Heterogeneous Effects of Medicaid and Efficient Policy Learning: Evidence from the Oregon Health Insurance Experiment*

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Abstract

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. 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 access to public insurance. However, identical policy intervention can often distinctly affect different individuals and subpopulations. This paper exploits Oregon's health insurance lottery selection to estimate the heterogeneous treatment effects of access to Medicaid on health care use, personal finance, health, and wellbeing. To establish this estimate, I use the cluster-robust generalized random forest − a causal machine learning approach. I find that the heterogeneous effects of Medicaid are more pronounced among poorer and older households, which is consistent with a standard adverse selection theory. Furthermore, I implement efficient policy learning strategies to identify policy-reforms that prioritize Medicaid allotments to the subgroups that are likely to benefit the most. These proposed reforms exclude gender, race, and residence variables for the ethical, legislative, and political rationale. On average, the proposed reforms improve average outcomes by a range of 4% to 10% over a random assignment baseline. My findings are useful for analysts, policymakers, and insurance planners to discover the underlying mechanisms that drive the health outcome results and to design or reform policy.

Keywords: Insurance, causal machine learning, heterogeneous treatment effect, efficient policy learning

JEL Classification:

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1 Introduction

This research exploits Oregon's health insurance lottery selection as an instrument and contributes to two primary domains that are relevant for policy development. First, unlike the series of papers[1] that have evaluated the average treatment effects of the Oregon Health Insurance Experiments on several outcomes, this paper contributes by estimating the heterogeneous treatment effect of lottery insurance on several issues of interest like health care use, financial strain, and self-reported physical and mental health. Second, this paper contributes insights regarding how to target health insurance interventions for effective policymaking. Understanding "who should be treated" with intervention is ubiquitous in policymaking. It can be unfair, unethical, and sometimes illegal to target policy to only a particular subpopulation. Moreover, intervening everybody in the population (a blanket policy) is welfare-maximizing but can be costly.[2]

As of May 13, 2019, 37 states and the District of Columbia have expanded Medicaid coverage for low-income adults to 138% of the federal poverty level through the Affordable Care Act (ACA). This provision to expand[3] the Medicaid program through the Affordable Care Act (ACA) has triggered a substantial nationwide debate among policymakers and diverse stakeholders about what effects - if any - insurance coverage has on the various dimension of health (Baicker, 2019). The findings of my paper provide valuable insights into some of the issues forward by the contemporary national debate. Furthermore, the results of my paper can exhibit the diverse impacts on the distinct population strata on health care use, personal finance, and wellbeing regarding the expansion of public access to insurance.

There exists an extensive literature studying the impact of insurance coverage on health outcomes which report average treatment effects. A concern with this literature is that establishing a causal effect is challenging due to endogeneity. Endogeneity arises because it is difficult to control for observed and unobserved confounding variables among the insured and uninsured population (Levy and Meltzer, 2008). For example, a comparison of the health between those with and without health insurance, can reveal that insurance 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).

A random assignment of insurance can circumvent such confounding problems (Finkelstein et al.,

[1] See Allen et al. (2010); Baicker et al. (2013, 2017, 2014); Baicker and Finkelstein (2011); Finkelstein et al. (2012); Grossman et al. (2016); Taubman et al. (2014); Zhou et al. (2017).

[2] 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).

[3] 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, 2019).

2012), and the Oregon Health Insurance Experiment renders a unique opportunity to test the causal effects of owning health insurance (Baicker and Finkelstein, 2011) on health and personal finance-related outcomes. In early 2008, the Oregon state had the budget to enroll an additional 10,000 adults to the state's Medicaid program. However, nearly 90,000 Oregon residents qualified for the program. 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). This lottery assignment of Medicaid in Oregon creates a randomized controlled study – the absolute gold-standard of experimental design – that allow comparison of various outcomes of the lucky Oregonians who received Medicaid to those who did not (Klein, 2013). In the year following the random assignment, 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 not the detectable improvements in physical health conditions like high blood pressure. The mishmash effects of Medicaid have presented policymakers with tough choices in balancing the costs and benefits of Medicaid (Baicker, 2019).

Randomized experiments like the Rand Health Insurance Experiment and the Oregon Health Insurance Experiment have generated some credible average effect estimates of expanding access to public health insurance on a population of interest. However, 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 effects of intervention vary across subpopulations. Identifying such heterogeneous treatment effects accommodate the discovery of underlying mechanisms that drive the results and allow for efficient design and reform of policy.

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 of standard "single" hypothesis tests are no longer valid because of the multiple hypothesis testing[4] problems (Lan et al., 2016; List et al., 2019). Moreover, performing ad-hoc searches or $p-$hacking[5] 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.[6] However, it may also prevent discovering unanticipated results and

[4]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%.

[5]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.

[6]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

developing new hypotheses (Athey and Imbens, 2016).

This paper implements the cluster-robust generalized random forest methods, developed by the Athey et al. (2019), to explore the heterogeneous treatment effects of the Oregon Health Insurance Experiment. This method re-engineers the strengths and innovations of the Breiman (2001) random forest, a predictive machine learning method, for causal inference. These modifications allow for systematic investigation of the heterogeneous treatment effects that are not prone to data mining and $p-$hacking. Moreover, these methods are especially useful when research includes high-dimensional covariates.

My paper shows the causal thresholds for distinct subpopulations where 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 provide a holistic perspective toward the mystery of mix-bagged Medicaid effects as reported by previous research.

I scrutinize these separate subgroup for 36 different outcomes of interest. These outcomes are extensive and intensive margins of health care use, preventive care use, financial strain, mental and physical wellbeing, and mechanisms of care, quality, and satisfaction of health care service usages. I find that the heterogeneous effects of Medicaid are more pronounced among poorer and older households. Impoverished families may need more medical care, and when Medicaid provides an opportunity, these households demand more health care in comparison to those who are uninsured just as standard adverse selection theory would predict.

Along with the heterogeneous treatment effects, "Who should get treatment?" is also a widespread issue in policy design. For example, whom to serve in youth employment programs (Davis and Heller, 2017), whom to allocate Medicare funding for hip or knee replacement surgery (Kleinberg et al., 2015), who should get job training, job search, and other assistance (Kitagawa and Tetenov, 2018). This paper implements the efficient policy learning strategies of Athey and Wager (2018) to answer how to set eligibility criteria to intervene with insurance coverage. This paper designs efficient policy rules considering two rationales − first, this paper constraint observable covariates like race, gender, and residence, etc. Constraining specific covariates is essential for ethical, legislative, and political considerations. Second, this paper follows the Kitagawa and Tetenov (2018) approach to design policy from an "intention-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[7] rate. I put forward efficient policies or reforms that can improve outpatient visits, preventive care utilization, self-reported health, having routine clinic-based care, having a personal doctor, and post-services happiness. I also quantify the cost of estimated policy rules in comparison to the random assignment policy. On average, the proposed reforms would improve average

randomized controlled trials is a reputable platform for conducting a preregistered analysis plan.

[7]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.

outcomes under random assignment by about 4% to 10% and also holds causal interpretation.

In summary, this research uses the Oregon Health Insurance Experiment public-use data and contributes to estimating the net impact of expanding access to Medicaid; examining the sources of treatment heterogeneity on such 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, my proposed reforms can help Oregon to develop a priority list against current blanket Medicaid policy which can help to reduce state budget-deficits[8] without hampering the current Medicaid welfare.

Section 2 summarizes the institutional background of the Oregon Health Insurance Experiment. Section 3 summarizes approaches to study health insurance and health outcomes and explains how causal machine learning can help to analyze different research questions. Section 4 lays out identification strategy and empirical methods for the cluster-robust random forest for heterogeneous estimation along with efficient policy learning strategies. Section 5 displays the results. Section 6 provides discussions on findings and concludes the study.

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 for the categorically eligible Medicaid population. Low-income children, pregnant women, welfare recipients, and poor elderly and disabled populations groups 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 who have been without health insurance for at least six months, have incomes below the federal poverty level and 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.,

[8]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.

2012; Baicker and Finkelstein, 2011). In 2001−2004, the average annual Medicaid expenditures for an individual on OHP Standard were about $3000, 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 Oregon state had the budget to enroll an additional 10,000 adults. However, 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 the new members through random lottery draws from a new reservation list (Finkelstein et al., 2012).

In early 2008, the Oregon state 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 by providing very little demographic information. The sign up form required few demographics information like 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 screen for program eligibility at sign up for the lottery to keep the entry barrier low. During a window from January 28 to February 29, 2008, a total of 89,824 individuals signed up. Ineligible individuals for the OHP Standard are excluded before the lottery. The exclusion comprises individual 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 during 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[9]. Up to eight supplemental forms, (Allen et al., 2010) could accompany it. 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

[9] "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).

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 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 random assignment of lottery and OHP Standard application form, an "initial survey" was conducted and again after a year, the "main survey" was performed. These surveys consist of data for 58,405 individual comprising 29,589 individuals in treatment and 28,816 individuals in the control group.

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 within small subgroup populations. For example, the public health insurance reduces mortality among infants and children (Currie and Gruber, 1996a,b; Hanratty, 1996) while for the elderly, public health insurance improves different outcome but not mortality (Card and Maestas, 2008; Finkelstein and McKnight, 2008; McWilliams et al., 2007b,a). Third, the nature of 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 & health outcomes. However, these approaches are less likely to address the issues of unobservable confounders between health insurance & 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 events 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 and therefore, 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). These "one size fit 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. However, recently, due to 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 utilizes flexible model selection, model ensembles, high dimensional data environment, and cross-validations. Therefore these

algorithms are useful in many policy applications where the causal inference is not central or may not be necessary. 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. Then they 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.

The machine learning algorithms behave well for out-of-sample prediction but 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 the 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 mainly 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, this paper utilizes efficient policy learning strategies of Athey and Wager (2018) to explore some strategies that can help to reform or redesign access to public health insurance programs.

4 Empirical Strategy

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. Note, shortly after the lottery assignment − that allows lucky Oregonians to apply for the OHP Standard Medicaid and the collection of the OHP Standard application form from these lucky Oregonians − an "initial survey," was conducted. After a year, 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 individual 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 Finkelstein et al. (2012) approach. This paper contemplates a situation where an analyst knows her outcome variable (Y) at the post-treatment and has 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 $\left(Y_i^{obs}, W_i, X_i\right), \ \forall \ i = 1, \ldots, N$. 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:

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

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.

$$\tilde{x}_{i,h} = \gamma_0 + \gamma_1 W_{i,h} + \eta_{ih} \tag{1}$$

where $\tilde{x}$ is a observable demographic variable in the pre-treatment period, γ_0 is 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. Table 1 exhibits the results.

4.3 Intent to Treatment 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"[10]. Under the assumption of sparsity[11], 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 outcomes 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

[10]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 $\min_{\beta_1 \in \mathbb{R}^p} E_n \left[(\tilde{y}_i - {}_i \Theta \beta_1)^2 \right] + \frac{\lambda}{n} \|\beta_1\|_1$, 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.

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

correct the inference with usual heteroscedasticity robust OLS standard error.

$$Y_{i,h} = \beta_0 + \beta_1 W_{i,h} + x_{ih}\beta_2 + \varepsilon_{it} \tag{2}$$

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.

4.4 Local Average Treatment Effect of Lottery

The ITT estimates from equation 2 provides 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 word, policymakers are interested in the causal effect of compliance to the lottery and not just winning the lottery. The "complier" is the subset[12] of individuals who obtain insurance on winning the lottery and who would not obtain insurance without winning the lottery. One way to retrieve this parameter is to utilize lottery selection as an instrument and perform a two-stage least square (2SLS). Equation 3 represents the first stage equation and second stage equation respectively.

$$\begin{aligned} Z_{i,h} &= \delta_0 + \delta_1 W_{i,h} + x_{ih}\delta_2 + \mu_{it} \\ Y_{i,h} &= \phi_0 + \phi_1 \widehat{Z}_{i,h} + x_{ih}\phi_2 + \nu_{it} \end{aligned} \tag{3}$$

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

[12] 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 causal impact of insurance among the "compliers". For just identified model, the LATE estimates, ϕ_1, is the ratio of ITT estimates from equation 2 and the first-stage coefficient on winning the lottery from equation 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).

4.5 Heterogeneous Treatment Effects of Lottery

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 insurance 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 Athey and Wager (2019) cluster-robust random forest approach 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)[13], a machine learning algorithms for causal inference. The remaining methods extend the "causal tree" approach utilizing the Breiman (2001) random forest[14] machine learning algorithm for causal inference.

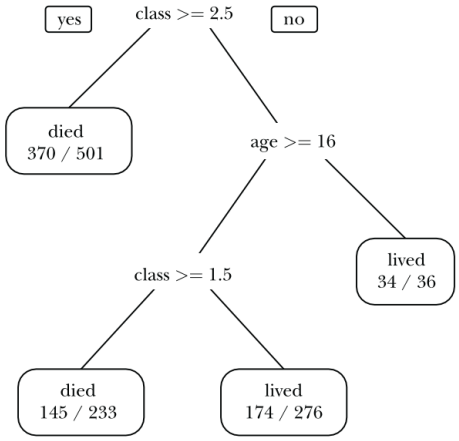
In a nutshell, CART recursively filters and partitions the large data-set into binary sub-groups (nodes) such that the samples within each subset become more homogeneous in their fit of the response variable, thus resulting in a tree-like format. Figure 1 shows an example of features of the Titanic survivors using the CART method, as shown 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) approach. 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 a "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

[13] 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).

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

Figure 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 a 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.

is valid no matter how many covariates are used in estimation. This paper employs the Chernozhukov et al. (2018a) cross-fitting approach (explained later in this section) to prevent the loss of precision.

One caveat of the causal tree is that it does not 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, which can provide personalized estimates. This method provides causal effects that change more smoothly with covariates and provides distinct individualized estimates and confidence intervals. Wager and Athey (2018) also provide an important 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 estimate how a causal parameter of interest varies with covariates.

4.6 Cluster-robust Random Forest

In a nutshell, the random forest approach makes prediction from an average of b 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:

$$\hat{\mu}(x) = \frac{1}{B}\sum_{b=1}^{B}\sum_{n=1}^{n}\frac{Y_i \mathbf{1}\left(\{X_i \in L_b(x), i \in S_b\}\right)}{|\{i : X_i \in L_b(x), i \in S_b\}|} \tag{4}$$

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 $i \notin S_b$. 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 4 as

$$\hat{\mu}(x) = \sum_{i=1}^{n} a_i(x) Y_i; \quad a_i(x) = \frac{1}{B}\sum_{b=1}^{B}\frac{Y_i \mathbf{1}\left(\{X_i \in L_b(x), i \in S_b\}\right)}{|\{i : X_i \in L_b(x), i \in S_b\}|} \tag{5}$$

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. The treatment effect estimation is

$$\hat{\tau} = \frac{\sum_{i=1}^{n} a_i(x_i)\left(Y_i - \hat{m}^{(-i)}(X_i)\right)\left(W_i - \hat{e}^{(-i)}(X_i)\right)}{\sum_{i=1}^{n} a_i(x_i)\left(W_i - \hat{e}^{(-i)}(X_i)\right)} \tag{6}$$

where, $e(x) = P[W_i|X_i = x]$ is the propensity score or probability of being treated; $m(x) = P[Y_i|X_i = x]$ is expected outcomes marginalizing over treatment; $(-i)$ superscript denote "out-of-bag" or "out-of-fold" prediction. Causal forest has several tuning parameters[15] and the cross-validation on the "$R-$learner" objective function helps to select these tuning parameters. Nie and Wager (2017) showed that "$R-$learner" objective function for heterogeneous treatment effect estimation as

$$\hat{\tau}(\cdot) = \arg\min_{\tau}\left\{\sum_{i=1}^{n}\left(\left(Y_i - \hat{m}^{(-i)}(X_i)\right) - \tau(X_i)\left(W_i - \hat{e}^{(-i)}(X_i)\right)\right)^2 + \lambda_n(\tau(\cdot))\right\} \tag{7}$$

where, $\lambda_n(\tau(\cdot))$ is a regularize that controls the complexity of the learned $\hat{\tau}(\cdot)$ function.

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 prediction using these two first-stage forest. 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 7, chooses the tuning parameters for causal forest.

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

[15] 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.

Equation 8 exhibits effectiveness of intervention in individual, household, and global levels.

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

$$\widehat{\Gamma}_i = \hat{\tau}^{(-i)}\left(X_i\right) + \frac{W_i - \hat{e}^{(-i)}\left(X_i\right)}{\hat{e}^{(-i)}\left(X_i\right)\left(1 - \hat{e}^{(-i)}\left(X_i\right)\right)} \left(Y_i - \hat{m}^{(-i)}\left(X_i\right) - \left(W_i - \hat{e}^{(-i)}\left(X_i\right)\right)\hat{\tau}^{(-i)}\left(X_i\right)\right) \tag{8}$$

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 the "doubly-robust" estimator with cross-fitting Chernozhukov et al. (2018a). The household-level effectiveness of lottery intervention is $\hat{\tau}_j$. 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).

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 Athey and Wager (2019). Davis and Heller (2017) also uses this approach to test for heterogeneity. However, another formal test is based on "best linear predictor" or BLP method of Chernozhukov et al. (2018b). The main idea is to compute 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 greater than 0, then one can reject the null of no heterogeneity. However, asymptotic results justifying such inference are not presently available.

4.7 Estimation of Treatment Policies

The optimal policy estimation has received greater attention in the machine learning literature[16] (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 in policymaking because an ad-hoc targeting of 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 (2018). 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, 2018).

Once, a policymaker understands the heterogeneity effect. She would like to assign the correct treatment to each individual or subpopulation. For that, I implement the Athey and Wager (2018) 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:

$$R(\pi) = E\left[Y_i\left(\pi^*\left(X_i\right)\right)\right] - E\left[Y_i\left(\pi\left(X_i\right)\right)\right] \tag{9}$$

Under unconfoundedness, the overlapping assumptions and binary treatment assignment Athey and Wager (2018) purpose 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

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

called double machine learning of Chernozhukov et al. (2018a) and given as:

$$\hat{\Gamma}_i = \hat{\mu}_{+1}^{-k(i)}(X_i) - \hat{\mu}_{-1}^{-k(i)}(X_i) + W_i \frac{Y_i - \hat{\mu}_{W_i}^{-k(i)}(X_i)}{\hat{e}_{W_i}^{-k(i)}(X_i)} \tag{10}$$

This is 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. I use L_1-penalty logistic regression to estimate $\hat{\mu}$ and $\hat{e}$ with $k(i)$ fold cross-validation. 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 (2018) 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 $\sqrt{VC(\Pi)/n}$.

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 virtually 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 are below the federal poverty level and have assets below $2,000.

5.0.1 Pre-treatment Comparison of Demographic Characteristics

Employing equation 1, Table 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 1 illustrates the mean of the control group and the difference of means between treatment group and 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.

Table 1: Pre-treatment Comparison of Demographic Characteristics

Variable	Control mean	Mean diff	Variable	Control mean	Mean diff
% Female §	0.600	-0.015***	% dont 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 disploma 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.

5.1 ITT, LATE and Heterogeneous Treatment Effects

The treatment effect often varies with an individual's observable characteristics. For example, if the treatment is costly and less accessible, then only those who are likely to benefit most will take up 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 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 an analyst knows her outcome variable, (Y), at post-treatment and has 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 important treatment effect modifiers. For example, some household 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, 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.

Table 2, 3, 4 and 5 comprises 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) represents "intent-to-treat" effect implementing double-selection post-LASSO method. Column (2) details a local average treatment effect 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, column (4), (5), and (6) explore the treatment heterogeneity. Column (4) provides a 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. Column (5) and (6) provides 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 mean 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 exhibit which subpopulations are more or less susceptible to Medicaid. However, a heatmap is a partial representation of overall treatment heterogeneity. Therefore, 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 which should be taken into consideration for proper interpretation of heterogeneous treatment effects. Appendix B provides relevant variables list to explain each of the heatmaps in this section.

5.1.1 Health Care Utilization

Table 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 estimates in Table 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 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 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, Mammogram test (for women of age 40+), or Pap test (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 2 column (4) renders the heuristic approach to test the treatment heterogeneity. Evidence of treatment heterogeneity for outpatient usages and preventive care utilization are found. Table 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 federal poverty along with the household size and other variables like education, and employment. Appendix A provides variable importance table for all the outcome variables. It lists the variables which were split (more

Table 2: Health Care Utilization

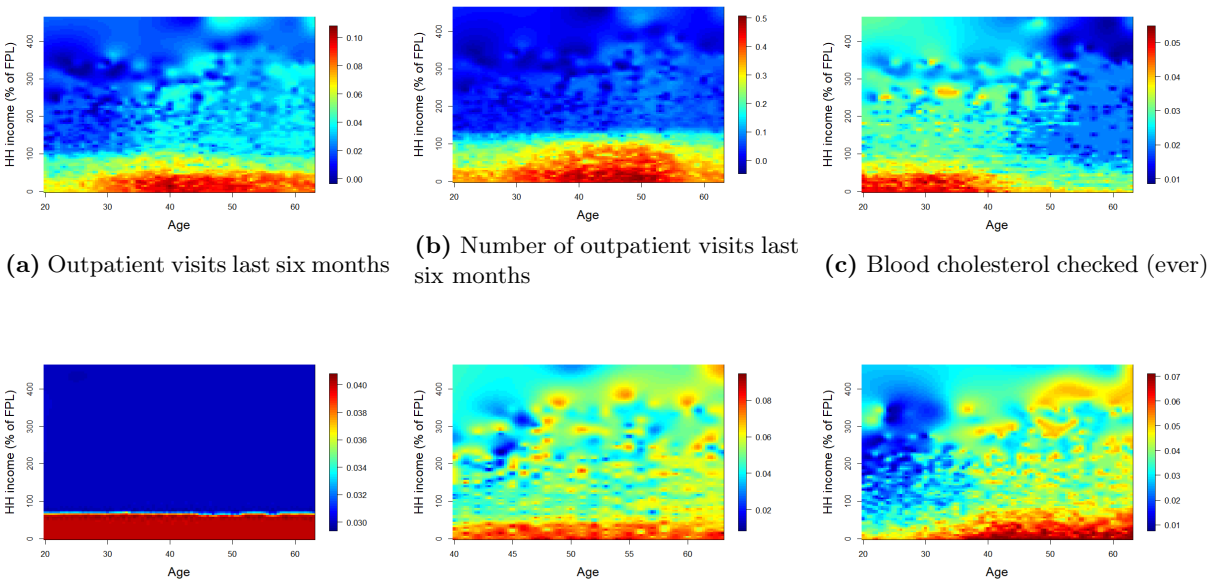
Outcome variables	ITT (1)	LATE (2)	ATE (3)	Heuristic (4)	MFP (5)	DFP (6)
Panel A: Health care utilization						
Extensive margins						
Currently taking any prescription medications	0.021**	0.067**	0.007	-0.018	0.801	-0.494
	(0.009)	(0.03)	(0.009)	(0.018)	(1.015)	(0.734)
Outpatient visits last six months	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.

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 in the x−axis and Household Income below the FPL (in percentage) in the y−axis.

Figure 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 cares. Figure 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

Figure 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.

belong to the household below 100% of the FPL, regardless of age cohorts. Similar results are valid for the intensive margin of outpatient usage CATE; however, there exist some additional heterogeneity for different age-cohorts.

Figure 2 panel (c) exhibits treatment heterogeneity if the blood test for cholesterol level were ever done within the study period. Mostly younger age cohorts, between 20 to 40, who belong to the poorer household have a higher likelihood for this preventive test. Figure 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 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 of Age 40 years and above who belongs below 50% of the FPL household are highly likely to 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 100% below the FPL are likely to test for the PAP.

5.1.2 Financial Strain

Table 3 displays extensive margins and intensive margins of the financial strains. Winning the 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 dollars terms as the net effect of winning the lottery. The ITT and ATE ranges describe that winning the lottery relates with reductions of various types of out-of-pocket cost 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 lottery winning group also has nearly $450 to $500 on average reduction of the medical debts.

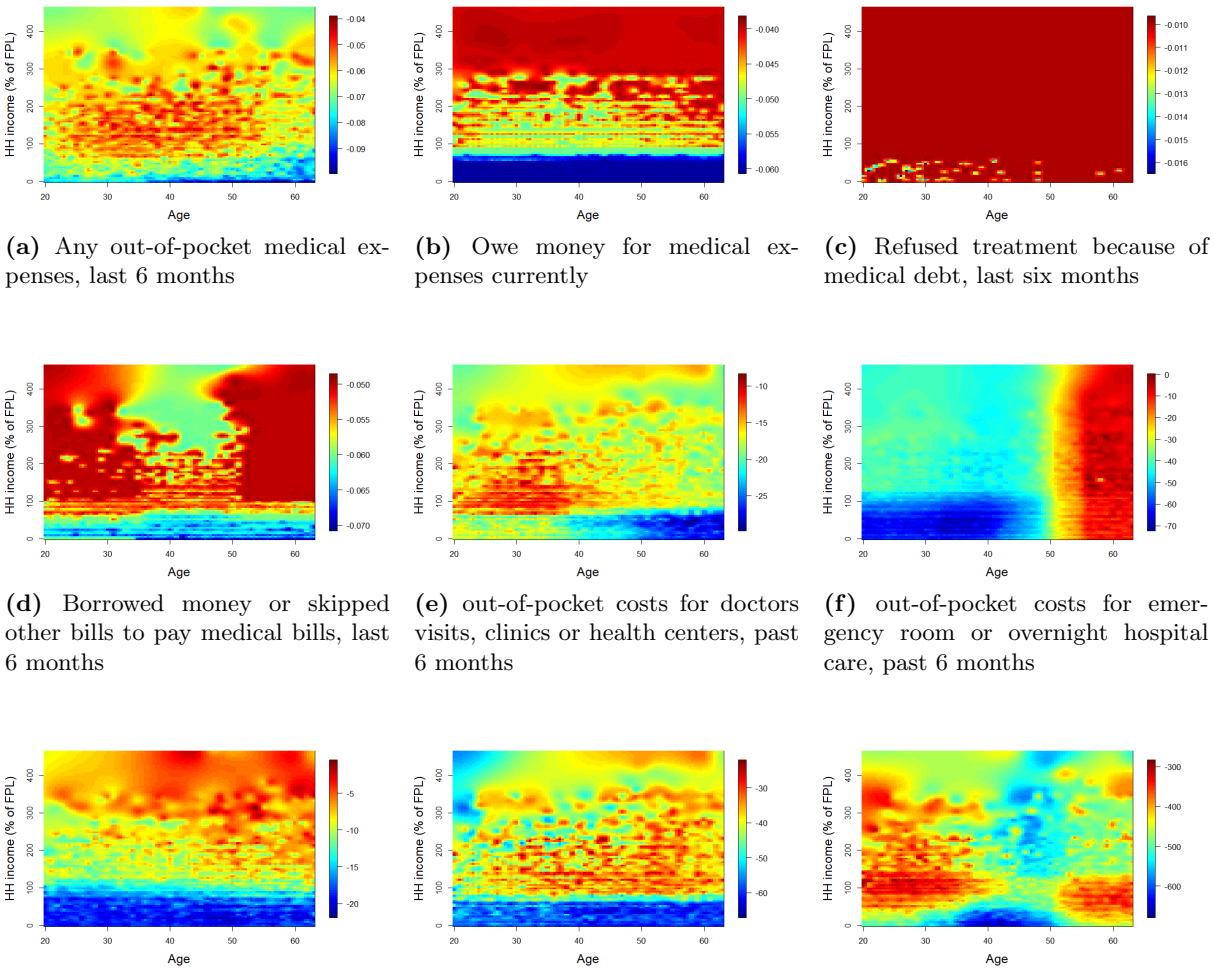
Table 3: Financial Strain

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.

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 illuminates some sources of treatment heterogeneity.

Figure 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 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.

The heatmap of Figure 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 winner of all age group and all household but the effects are more pronounced for lottery winning household with income that ranges below 80% the FPL and age group of above 40 years. Figure 3 panel (b) exhibits a sharp discontinuity

of owing money for medical expenses for lottery winning household 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 Finkelstein et al. (2012).

Figure 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 resist care. The Emergency Medical and Treatment Labor Act (EMTLA) enacted by Congress in 1986 explicitly prohibits the denial of care to indigent or uninsured patients even if they cannot pay.

Figure 3 panel (d) shows lottery winners have an overall reduction of borrowing money or skipping other bills to pay medical bills compared to the control group. However, the effect is more pronounced for lottery winning household 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 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 house 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 reductions of such cost, compared to the control group. Figure 3 panel (f) shows about \$60 to \$70 or little more reduction in the out-of-pocket costs for emergency room or overnight hospital care in past six months for 40 below age group for the lottery winning household with income below 100% of the FPL. The reduction of such cost is less than \$20 for above 50 years, regardless of their household-level income status. The remaining subgroup of age below 50 who belong to a household with income more than 100% of the FPL has about \$30 to \$50 reductions on the cost of the out-of-pocket costs for emergency room or overnight hospital. Figure 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 panel (h) illuminates that the lottery winners who belong to the 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 panel (i) exhibits the decline of the total amount currently owed for medical expenses. Compare to the control group, the treatment group with 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 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.

5.2 Self-reported Health

Table 4 describes the effectiveness of the Oregon Health Insurance Experiment in the various dimension 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 is even higher for compliance subgroup. There exist detectable treatment heterogeneity.

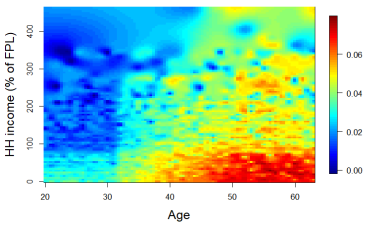
Table 4: Self-reported Health

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)

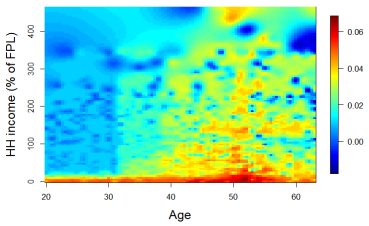
Notes: The ***, **, and * represent 1%, 5%, and 10% level of significance, respectively. Enclosed in the parenthesis are household-level clustered heteroscedasticity-consistent standard errors. The regressions in columns (1) and (2) include household size dummies, survey wave dummies, and survey wave 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.

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 points of references to what good health represent 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 4 panel (a) renders, compared to the control group, the lottery winning subgroup of age above 40 from the household income below

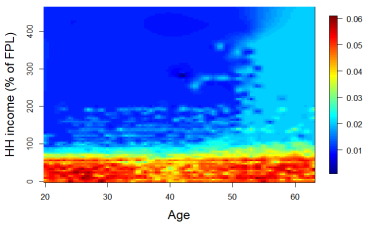
Figure 4: Self-reported Health



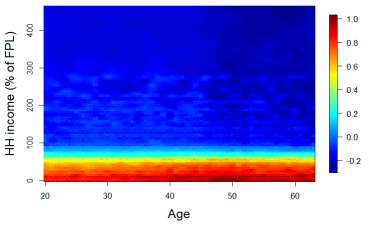
(a) Self-reported health good/very good/excellent (not fair or poor)



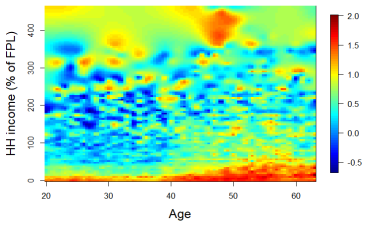
(b) Self-reported health not poor (fair, good, very good, or excellent)



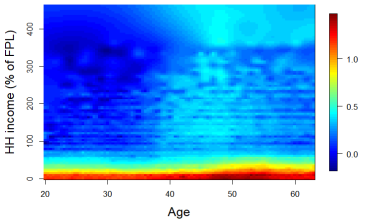
(c) Health about the same or gotten better over last six months



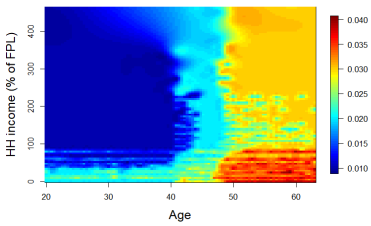
(d) Number of days physical health good, past 30 days



(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 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.

100% of the FPL are more likely to report better health. Again, the choice options are recoded 1 for the self-reported health not poor (fair, good, very good, or excellent), and 0 for poor. Only the small subgroup of age around 50 reported at least not poor health, as exhibited in Figure 4 panel (b). Figure 4 panel (c) depicts heterogeneity for another question regarding if responder perceived the health better or worse health in comparison to last six months. The lottery winners from the household income below 70% of the FPL report better health to compare to the control group. When asked to quantify the number of good physical health days in the past 30 days, lottery winning household closer to the FPL report higher numbers, as presented in Figure 4 panel (d). However, in Figure 4 panel (e), the number

of good mental health days in past 30 days is reported to be higher for the age group above 40 from the lottery winning household 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 4 panel (f). Figure 4 panel (g) shows the age group 50 above who are from a household below 100% of the FPL are more likely not to be detected as positive for depression (in last two weeks). In all these panels, it is repeatedly observed the lottery winning poorer household report slightly better health compared to lottery winning. These results could arise due to adverse selection. As the theory suggest, that poorer requires better health care, and when they get access to health care, they report better health to compare to their counterpart.

5.3 Potential Mechanism for Improved Health

Table 5 depicts some potential mechanism by which health insurance could have improved objective physical health along the heterogeneities in these mechanisms. Table 5 column (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-outs 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 non-emergence (last six months), there are treatment heterogeneities in the access to care, quality of care, and happiness detailed in Table 5 column (4), (5), and (6).

Figure 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 4 panel (a) exhibits a clear threshold that the household 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 4 panel (b) depicts households income above the FPL 100% with age above 40 years are less likely to have a personal doctor compared to the household income below the FPL 100% with age below 40 years. Most of the poorer household are likely to get all the needed medical care (Figure 4 panel (c)) and drugs (Figure 4 panel (d)) while household income below the FPL 50% and above 40 age are less likely to avoid the use of the ER for non-emergencies (Figure 4 panel (e)). Perceived quality of care is very uniformly distributed among the household and all ages (Figure 4 panel (f)). However, age above 40 in the households with income below 180% the FPL are more likely to have perceived happiness (Figure 4 panel (d)).

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

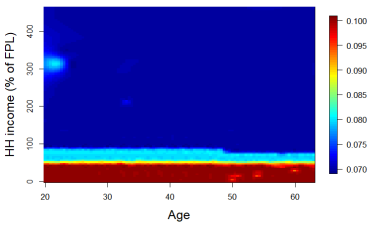
5.4 Efficient Policies

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 by Athey and Wager (2019), I compute doubly robust scores for the treatment effect as in equation 10, and learning policies empirical maximization as in equation 9.

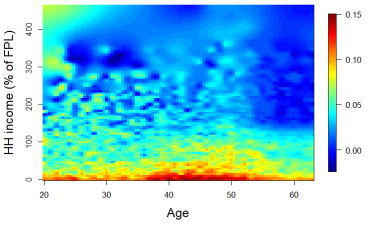
Table 6 column (1) details the average outcome for each policy variable of interest under the random assignment of treatment. Table 6 column (2) to (5) presents the estimates of the average outcome improvement (in percentage) of various policies over a random assignment baseline for the selected variable of interest. Efficient policy for each of the variable 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 6 column (2) the assignment policy is based on a probability rule. The 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 6 column (3) the assignment policy is the CATE rule, i.e., assign Medicaid if CATE is positive. In Table

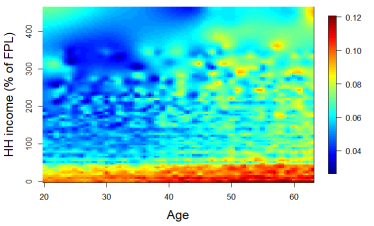
Figure 5: Potential Mechanism for Improved Health



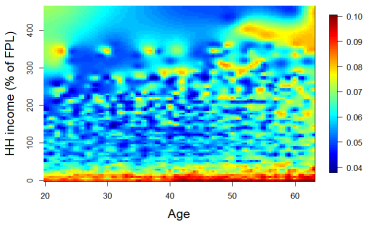
(a) Have usual place of clinic-based care



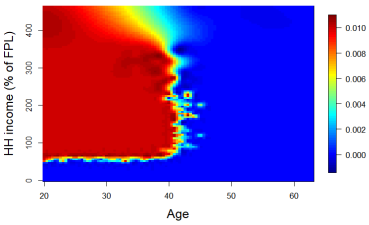
(b) Have personal doctor



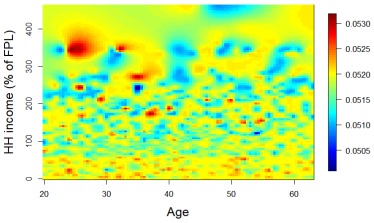
(c) Got all needed medical care, last six months



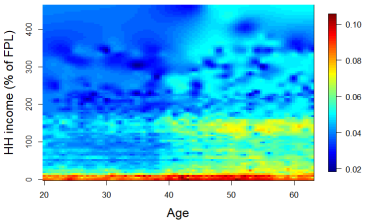
(d) Got all needed drugs, last six months



(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 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.

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 6 Panel A, column (1) describes the percentage of the households with an outpatient visit 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

Table 6: Estimate of the utility improvement of various policies over a random assignment baseline.

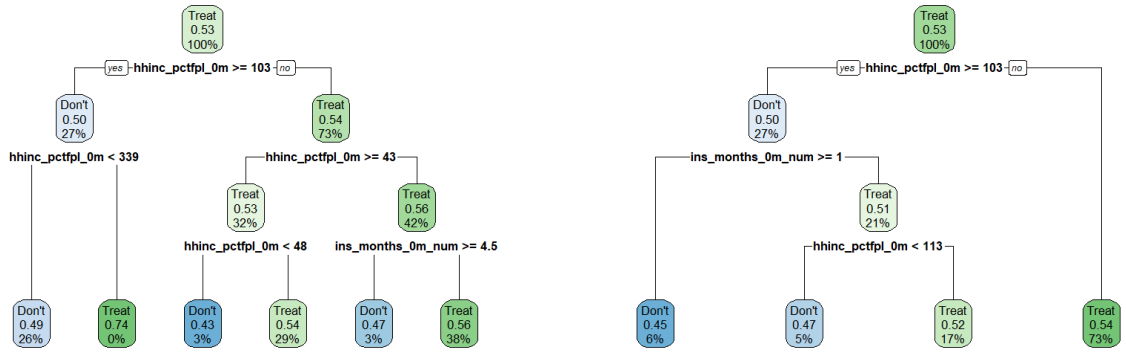
Variable	Random assignment (1)	Probability rule (2)	CATE rule (3)	Shallow tree (4)	Deeper tree (5)
Panel A: Health care utilization					
Outpatient visits	0.604***	4.74***	5.119***	4.228***	2.898***
last six months	(0.002)	(0.182)	(0.17)	(0.197)	(0.177)
Panel B: Preventive care utilization					
Blood 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 eligible registrants using the probability rule, then it would improve outpatient visit by additional 4.74%. Panel A, column (3), exhibits, if the Medicaid assignment is based on the CATE rule, then it would improve outpatient visit by 5.12%. The optimal depth-3 policy tree or shallow tree would improve outpatient visit by additional 4.23%. The optimal depth for policy trees based on the cross-validation for pruning would improve outpatient visit by extra 2.9%. All of these improvements are statistically significant.

Figure 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. And it can be interesting to examine them to gain an intuition for the stability of the learned rule. Table 6 exhibits the stability of learned rule. However, Figure 6 is a graphical depiction of a learned policy and can be different for different subsets of the data. To save space, learned efficient policies for the rest of the variables that are presented in Table 6 are

Figure 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 6 exhibits the stability of learned rule, however, Figure 6 is a graphical depiction of a learned policy and can be different to different subsets of the data.

compiled in Appendix C.

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, this 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 by 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 an analyst knows her outcome variable, (Y), at the post-treatment and has 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 effect 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 more pronounced among household below 100% of the federal poverty line. A possible answer would be that more an impoverished family may need more medical care. Medicaid provides an opportunity for these households to gain access to health care, and they, therefore, may utilize more health care than those who are uninsured which 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 which 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 Oregons 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. 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. 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 of 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.

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A Causal machine learning approaches

A.1 Average treatment effect

In this paragraph, I show a few examples of 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"[17] method and purpose a correction[18] called the "double-selection post-LASSO"[19] method. This method is useful for estimating average treatment effect when the analyst is required to select a "sparse" outcome model[20] 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"[21] method and LASSO method and purpose "residual balancing"[22] approach for estimating average treatment effect under the assumption of unconfoundedness[23] 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

[17] 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 $\min_{\beta_1 \in \mathbb{R}^p} E_n \left[(\tilde{y}_i - {}_i\Theta\beta_1)^2 \right] + \frac{\lambda}{n} \|\beta_1\|_1$, 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.

[18] 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.

[19] 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 usual heteroscedasticity robust OLS standard error.

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

[21] 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).

[22] 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).

[23] 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.

similar to Frish-Waugh-Lovell theorem[24] and close to the concept of Robinson (1988) residual-on-residual regression approaches where the estimator was a kernel regression.

A.1.1 Heterogeneous treatment effects

Along with the average treatment effect, heterogeneous treatment effects estimation interests policy-makers 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) point 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 recent 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 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) utilize the Breiman et al. (1984) classification and regression tree (CART)[25] 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 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

[24] 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.

[25] 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 a tree format (Athey and Imbens, 2017b).

(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 "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 show 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 estimate how a causal parameter of interest varies with covariates.

A.1.2 Optimal policy estimation

The optimal policy estimation have received greater attention in the machine learning literature[26] (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 estimation 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 (2018). 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 (2018).

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

B Variable importance

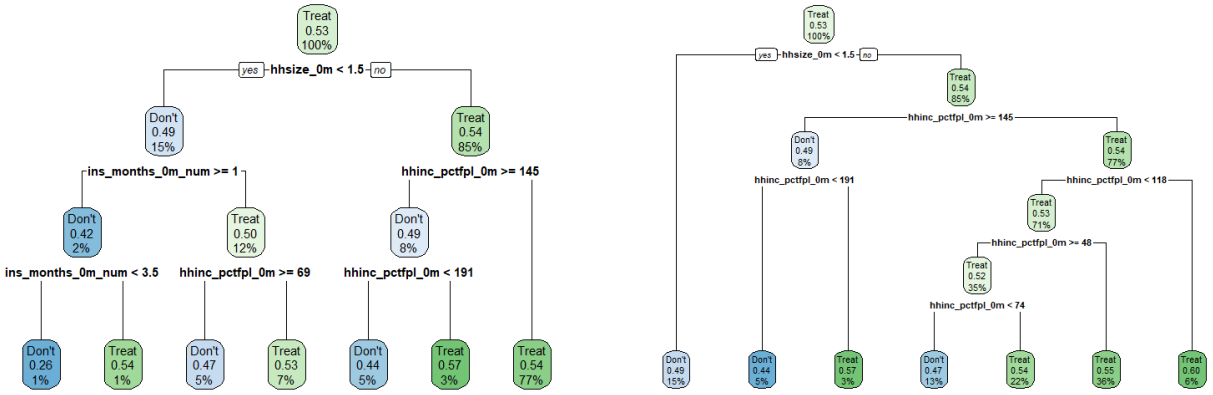
Table 7: Variable importance

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.

C Efficient Policies

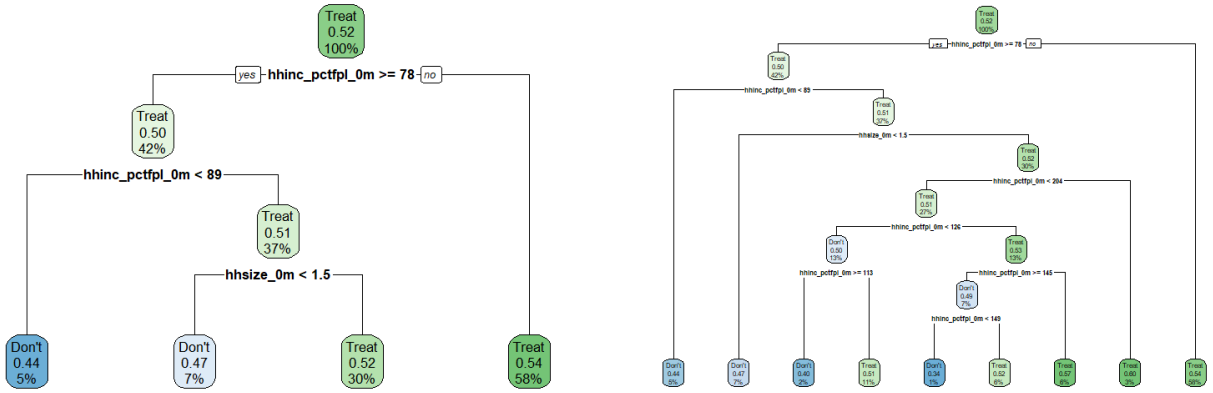
Figure 7: Efficient policy to improve the blood cholesterol check participation.



(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.

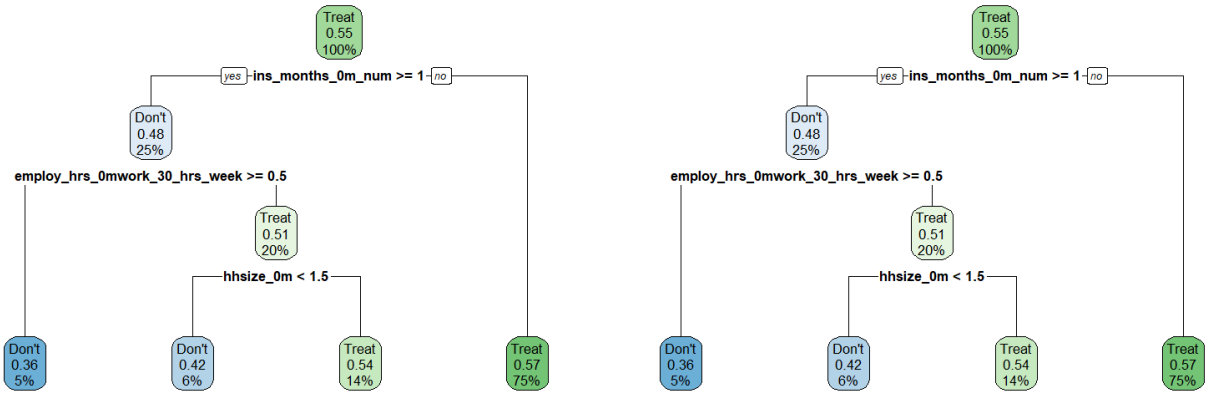
Figure 8: Efficient policy to improve blood tests participation for high blood sugar/diabetes.



(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.

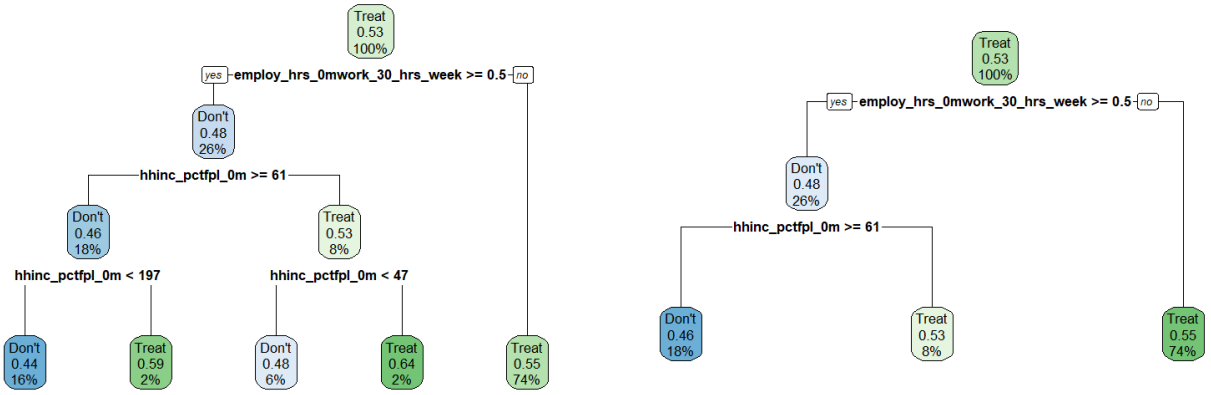
Figure 9: Efficient policy to improve Mammogram test participation for women.



(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.

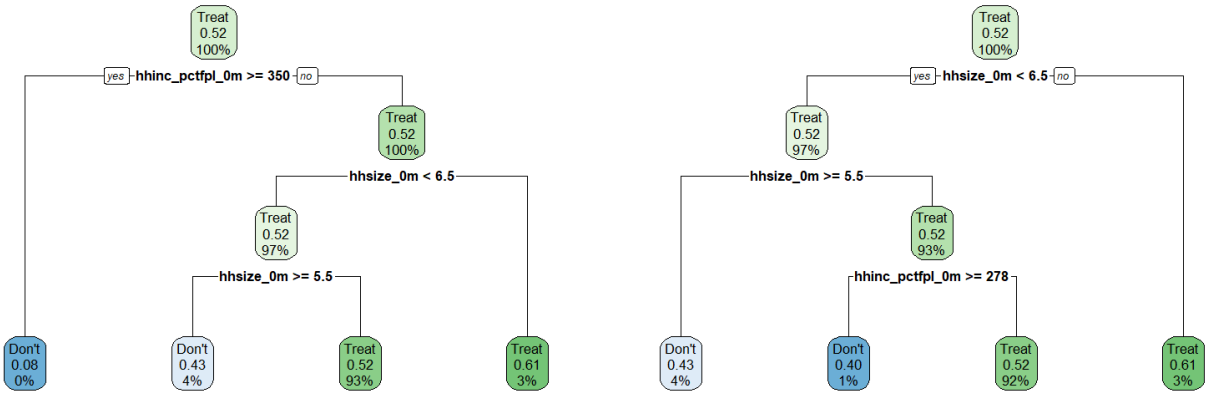
Figure 10: Efficient policy to improve Pap test participation for women.



(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.

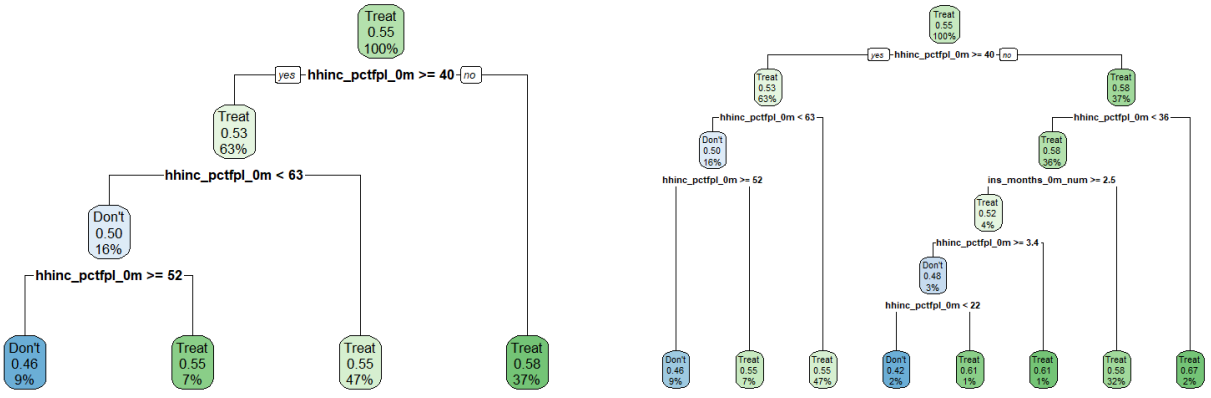
Figure 11: Efficient policy to improve Self-reported health.



(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.

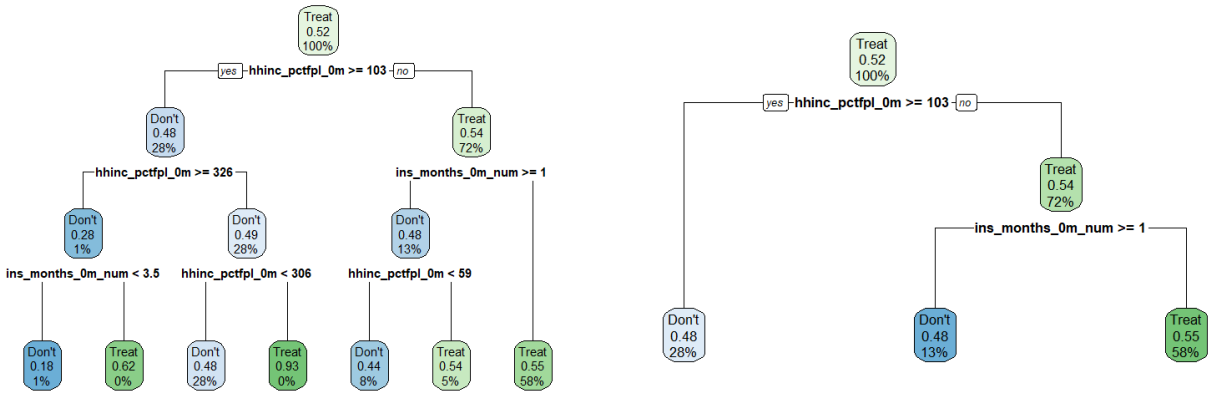
Figure 12: Efficient policy to improve to have usual place of clinic-based care.



(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.

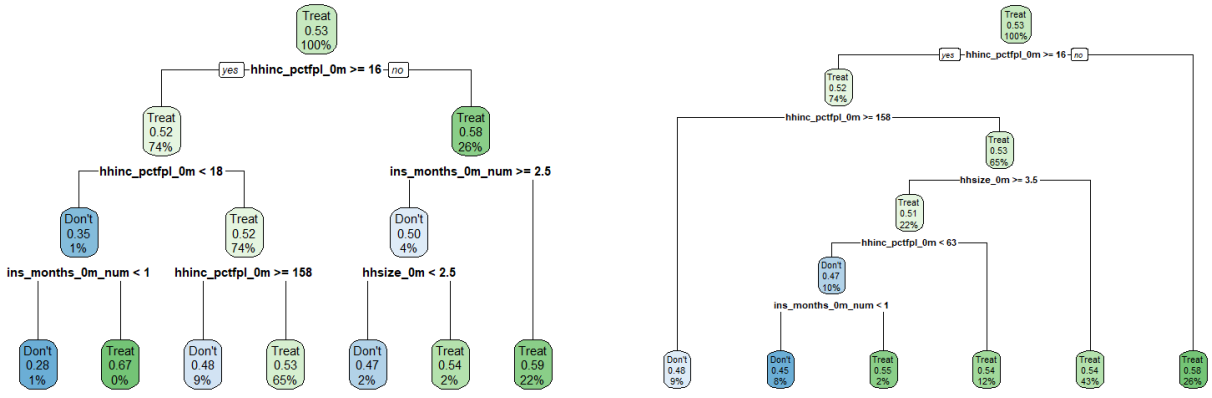
Figure 13: Efficient policy to improve to have have a personal doctor.



(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 14: 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.