assignment baseline and considered as a parameter measuring the cost of treatment. The estimates in Column (2) to (5) presents the estimates of the average outcome improvement (in percentage) of various policies over a random assignment baseline for selected variable of interest. Policies learned on different subsets of the data will in general be different from the policies learned on the full data. Therefore, to examine the stability of the learned rule, 100 different policy are learned from randomly sample subdata and estimates are based on the out-of-bag sample.

the estimates of the average outcome improvement (in percentage) of various policies over a random assignment baseline for the selected variable of interest. Efficient policy for each of the variables of interest uses a particular set of covariates, as given in appendix B. However, I did not use covariates like gender and race for the ethical and political rationale because these covariates cannot legally be used for treatment allocation.

In Table 3.6 Column (2) the assignment policy is based on a probability rule. The

Figure 3.6: Efficient Policy to Improve Outpatient Visits



(a) Shallow tree **(b)** Deep tree

Notes: The hhinc_pctthe FPL_0m shows household income as percentage of the federal poverty line in the baseline and the ins_months_0m_num shows numbers of months that a responder has insurance in last six months. Policies learned on different subsets of the data will in general be different from the policies learned on the full data, and it can be interesting to examine them to gain intuition for the stability of the learned rule. Table 3.6 exhibits the stability of learned rule, however, Figure 3.6 is a graphical depiction of a learned policy and can be different to different subsets of the data.

probability rule allocates Medicaid for those whose probability is less than the average probability of each outcome of interest. The generalized random forest provides the probability for each outcome of interest. In Table 3.6 Column (3), the assignment policy is the CATE rule, i.e., assign Medicaid if CATE is positive. In Table 3.6 Columns (4) and (5), the shallow and deeper causal tree provides the Medicaid assignment policies. The shallow causal tree allows a max-depth of 3 policy trees while the deeper causal tree allows the max-depth of policy tree to be obtained by optimal pruning of the causal tree using cross-validation. Caution is warranted as asymptotic results hold only for trees with little complexity.

Table 3.6 Panel A, Column (1) describes the percentage of the households with outpatient visits over the last six months using the full sample data. About 60% of the whole sample has an outpatient visit in the previous six months. Note, this estimate is based on the lottery assignment of the OHP Standard or Medicaid. Panel A, Column (2) presents that if the Medicaid or OHP Standard is assigned among the eligible registrants using the probability rule, then it would improve outpatient visits by an

additional 4.74%. Panel A, Column (3), exhibits, if the Medicaid assignment is based on the CATE rule, then it would improve outpatient visits by 5.12%. The optimal depth-3 policy tree or shallow tree would improve outpatient visits by an additional 4.23%. The optimal depth for policy trees based on the cross-validation for pruning would improve outpatient visits by an extra 2.9%. All of these improvements are statistically significant.

Figure 3.6 is a graphical depiction of the proposed efficient policy with the shallow tree in Panel (a) and deep tree in Panel (b). Note that the policies learned on different subsets of the data will, in general, be different from the policies acquired on the full data. It can be interesting to examine them to gain an intuition for the stability of the learned rule. Table 3.6 exhibits the stability of learned rule. However, Figure 3.6 is a graphical depiction of a learned policy and can vary for different subsets of the data. To save space, learned efficient policies for the rest of the variables that are presented in Table 3.6 are compiled in Appendix C.

3.6 Discussion and Conclusion

In 2008, 10,000 low-income Oregonian adults (19 to 64 years of age) were randomly chosen to qualify for Medicaid, which provides a unique opportunity to study the causal effect of Medicaid coverage. Finkelstein et al. (2012) found in the year following the random assignment of Medicaid, the treatment group had higher health care use, lower out-of-pocket medical expenditures and medical debt, and better self-reported physical and mental health than the control group, but it did not have detectable improvements in physical health conditions like high blood pressure. However, these mixed-bag effects of Medicaid puzzle researchers to determine what drives the relationship between Medicaid and other outcomes of interest. My paper puts forward an argument of heterogeneous treatment effect where Medicaid distinctly affects different individuals and subpopulations differently. Furthermore, I use these heterogeneous treatment effects to reveal policy reforms. These reforms prioritize Medicaid

allotments to the subgroups that are likely to benefit the most. I also quantify how much these reforms improve from the baseline Medicaid impacts on health care use, personal finance, health, and well-being.

In this section, I present discussions on some of the obvious questions that the reader may have. This paper contemplates a situation where analysts know their outcome variable, (Y), at the post-treatment and have data of observables, (X), at the pre-treatment period. This situation may be a standard for many researchers. For this reason, this paper analyzes the data as an observational rather than a genuinely randomized study. Therefore, the unconfoundedness assumption to identify causal effects is crucial for this paper.

This paper focuses on "intent-to-treat" rather than "local average treatment effects." A local average treatment effect can be interpreted as the impact of Medicaid among compliers while an intent-to-treat estimates the net impact of expanding access to Medicaid. The results present both facts, but I mainly focus on the intent-to-treat because the problem policymakers face only a choice of the eligibility criteria and not the take-up. There can be many reasons for eligible people (lottery winner) not to accept Medicaid and people who do not win the lottery to get other insurance from other sources. This is the consumer's sovereignty, and policymakers cannot micromanage.

The heterogeneous effects of Medicaid are pronounced among households below 100% of the federal poverty line. A possible answer would be that more impoverished families may need more medical care. Medicaid provides an opportunity for these households to gain access to health care, and they, therefore, may utilize health care more than those who are uninsured that can be an exemplification of a standard adverse selection theory prediction. Also, I did not use the covariates like gender and race for the ethical and political rationale because these covariates cannot legally be used for treatment allocation. However, these are essential covariates, and not including these covariates can lead to higher standard errors in the estimates.

The proposed policy can be thought of as small reforms in Medicaid. Rather than

a blanket policy that can be welfare-maximizing yet highly costly, these reforms target the subpopulation who are more likely to derive benefit and because these reforms are aimed, therefore, can be less expensive. For example, the federal government started to defund Oregon's Medicaid Expansion from 2016 which has led to a budget deficit and Oregon Measure101 a two-year budget fix to close the state budget deficit by taxing hospital and insurance agencies, is nearing to end in 2020, these proposed reforms can help Oregon to reduce the state budget deficit.

To generalize the results outside the sample size, one needs to robustly account for the sampling variability of potentially unexplained household-level effects. This study takes a conservative approach and assumes that the outcome variables of an individual within the same household may be arbitrarily correlated within a household (or "cluster"), and therefore, utilizes the cluster-robust analysis. Each household is equally weighted, such that the model allows the prediction of the effect on a new individual from a new household, to generalize beyond the households given in the data. However, caution must be taken. First, these estimates are the one-year impact of expanding Medicaid access, and effects can change over longer time horizons than we can analyze. Second, these findings are the partial equilibrium effects of covering a small number of people, holding constant the rest of the health care system; the results of much more extensive health insurance expansions might differ because of supply-side responses by the health care sector. Third, the population is not representative of the low-income uninsured adults in the rest of the United States on several observable (and presumably unobservable) dimensions.

To conclude, I provide some evidence of heterogeneous treatment effects of Medicaid that can reconcile the mixed-bag results of Medicaid, as reported by previous literature. I also proposed some reforms that can improve program effectiveness. The Medicaid expansion, through the Affordable Care Act (ACA) and the contemporary fiscal pressure, has triggered a national debate amongst diverse stakeholders regarding the impacts of Medicaid coverage on various dimensions of public health, costs, and benefits. Some have argued that Medicaid decreases total health care spending by

improving health and reducing inefficient hospital and emergency room utilization. Others have disputed that Medicaid reneges the promised benefits because Medicaid reimburses providers insufficiently, and therefore, recipients struggle to obtain access to care, and the low income uninsured already have reasonable access to care through clinics, uncompensated care, emergency departments, and out-of-pocket spending. Both of these arguments eventually motivate a need for substantial discussion and rigorous empirical assessment of what effects, if any, Medicaid coverage has on health care, health, and well-being and how to strike a balance between cost and benefits.

Appendix A

Causal Machine Learning Approaches

Average Treatment Effect In this paragraph, I show a few examples of a causal machine learning approach to estimate the average treatment effect. For example, Belloni et al. (2014b) and Belloni et al. (2014a) utilize "off-the-shelf" or readily available predictive machine learning algorithm called the "LASSO"[1] method and purpose a correction[2] called the "double-selection post-LASSO"[3] method. This method is use-

[1]The Least Absolute Shrinkage and Selection Operator (LASSO) is an appealing method to estimate the sparse parameter from a high-dimensional linear model is introduce by Frank and Friedman (1993) and Tibshirani (1996). The LASSO simultaneously performs model selection and coefficient estimation by minimizing the sum of squared residuals plus a penalty term. The penalty term penalizes the size of the model through the sum of absolute values of coefficients. Consider a following linear model $\tilde{y}_i = \Theta_i\beta_1 + \varepsilon_i$, where Θ is high-dimensional covariates, the LASSO estimator is defined as the solution to $\min_{\beta_1 \in \mathbb{R}^p} E_n\left[\left(\tilde{y}_i - {}_i\Theta\beta_1\right)^2\right] + \frac{\lambda}{n}\|\beta_1\|_1$, the penalty level λ is a tuning parameter to regularize/controls the degree of penalization and to guard against over-fitting. The cross-validation technique chooses the best λ in prediction models and $\|\beta\|_1 = \sum_{j=1}^{p} |\beta_j|$. The kinked nature of penalty function induces $\hat{\beta}$ to have many zeros; thus LASSO solution feasible for model selection.

[2]When LASSO of outcome variable is implemented to select the covariates while always restricting the treatment indicator, the estimated treatment effect is biased because LASSO's sole objective is to select variables that predict outcome thus LASSO fails to select confounders that are also strong predictor of treatment assignment.

[3]Belloni et al. (2014a) simplify the double-selection post-LASSO procedure as following. First, run LASSO of outcome variables on a large list of potential covariates to select a set of predictors for the outcome variable. Second, run LASSO of treatment variable on a large list of potential covariates to select a set of predictors for treatment. If the treatment is truly exogenous, we should expect this second step should not select any variables. Third, run OLS regression of outcome variable on treatment variable, and the union of the sets of regressors selected in the two LASSO runs to estimate the effect of treatment on the outcome variable then correct the inference with

ful for estimating the average treatment effect when the analyst is required to select a "sparse" outcome model[4] from high-dimensional observables when some covariates correlate with treatment and outcome, and the analyst does not know which ones are important. Similarly, Athey et al. (2018) utilize "doubly-robust"[5] method and LASSO method and purpose "residual balancing"[6] approach for estimating average treatment effect under the assumption of unconfoundedness[7] and the assumption of the outcome model is linear and sparse. Similarly, Chernozhukov et al. (2018a) purpose "double machine learning" for estimating the average treatment effect under unconfoundedness. The idea is to first run any feasible machine learning methods of outcomes on covariates, and then second run another feasible machine learning methods of the treatment indicator on covariates; then, the residuals from the first machine learning are regressed on the residuals from the second machine learning to estimate the average treatment effect. This idea is similar to Frish-Waugh-Lovell theorem[8] and close to the concept of Robinson (1988) residual-on-residual regression approaches where the estimator was a kernel regression. Heterogeneous Treatment Effects Along with the average treatment effect, heterogeneous treatment effects es-

usual heteroscedasticity robust OLS standard error.

[4]The "sparse" outcome model means a model with a few meaningful covariates that affect the average outcome. These few meaningful covariates are selected from a given list of many observables covariates, and potentially a situate when numbers of observables k are greater than numbers of observations n, i.e., $k > n$. When $k > n$, an estimation based on the least-squares estimation is infeasible. However, traditionally, the principal component analysis (PCA) is commonly used to reduce dimension when the likelihood function is normal. The PCA creates principal components using linear combinations of a much larger set of variables from a multivariate data-set. Interpreting the coefficients on the principal components requires the researcher first to interpret the principal components, which can prove a challenge as all variables have non-zero loadings.

[5]The "doubly-robust" estimator proceeds by taking the average of the efficient score, which involves the estimation of conditional mean of outcomes given covariates as well as the inverse propensity score Athey (2018).

[6]The "residual balancing" replaces inverse propensity score weights with weights obtained using quadratic programming, where the weights are designed to achieve balance between the treatment and control group. The conditional mean of outcomes is estimated using LASSO Athey (2018).

[7]The unconfoundedness assumption implies treatment is randomly assigned and knowing observable characteristics of an individual, and their treatment status gives no additional information on the potential outcomes. This means the treatment assignment is independent of the outcome variable.

[8]The Frisch-Waugh-Lovell theorem is that estimating a parameter in a multiple regression is equivalent to estimating the same parameter in a simple regression of the residual of the regress and regressed on all other predictors on the residual of the regressor regressed on all other predictors.

timation interests policymakers because it helps to quantify the sizes of effects on different subpopulations, which is valuable to improve program targeting and to understand the underlying mechanisms driving the results. Usually, data are stratified in mutually exclusive groups or include interactions in a regression to explore heterogeneous treatment effects. However, ad-hoc searches for the responsive subgroups may lead to false discoveries or may mistake noise for a true treatment effect (Davis and Heller, 2017). Knaus et al. (2017) points out that for large-scale investigations of effect heterogeneity, standard $p-$values of standard (single) hypothesis tests are no longer valid because of the multiple hypothesis testing problems (Lan et al., 2016; List et al., 2019) and leads to so-called "ex-post selection" problem which is widely recognized in the program evaluation literature. For example, for fifty single hypotheses tests, the probability that at least one test falsely rejects the null hypotheses at the 5% significance level (assuming independent test statistics as an extreme case) is $1 - 0.95^{50} = 0.92$ or 92%.

The new avenue of causal machine learning provides a better systematic approach to search the groups with heterogeneous treatment effects. One intuitive approach proposed by Imai and Ratkovic (2013) is to sample-split and use the first sample to run the LASSO regression model with the treatment indicator interacted with covariates and perform variables selections then use the selected model with the second sample to perform an ordinary least squares regression to guard against over-fitting. While Athey and Imbens (2016) utilizes the Breiman et al. (1984) classification and regression tree (CART)[9] machine learning algorithms and purpose "causal tree" method. The CART recursively filters and partitions the large data-set into binary sub-groups (nodes) such that the samples within each subset become more homogeneous that fit the response variable. Unlike the CART that minimizes the mean-squared error of the prediction of outcomes to capture heterogeneity in outcomes, the "causal" tree

[9]In simplest, the CART algorithm chooses a variable and split that variable above or below a certain level (which forms two mutually exclusive subgroups or leaves) such that the sum of squared residuals is minimized. This splitting process is repeated for each leave until the reduction in the sum of squared residuals is below a certain level as defined by users, thus resulting in a tree format (Athey and Imbens, 2017b).

minimizes the mean-squared error of treatment effects to capture treatment effect heterogeneity. The approach to estimate the "causal" tree is similar to Imai and Ratkovic (2013) approach, in which half of the sample is used to determine the optimal partition of covariates space, while the other half is used to estimate treatment effects within the leave based on the optimal partition of covariates selected from the first partition (Athey and Imbens, 2016). The sample-splitting approach also known as "honest" estimation lead to loss of precision as only half of the data is used to estimate the effect, but generates a treatment effect and a confidence interval for each subgroup that is valid no matter how many covariates are used in estimation. Athey and Imbens (2017b) points out that the researcher is free to estimate a more complex model in the second part of the data, for example, if the researcher wishes to include fixed effects in the model, or model different types of correlation in the error structure.

The causal tree doesn't provide personalized estimates, Wager and Athey (2018) utilize the "random forest" machine learning approach and propose a "causal forest" method, where many different causal trees are generated and averaged. This method provides causal effects that change more smoothly with covariates and provides distinct individualized estimates and confidence intervals. Wager and Athey (2018) also shows that the predictions from causal forests are asymptotically normal and centered on the true conditional average treatment effect for each individual. Athey et al. (2016) extend the approach to other models for causal effects, such as instrumental variables, or other models that can be estimated using the generalized method of moments (GMM). In each case, the goal is to evaluate how a causal parameter of interest varies with covariates. Efficient Policy The optimal policy estimation have received greater attention in the machine learning literature[10] (Athey, 2018). The optimal policy function map the observable characteristics of an individual to a policy or treatment assignment. In simplest, the main goal of optimal policy estima-

[10]See Strehl et al. (2010); Dudík et al. (2011); Li et al. (2012); Dudík et al. (2014); Swaminathan and Joachims (2015); Jiang and Li (2015); Thomas and Brunskill (2016) and Kallus (2018).

tion is to answer—- "who should be treated?" or optimal treatment allocation. The understanding of optimal policy is essential in policymaking because an ad-hoc targeting a specific subpopulation with positive interventions can be unfair, unethical, illegal, and unpolitical to some other subpopulations while intervening everyone in the population (a blanket policy) is welfare-maximizing but can be extremely costly.

The optimal policy estimation or optimal treatment allocation has been recently studied in using causal machine learning in economics, mainly by Kitagawa and Tetenov (2018) and Athey and Wager (2019a). The main idea is to select a policy function that minimizes the loss from failing to use the ideal policy, referred to as the "regret" of the policy. Note that estimating conditional average treatment effect or heterogeneous treatment effect focus on the squared-error loss while the optimal policy estimation focuses on utilitarian regret Athey and Wager (2019a).

Appendix B

Variable Importance

Table B.1: Variable Importance

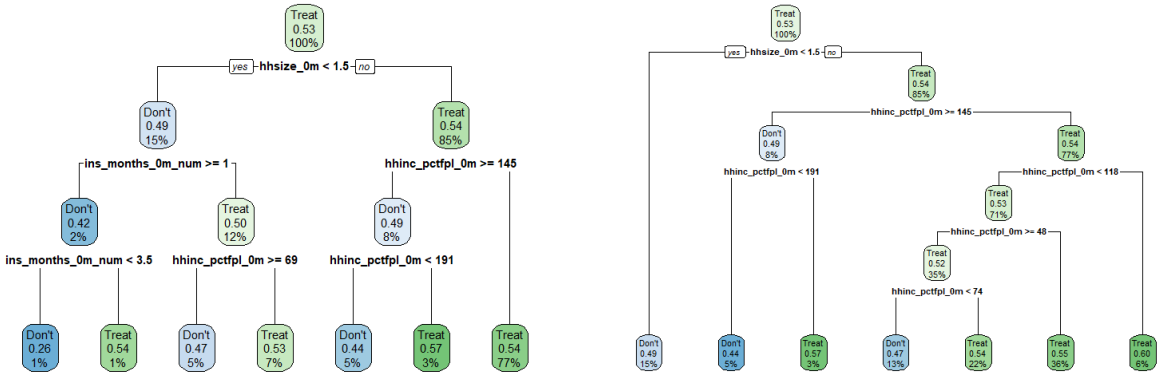
Variables	FPL	Age	HHS	INS	Other variables
Currently taking any prescription medications	✓	✓	✓		% MSA
Outpatient visits last six months	✓	✓		✓	
ER visits last six months	✓	✓	✓		
Inpatient hospital admissions last six months	✓	✓	✓	✓	
Number of prescription medications currently taking	✓	✓	✓	✓	
Number of Outpatient visits last six months	✓	✓		✓	
Number of ER visits last six months	✓	✓	✓	✓	% High school disploma or GED
Number Inpatient hospital admissions last six months	✓	✓	✓		% Self signup
Any out of pocket medical expenses, last six months	✓	✓	✓	✓	% MSA
Owe money for medical expenses currently	✓	✓	✓		
Borrowed money or skipped other bills to pay medical bills, last six months	✓	✓	✓		
Refused treatment because of medical debt, last six months	✓	✓	✓		
Out of pocket costs for doctors visits, clinics or health centers, past 6 months	✓	✓			% work 30+ hrs/week
Out of pocket costs for emergency room or overnight hospital care, past 6 months	✓	✓	✓		
Out of pocket costs for prescription medicine, past 6 months	✓	✓	✓	✓	
Out of pocket costs for other medical care, past 6 months	✓	✓		✓	
Total out of pocket costs for medical care, last 6 months	✓	✓	✓	✓	% work 30+ hrs/week
Total amount currently owed for medical expenses	✓	✓	✓	✓	
Have usual place of clinic-based care	✓	✓			
Have personal doctor	✓	✓		✓	% work 30+ hrs/week
Got all needed medical care, last six months	✓	✓	✓	✓	% work 30+ hrs/week
Got all needed drugs, last six months	✓	✓	✓		% dont currently work
Didn't use ER for non emergency, last six months	✓	✓	✓		% work 30+ hrs/week
Quality of care received last six months good/very good/excellent (conditional on any)	✓	✓	✓		% MSA
Happiness, very happy or pretty happy (vs. not too happy)	✓	✓	✓	✓	
Blood cholesterol checked (ever)	✓	✓	✓	✓	
Blood tested for high blood sugar/diabetes (ever)	✓	✓	✓		
Mammogram within last 12 months (women 40)	✓	✓	✓	✓	% work 30+ hrs/week
Pap test within last 12 months (women)	✓	✓	✓		% work 30+ hrs/week
Self-reported health good/very good/excellent (not fair or poor)	✓	✓	✓	✓	
Self-reported health not poor (fair, good, very good, or excellent)	✓	✓		✓	
Health about the same or gotten better over last six months	✓	✓	✓		% High school diploma or GED
Number of days physical health good, past 30 days	✓	✓	✓		
Number days poor physical or mental health did not impair usual activity, past 30 days	✓	✓	✓	✓	
Number of days mental health good, past 30 days	✓	✓	✓	✓	% Female
Did not screen positive for depression, last two weeks	✓	✓	✓		

Notes: FPL represents household below the federal poverty line (in %), HHS represents household size, INS represents the nummber of non insurance months in last six months. The random forest model always splits on FPL and Age along with HHS and INS. Along with these variables the random forest also splits on different variables included in the last column. For example, consider the model called "Currently taking any prescription medications", the random forest splits (more than average) the data on FPL, Age, HHS, and % MSA. Therefore, the treatment heterogeneity is likely within these variables.

Appendix C

Efficient Policies

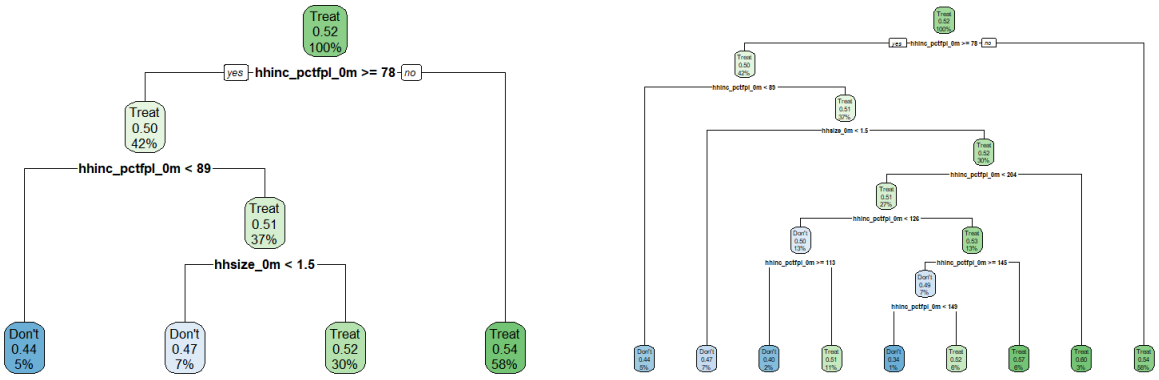
Figure C.1: Efficient Policy to Improve the Blood Cholesterol Check Participation



(a) Shallow tree **(b)** Deep tree

Notes: The hhinc_pctfpl_0m shows household income as percentage of the federal poverty line. The ins_months_0m_num shows numbers of months that a responder has insurance in last six months. The employ_hrsmwork_30_hrs_week shows > 0.5 shows the responder work more than 30 hours per/week. The hhsize_0m is household size.

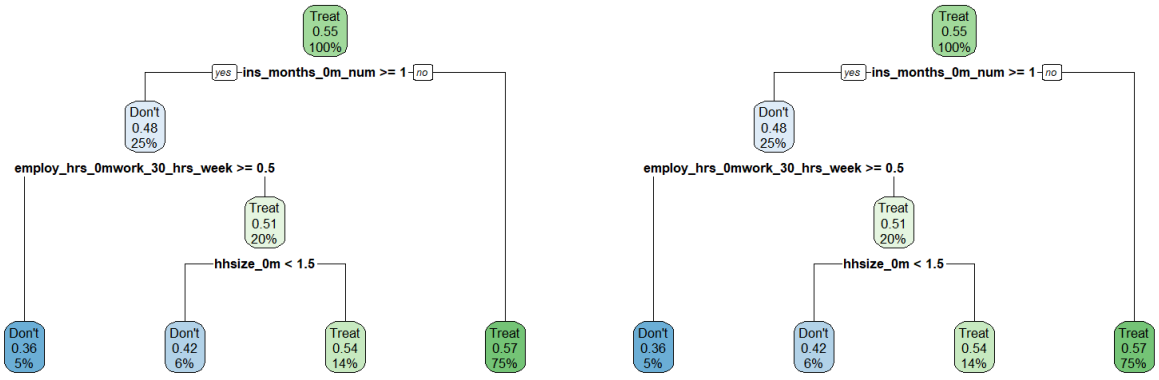
Figure C.2: Efficient Policy to Improve Blood Tests Participation for High Blood Sugar/Diabetes



(a) Shallow tree **(b)** Deep tree

Notes: The hhinc_pctfpl_0m shows household income as percentage of the federal poverty line. The ins_months_0m_num shows numbers of months that a responder has insurance in last six months. The employ_hrsmwork_30_hrs_week shows > 0.5 shows the responder work more than 30 hours per/week. The hhsize_0m is household size.

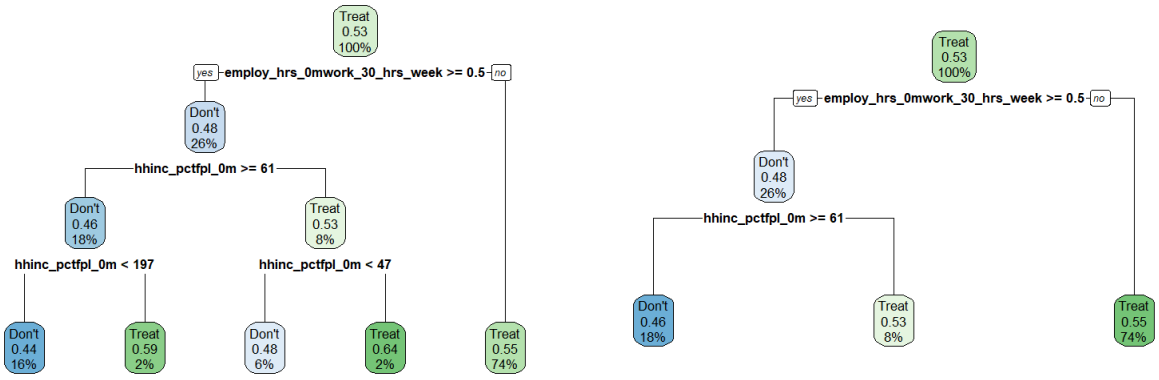
Figure C.3: Efficient Policy to Improve Mammogram Test Participation for Women



(a) Shallow tree **(b)** Deep tree

Notes: The hhinc_pctfpl_0m shows household income as percentage of the federal poverty line. The ins_months_0m_num shows numbers of months that a responder has insurance in last six months. The employ_hrsmwork_30_hrs_week shows > 0.5 shows the responder work more than 30 hours per/week. The hhsize_0m is household size. Valid only for women.

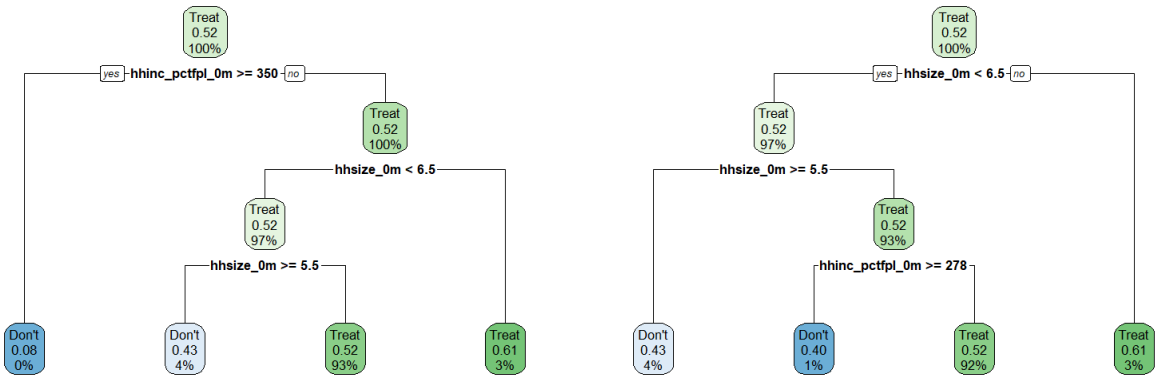
Figure C.4: Efficient Policy to Improve Pap Test Participation for Women



(a) Shallow tree **(b)** Deep tree

Notes: The hhinc_pctfpl_0m shows household income as percentage of the federal poverty line. The ins_months_0m_num shows numbers of months that a responder has insurance in last six months. The employ_hrsmwork_30_hrs_week shows > 0.5 shows the responder work more than 30 hours per/week. The hhsize_0m is household size. Valid only for women.

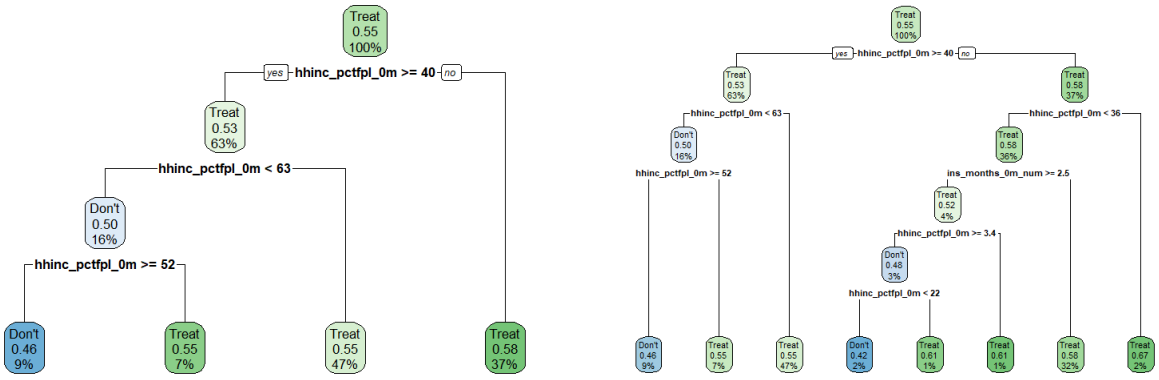
Figure C.5: Efficient Policy to Improve Self-reported Health



(a) Shallow tree **(b)** Deep tree

Notes: The hhinc_pctfpl_0m shows household income as percentage of the federal poverty line. The ins_months_0m_num shows numbers of months that a responder has insurance in last six months. The employ_hrsmwork_30_hrs_week shows > 0.5 shows the responder work more than 30 hours per/week. The hhsize_0m is household size. Valid only for women.

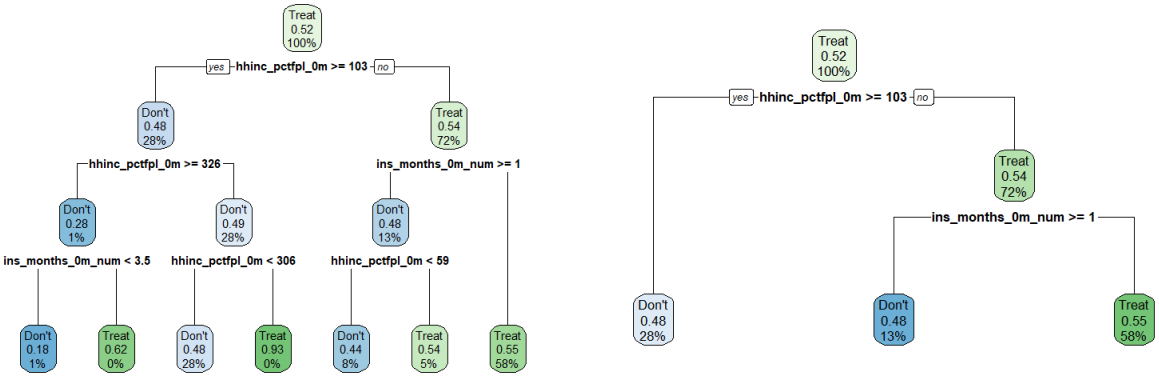
Figure C.6: Efficient Policy to Improve to have Usual Place of Clinic-based Care



(a) Shallow tree **(b)** Deep tree

Notes: The hhinc_pctfpl_0m shows household income as percentage of the federal poverty line. The ins_months_0m_num shows numbers of months that a responder has insurance in last six months. The employ_hrsmwork_30_hrs_week shows > 0.5 shows the responder work more than 30 hours per/week. The hhsize_0m is household size. Valid only for women.

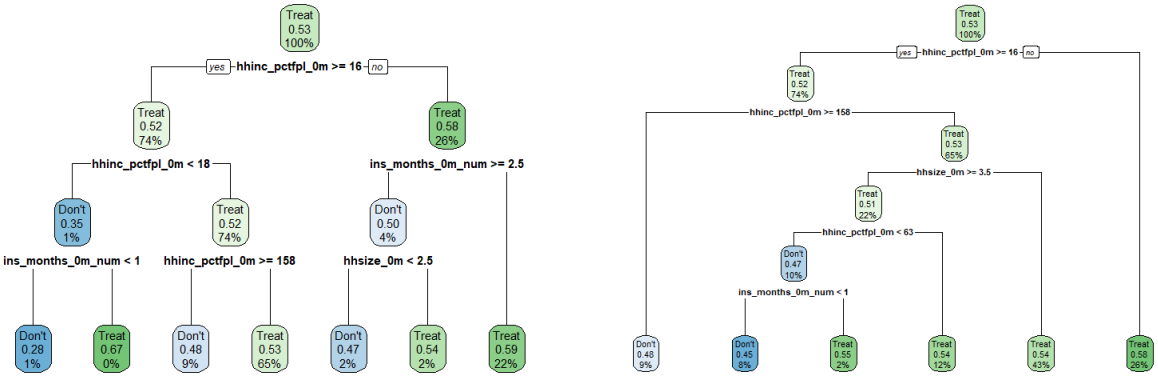
Figure C.7: Efficient Policy to Improve to have a Personal Doctor



(a) Shallow tree **(b)** Deep tree

Notes: The hhinc_pctfpl_0m shows household income as percentage of the federal poverty line. The ins_months_0m_num shows numbers of months that a responder has insurance in last six months. The employ_hrsmwork_30_hrs_week shows > 0.5 shows the responder work more than 30 hours per/week. The hhsize_0m is household size. Valid only for women.

Figure C.8: Efficient Policy to Improve to Post Health-care Service Happiness



(a) Shallow tree **(b)** Deep tree

Notes: The hhinc_pctfpl_0m shows household income as percentage of the federal poverty line. The ins_months_0m_num shows numbers of months that a responder has insurance in last six months. The employ_hrsmwork_30_hrs_week shows > 0.5 shows the responder work more than 30 hours per/week. The hhsize_0m is household size. Valid only for women.

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