Risk, Liquidity, and the Heterogeneous Value of Medicaid: Evidence from the Oregon Health Insurance Experiment

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Abstract

This paper develops and tests a two-channel model of public health insurance value using the Oregon Health Insurance Experiment (OHIE). Building on Rothschild-Stiglitz and Nyman, I formalize how Medicaid generates welfare gains through (i) consumption-smoothing for risk-averse but liquidity-unconstrained households and (ii) liquidity-relaxing access value for financially constrained households. The model predicts that Medicaid's financial protection should increase with ex-ante health risk, decrease with liquidity, and exhibit a negative risk-by-liquidity interaction, producing the largest gains for households that are both high-risk and liquidity-constrained.

Using the OHIE lottery as a strong instrument, I estimate heterogeneous treatment effects among around 16,500 low-income adults with rich baseline data. An instrumental forest estimator non-parametrically recovers the Conditional LATE (C-LATE) of Medicaid on medical debt, revealing substantial heterogeneity. The survey-weighted mean of the forest C-LATEs is -12.3 percentage points, somewhat smaller in magnitude than the benchmark Wald LATE of -17.4 percentage points from a conventional IV specification, but this average masks large variation across the risk–liquidity distribution: low-risk/low-constraint households experience limited effects (about -6.8 percentage points), while high-risk/high-constraint households see reductions of about -16.5 percentage points in debt prevalence. A second-stage regression of C-LATEs on theoretical indices explains 37% of this variation and yields large, oppositely signed main effects of risk and liquidity (roughly -5.8 and +6.2 percentage points per index unit) and a large negative interaction (-9.1 percentage points), consistent with the model's key prediction that risk and liquidity channels are jointly operative and non-additive.

These results demonstrate that Medicaid's financial protection is not a uniform income effect but is instead concentrated among households where high expected medical costs meet binding budget constraints. Counterfactual policy simulations that explicitly account for noncompliance demonstrate that targeting lottery offers based on predicted ITT gains can deliver a disproportionate share of the aggregate debt reduction while extending offers to only a subset of the eligible population. These findings highlight the value of combining theory-driven heterogeneity analysis with causal machine learning to design more efficient social insurance policies.

Introduction

Medicaid is a joint federal and state program that provides health coverage to over 70 million Americans (Medicaid, 2025) [13] at an annual cost of around $900 billion in 2023 (CMS, 2025) [6]. This public expenditure, representing approximately 18% of all national health spending, primarily provides healthcare coverage for eligible low-income individuals and is rationalized by two primary goals: improving health outcomes and, critically, providing financial protection against the high cost of medical care. Yet, severe medical-related financial distress remains a pervasive feature of the U.S. economy. Medical debt is the leading cause of bankruptcy in the United States; an estimated 100 million individuals owe medical debt, with a total burden of at least $195 billion (AMA, 2024) [1]. This burden is not randomly distributed; it is concentrated among the same low-income and uninsured populations that Medicaid is designed to protect (Kluender et al., 2024) [12]. This juxtaposition of a nearly trillion-dollar financial protection program coexisting with around a $200 billion medical debt crisis frames the central policy question: How, and for whom, does this public insurance mechanism function?

To answer this question, I will use the 2008 Oregon Health Insurance Experiment (OHIE). In this experiment, Oregon used a lottery to select individuals from a waiting list, granting them the opportunity to apply for Medicaid. Because the lottery (the instrument, Z) was randomized, but actual enrollment (the treatment, W) was a matter of individual choice, the experiment features non-compliance. Compliance with the randomized offer was limited, with only about 30% of lottery-selected individuals in the analytical sample ultimately enrolling in the program.

Consequently, the seminal literature (Finkelstein et al., 2012 [8]; Baicker et al. 2013 [4]) uses the lottery as an instrument to identify a Local Average Treatment Effect (LATE). Their LATEs measure the average causal effect of Medicaid, but only for the complier subpopulation. The findings from this literature are a benchmark in health economics. Finkelstein et al. (2012) [8] and Baicker et al. (2013) [4] collectively establish that, for this complier population, Medicaid provides substantial financial protection. It significantly reduces out-of-pocket medical expenditures, the probability of having medical debt sent to collection, and, in Baicker et al. (2013) [4], the incidence of catastrophic medical expenditures (defined as out-of-pocket medical expenses exceeding 30% of income). Alongside this financial protection, Medicaid also increases health care utilization, including primary care and prescription drugs.

While the LATE estimated in this literature is a well-defined and statistically rigorous causal parameter, its economic interpretation for broader policy is limited. It is a single average effect for compliers, even though this group is likely to be highly heterogeneous in baseline health, income, and demographic characteristics that can systematically affect both the health and financial value of insurance. This critique has clear precedent. In the RAND Health Insurance Experiment, the headline result was that increased cost sharing had negligible effects on average health outcomes, but subgroup analyses showed clinically meaningful health gains for the sickest and poorest participants under lower cost-sharing plans (Brook et al., 2006) [5].

In practice, the OHIE LATE is a reduced-form object that bundles together at least two conceptually distinct microeconomic mechanisms. For enrollees who are not primarily liquidity-constrained but are risk-averse, Medicaid delivers consumption smoothing akin to the variance-reducing role of a Rothschild-Stiglitz (1976) [15] insurance contract: it compresses the dispersion of out-of-pocket spending across health states, from the beneficiary's perspective without an ex-ante premium, thereby replicating the state-contingent transfers that private insurance would provide, albeit through a publicly financed program rather than a market-priced contract.

On the other hand, for liquidity-constrained enrollees, Medicaid functions along the margin emphasized by Nyman (2003) [14]: an illness-contingent income transfer that generates access value by relaxing short-run liquidity constraints and enabling discrete purchases of high-value care that would otherwise be unaffordable, rather than simply reducing the variance of consumption across health states. For example, Baicker et al. (2013) [4] identify a LATE of Medicaid coverage on outcomes for compliers. This LATE is a reduced-form object with respect to underlying mechanisms: it embeds, without separating, the joint impact of risk smoothing and illness-contingent income transfers. This reduced-form estimate alone does not fully address questions related to targeting: optimal design and welfare evaluation hinge on identifying which mechanism drives marginal gains, and for which subpopulations.

Going beyond the scalar LATE, this paper characterizes how Medicaid's financial protection varies across individuals by tracing that variation to its two primary economic coordinates: ex-ante health risk (proxied by baseline conditions and utilization) and financial liquidity constraints (proxied by income and debt). These variables proxy for risk exposure, expected spending, and liquidity constraints, delivering an interpretable profile of the subpopulations that gain most from Medicaid coverage.

The seminal experimental work (Finkelstein et al. 2012 [8]; Baicker et al. 2013 [4]) established benchmark intent-to-treat and local average treatment effects, documenting substantial average reductions in financial strain for the complier subpopulation, but beyond a limited set of pre-specified subgroups did not systematically characterize heterogeneity. Subsequent causal machine learning work has begun to explore heterogeneous effects in OHIE. Hattab et al. (2024) [10], using causal and instrumental forests across a broad set of outcomes, find quite weak evidence of heterogeneity for the outcomes they consider, whereas Goto et al. (2024) [9], applying causal forests to the depression outcome, report sizable and predictable heterogeneous effects along that mental-health margin. Although Hattab et al. (2024) [10] also provide IV-forest estimates in their supplemental appendices, they do not use them to test explicit economic mechanisms.

The way Hattab et al. (2024) [10] and Goto et al. (2024) [9] employ their machine learning approaches has two major limitations that this analysis is designed to address. First, existing causal machine learning applications to the OHIE are primarily framed as data-driven detection exercises of heterogeneity or as demonstrations of econometric methodology, rather than as tests of explicit, theory-driven economic hypotheses about how and for whom Medicaid should matter. Second, they have either centered their main analysis on heterogeneity in the intent-to-treat (ITT) effect of lottery assignment, relegating coverage effects to supplementary IV analyses (Hattab et al., 2024 [10]), or have estimated heterogeneous effects of Medicaid coverage using the lottery as an instrument without foregrounding the local, complier-specific nature of the IV estimand (Goto et al., 2024 [9]). Yet the OHIE is an encouragement design with roughly 30% take-up under this paper's analytical sample, and for questions about Medicaid's financial protection, the policy-relevant object is a local average effect of coverage for compliers rather than the effect of the lottery offer itself.

This paper's contribution is to provide a theory-driven, econometrically appropriate deconstruction of the OHIE LATE for Medicaid's financial protection effects. Building on this decomposition of the OHIE LATE into a Rothschild-Stiglitz risk-smoothing channel and a Nyman-style access-value channel, the theory side of the paper uses these two mechanisms to discipline the heterogeneity analysis. This paper treats ex-ante proxies for health risk (baseline conditions, utilization, and self-reported health) and for liquidity constraints (income, debt, and related financial indicators) as the economically relevant coordinates along which financial protection effects should vary, and views heterogeneity along other dimensions as secondary.

On the econometric side, this paper implements instrumental forests, the generalized random forest variant of Athey et al. (2019) [3] designed to estimate conditional local average treatment effects (C-LATEs) in an IV setting. This distinction matters in the OHIE context: a causal forest recovers the conditional effect of the randomized lottery offer, whereas an instrumental forest recovers the conditional effect of Medicaid coverage for compliers, and the two coincide under (near) perfect compliance, which is violated by the roughly 30% take-up rate in this paper's OHIE sample. By combining this two-channel economic structure with an estimator aligned with the experiment's encouragement design, the paper nonparametrically characterizes the distribution of Medicaid's financial protection effects across observably different complier types.

1 Microeconomic Framework

This section develops the formal microeconomic structure that disciplines the subsequent econometric analysis. As discussed above, prior OHIE papers estimate ITT and LATEs of Medicaid coverage on a range of outcomes. The ITT summarizes the average effect of being offered coverage for the lottery-eligible population, and the LATE, under standard instrumental variables assumptions, summarizes the average effect of obtaining coverage for compliers whose enrollment status is changed by the lottery. By construction, however, neither object decomposes the contribution of distinct microeconomic channels such as risk-smoothing versus liquidity-driven access to care. For this paper's purposes, this scalar object is policy-incomplete, because it conflates at least two conceptually distinct channels through which Medicaid can generate value: (1) a Rothschild-Stiglitz (R-S) consumption-smoothing channel, which provides variance reduction for risk-averse individuals, and (2) a Nyman access-value channel, which provides an illness-contingent income transfer that relaxes liquidity constraints for financially fragile individuals.

To deconstruct the LATE and move toward a policy-relevant welfare evaluation, this section constructs a stylized two-period, two-state model. The model's objective is to formalize these two channels and derive a set of testable hypotheses regarding $\tau(x)$, the heterogeneous financial value of Medicaid, as a function of an individual's observable ex-ante characteristics x. Specifically, I focus on the two economic coordinates identified in the analysis: ex-ante health risk and financial liquidity.

1.1 Model Setup: Agents, States, and Constraints

I model a population of agents indexed by i, each living in a two-period, two-state environment.

Time and states. Time is discrete with $t \in \{0, 1\}$. Period $t = 0$ is the ex-ante period, when the opportunity to enroll in Medicaid is determined. Period $t = 1$ is the ex-post period, when health states are realized and consumption occurs. In period $t = 1$, agents realize one of two health states $s \in \{H, S\}$, where H denotes *Healthy* and S denotes *Sick*.

Heterogeneity. Agents are heterogeneous along two observable ex-ante dimensions, which constitute the vector of covariates

$$X_i = (p_i, L_i).$$

Health risk p_i is defined as the individual-specific probability of realizing the sick state,

$$p_i = \Pr(s = S),$$

and corresponds in the data to proxies such as baseline health conditions, prior utilization, or self-reported health. *Financial liquidity* L_i denotes the agent's available liquid financial resources backing period-1 consumption (for example, wealth carried from $t = 0$ or access to short-term credit). In the data, this corresponds to proxies such as income, existing debt, or other financial indicators.

Endowments and medical costs. Each agent i receives non-stochastic income Y_{1i} at the start of period-1, before the health state is realized. Without loss of generality, I normalize $Y_0 = 0$ and let L_i summarize all additional liquid resources that can be used to finance period-1 consumption (assets carried from $t = 0$, access to short-term credit, etc.). If an agent becomes sick ($s = S$), she requires a discrete high-value medical treatment m_S that can be obtained only by paying a monetary cost M. This captures the discrete purchases of high-value care central to the Nyman channel. For tractability, I assume a single representative cost M; in practice, medical costs are heterogeneous and the threshold is stochastic. I also assume that M is large relative to period-1 income, specifically $M > Y_{1i}$, so that current income alone cannot finance the treatment and affordability in the absence of insurance depends on the agent's total resources $Y_{1i} + L_i$.

Preferences. Agents have von Neumann-Morgenstern preferences and maximize expected utility over non-medical consumption c and medical treatment. Period-1 utility in health state $s \in \{H, S\}$ is given by

$$U_s(c_{1s}, m_s) = u(c_{1s}) + v_s(m_s).$$

The function $u(\cdot)$ is utility from non-medical consumption, assumed strictly increasing and strictly concave ($u' > 0, u'' < 0$); concavity captures risk aversion over consumption. The function $v_s(\cdot)$ is utility from medical care, where $m_s = 1$ if care is received and $m_s = 0$ otherwise. I assume $v_H(1) = v_H(0)$ and $v_S(1) > v_S(0)$. The model focuses on the discrete extensive margin (forgo vs. receive care) to isolate the access-value channel; intensive-margin changes in the quantity of care are absorbed into $v_s(m_s)$.

Insurance contract (Medicaid). Medicaid is the treatment, denoted by $W \in \{0, 1\}$. From the beneficiary's perspective, Medicaid fully covers the cost M of treatment m, so the marginal price at the point of use is normalized to zero:

- **Uninsured ($W = 0$).** The agent must pay the full cost M out of pocket if she becomes sick and chooses to consume the treatment.
- **Insured ($W = 1$).** Medicaid pays the full cost M; the agent faces a price of zero for care.

1.2 The Unconstrained Agent: The Rothschild-Stiglitz (R-S) Channel

I first analyze a standard agent with perfect liquidity, as in canonical insurance models. This agent is defined as having sufficient financial liquidity to afford the medical shock without forgoing care.

- **Definition (Unconstrained agent).** An agent i is unconstrained if

  $$L_i \geq M - Y_{1i}.$$

  Their total available resources $Y_{1i} + L_i$ are sufficient to cover the medical cost M and still have non-negative consumption. For this agent, the decision to consume $m_S = 1$ in the sick state is always optimal (given $v_S(1) > v_S(0)$ is sufficiently large). The insurance effect is therefore purely financial.

- **Budget constraints (unconstrained).**

  – *Uninsured ($W = 0$).*

  $$\begin{aligned} s = H: \quad c_{1H}^{W=0} &= Y_{1i} + L_i, \\ U_H^{W=0} &= u(Y_{1i} + L_i) + v_H(0), \\ s = S: \quad c_{1S}^{W=0} &= Y_{1i} + L_i - M, \\ U_S^{W=0} &= u(Y_{1i} + L_i - M) + v_S(1). \end{aligned}$$

  – *Insured ($W = 1$).*

  $$\begin{aligned} s = H: \quad c_{1H}^{W=1} &= Y_{1i} + L_i, \\ U_H^{W=1} &= u(Y_{1i} + L_i) + v_H(0), \\ s = S: \quad c_{1S}^{W=1} &= Y_{1i} + L_i, \\ U_S^{W=1} &= u(Y_{1i} + L_i) + v_S(1). \end{aligned}$$

- **Ex-ante expected utility (unconstrained).**

– *Uninsured ($W = 0$).*

$$
\begin{aligned}
E^{W=0} &= (1 - p_i)\big[u(Y_{1i} + L_i) + v_H(0)\big] \\
&\quad + p_i\big[u(Y_{1i} + L_i - M) + v_S(1)\big].
\end{aligned}
$$

– *Insured ($W = 1$).*

$$
\begin{aligned}
E^{W=1} &= (1 - p_i)\big[u(Y_{1i} + L_i) + v_H(0)\big] \\
&\quad + p_i\big[u(Y_{1i} + L_i) + v_S(1)\big].
\end{aligned}
$$

- **The value of Medicaid (R-S channel).** The financial value of Medicaid for this unconstrained agent is

$$
\begin{aligned}
\tau_{R\text{-}S}(p_i, L_i) &= E^{W=1} - E^{W=0} \\
&= p_i\big[u(Y_{1i} + L_i) - u(Y_{1i} + L_i - M)\big].
\end{aligned}
$$

This is the formalization of the Rothschild-Stiglitz channel. The value $\tau_{R\text{-}S}(p_i, L_i)$ is positive if and only if $p_i > 0$ and $u(\cdot)$ is strictly increasing. With concave u, this gain can be interpreted as the value of improved consumption insurance across health states: Medicaid eliminates the low-consumption state $Y_{1i} + L_i - M$ and compresses the dispersion of out-of-pocket spending, thereby smoothing marginal utility across states of the world. In this partial-equilibrium, beneficiary-only perspective, I abstract from the financing side and focus on this risk-smoothing component of the gain.

1.3 The Constrained Agent: The Nyman Access-Value Channel

I now analyze a hand-to-mouth (HTM) or liquidity-constrained agent.

- **Definition (Constrained agent).** An agent i is constrained if

$$
L_i < M - Y_{1i}.
$$

Their total available resources $Y_{1i} + L_i$ are insufficient to cover the medical cost M. For this agent, the discrete purchase of care is unaffordable in the absence of insurance. This is the key to formalizing Nyman's "access motive."

- **Budget constraints (constrained).**

– *Uninsured ($W = 0$).*

$$
\begin{aligned}
s = H: \quad & c_{1H}^{W=0} = Y_{1i} + L_i, \\
& U_H^{W=0} = u(Y_{1i} + L_i) + v_H(0), \\
s = S: \quad & \text{binding liquidity constraint: } Y_{1i} + L_i < M, \\
& m_S = 0, \quad c_{1S}^{W=0} = Y_{1i} + L_i, \\
& U_S^{W=0} = u(Y_{1i} + L_i) + v_S(0).
\end{aligned}
$$

– *Insured ($W = 1$).*

$$\begin{aligned} s = H: \quad & c_{1H}^{W=1} = Y_{1i} + L_i, \\ & U_H^{W=1} = u(Y_{1i} + L_i) + v_H(0), \\ s = S: \quad & \text{Medicaid pays } M, \text{ constraint relaxed, } m_S = 1, \\ & c_{1S}^{W=1} = Y_{1i} + L_i, \\ & U_S^{W=1} = u(Y_{1i} + L_i) + v_S(1). \end{aligned}$$

- **Ex-ante expected utility (constrained).**

  – *Uninsured ($W = 0$).*

$$\begin{aligned} E^{W=0} = {} & (1 - p_i)\big[u(Y_{1i} + L_i) + v_H(0)\big] \\ & + p_i\big[u(Y_{1i} + L_i) + v_S(0)\big]. \end{aligned}$$

  – *Insured ($W = 1$).*

$$\begin{aligned} E^{W=1} = {} & (1 - p_i)\big[u(Y_{1i} + L_i) + v_H(0)\big] \\ & + p_i\big[u(Y_{1i} + L_i) + v_S(1)\big]. \end{aligned}$$

- **The value of Medicaid (Nyman channel).** The value of Medicaid for the constrained (HTM) agent is

$$\begin{aligned} \tau_{\text{NYMAN}}(p_i, L_i) &= E^{W=1} - E^{W=0} \\ &= p_i\big[v_S(1) - v_S(0)\big]. \end{aligned}$$

  This is the formalization of Nyman's access-value channel, which is primarily about access value for constrained agents. Medicaid acts as an illness-contingent income transfer that relaxes the liquidity constraint $Y_{1i} + L_i < M$ and unlocks the utility gain $v_S(1) - v_S(0)$ from accessing high-value care. This liquidity effect is distinct from a standard income effect and is the source of Nyman's "efficient moral hazard."

1.4 Unifying the Model and Derivation of Testable Hypotheses

I can now write the total value of Medicaid, $\tau(x) = \tau(p_i, L_i)$, as a single function that maps an individual's ex-ante risk–liquidity coordinates $X_i = (p_i, L_i)$ into their heterogeneous treatment effect. Let

$$L_i^* = M - Y_{1i}$$

denote the liquidity threshold. The value of Medicaid is then

$$\begin{aligned} \tau(p_i, L_i) = {} & \mathbf{1}\{L_i < L_i^*\}\, p_i\big[v_S(1) - v_S(0)\big] \\ & + \mathbf{1}\{L_i \geq L_i^*\}\, p_i\Big[u(Y_{1i} + L_i) - u(Y_{1i} + L_i - M)\Big]. \end{aligned}$$

This unified expression formalizes the central argument: the value of Medicaid is driven by two distinct mechanisms, and the dominant mechanism switches with the agent's position in the liquidity distribution:

- For $L_i < L_i^*$, the agent is on the *Nyman margin*: Medicaid primarily relaxes a binding liquidity constraint and unlocks access to high-value care.
- For $L_i \geq L_i^*$, the agent is on the *Rothschild-Stiglitz margin*: Medicaid primarily provides risk-smoothing value by reducing consumption risk across health states.

From this model, I derive three testable hypotheses about the function $\tau(x)$, which the econometric framework is designed to estimate and test.

1.4.i Hypothesis 1 (Rothschild-Stiglitz Channel)

Prediction. The value of Medicaid is strictly increasing in ex-ante health risk p_i:

$$\frac{\partial \tau(p_i, L_i)}{\partial p_i} > 0.$$

This follows directly from the unified model and the earlier assumptions. For constrained agents ($L_i < L_i^*$),

$$\frac{\partial \tau(p_i, L_i)}{\partial p_i} = v_S(1) - v_S(0) > 0,$$

and for unconstrained agents ($L_i \geq L_i^*$),

$$\frac{\partial \tau(p_i, L_i)}{\partial p_i} = u(Y_{1i} + L_i) - u(Y_{1i} + L_i - M) > 0,$$

since $u(\cdot)$ is strictly increasing and $M > 0$. This is the standard insurance result: individuals who are more likely to get sick place a higher value on insurance.

1.4.ii Hypothesis 2 (Nyman Channel)

Prediction. For any $p_i > 0$, the value of Medicaid is flat in financial liquidity for constrained agents and strictly decreasing in financial liquidity for unconstrained agents. Equivalently, for all $L_i \neq L_i^*$,

$$\frac{\partial \tau(p_i, L_i)}{\partial L_i} \leq 0,$$

with equality when $L_i < L_i^*$ and strict inequality when $L_i > L_i^*$.

Again, this follows from the unified model:

- If $L_i < L_i^*$ (constrained region),

  $$\frac{\partial \tau(p_i, L_i)}{\partial L_i} = 0.$$

  In this region, the value is purely an access value: without coverage, the agent cannot purchase treatment at all, so small changes in liquidity do not affect the gain from coverage.

- If $L_i > L_i^*$ (unconstrained region),

$$\frac{\partial \tau(p_i, L_i)}{\partial L_i} = p_i\Big(u'(Y_{1i} + L_i) - u'(Y_{1i} + L_i - M)\Big).$$

  Under strict concavity, $u'' < 0$, marginal utility is decreasing in consumption. Because $Y_{1i} + L_i > Y_{1i} + L_i - M$, we have

$$u'(Y_{1i} + L_i) < u'(Y_{1i} + L_i - M),$$

  so, for any $p_i > 0$,

$$\frac{\partial \tau(p_i, L_i)}{\partial L_i} < 0.$$

Thus the value of Medicaid is flat in the constrained region and strictly decreasing in the unconstrained region. This implies that treatment effects will be larger among more liquidity-constrained agents.

1.4.iii Hypothesis 3 (The Key Interaction: R-S vs. Nyman)

Prediction. The marginal value of health risk p_i is larger at lower levels of liquidity L_i. Equivalently, the increase in the value of Medicaid associated with higher ex-ante risk is attenuated as liquidity rises:

$$\frac{\partial^2 \tau(p_i, L_i)}{\partial p_i \, \partial L_i} \le 0.$$

Let

$$S(L_i) \equiv \frac{\partial \tau(p_i, L_i)}{\partial p_i}$$

denote the slope of τ with respect to health risk. From the unified model,

- If $L_i < L_i^*$ (constrained / Nyman region),

$$S(L_i) = v_S(1) - v_S(0),$$

  so

$$\frac{\partial S(L_i)}{\partial L_i} = 0.$$

- If $L_i > L_i^*$ (unconstrained / R-S region),

$$S(L_i) = u(Y_{1i} + L_i) - u(Y_{1i} + L_i - M),$$

  so

$$\frac{\partial S(L_i)}{\partial L_i} = u'(Y_{1i} + L_i) - u'(Y_{1i} + L_i - M).$$

  Under strict concavity ($u'' < 0$), marginal utility is decreasing in consumption, implying

$$\frac{\partial S(L_i)}{\partial L_i} < 0.$$

Thus, for all $L_i \neq L_i^*$, the cross-partial

$$\frac{\partial^2 \tau(p_i, L_i)}{\partial p_i \, \partial L_i} = \frac{\partial S(L_i)}{\partial L_i}$$

is zero when agents are liquidity-constrained and strictly negative once they become unconstrained; overall it is weakly negative.

For high-value treatments, it is also reasonable to assume that the utility gain from gaining access to care, $v_S(1) - v_S(0)$, far exceeds the utility gain from merely insuring the financial cost, $u(Y_{1i} + L_i) - u(Y_{1i} + L_i - M)$. Under this additional assumption, the slope $S(L_i)$ exhibits a discrete downward jump at the individual-specific liquidity threshold L_i^*, where agents transition from the Nyman access region to the Rothschild-Stiglitz smoothing region.

Taken together, these properties imply that the risk gradient in the value of Medicaid is steepest at low levels of liquidity: being at high ex-ante health risk is especially valuable when liquid resources are scarce. This interaction (high risk combined with low liquidity generating disproportionately large gains from coverage) is the key empirical signature that distinguishes the Nyman access-value channel from the Rothschild-Stiglitz smoothing channel. Empirically, it motivates testing for especially large treatment effects among individuals who are both high-risk and liquidity-constrained, a distinctive prediction of the two-channel framework.

2 Econometric Framework

This section constructs the econometric framework required to identify and estimate the heterogeneous treatment effect function $\tau(x)$ that corresponds to the theoretical value of Medicaid $\tau(p_i, L_i)$ derived above, where x denotes the vector of observable covariates. The OHIE is an encouragement design with significant noncompliance: the randomized lottery offer serves as an instrument, while actual Medicaid enrollment is the endogenous treatment. Because the object of interest is the complier-specific causal effect of coverage, rather than the reduced-form effect of the lottery offer itself, this structure naturally calls for an instrumental variables (IV) approach.

I first define the causal parameters of interest using the potential outcomes framework. I then argue that the Conditional Local Average Treatment Effect (C-LATE), denoted by $\tau(x)$, is the appropriate empirical counterpart to the theoretical value derived in the microeconomic model. Consequently, the instrumental forest estimator of Athey et al. (2019) [3] , within the generalized random forest framework, is the natural econometric tool for estimating $\tau(x)$.

2.1 The Causal Model: Potential Outcomes and Noncompliance

I adopt the potential outcomes framework (Rubin, 1974, 1978) [16] [17], as formalized for IV by Imbens and Angrist (1994) [11]. For each agent i in the OHIE sample:

- $Z_i \in \{0, 1\}$ is the instrument: $Z_i = 1$ if agent i was selected in the lottery and $Z_i = 0$ otherwise. By design, Z_i is randomly assigned.

- $W_i \in \{0, 1\}$ is the treatment: $W_i = 1$ if agent i enrolled in Medicaid and $W_i = 0$ otherwise. Because enrollment is a choice among those offered coverage (i.e., there is noncompliance with the lottery assignment), W_i is not randomly assigned and is treated as an endogenous treatment variable determined by Z_i and agent characteristics.

- Y_i is the observed financial outcome of interest.

- X_i is a vector of k pre-randomization covariates, including empirical proxies for the latent primitives p_i (health risk) and L_i (liquidity) introduced in the microeconomic framework.

2.1.i Potential Outcomes and Principal Strata

To handle noncompliance, I define two sets of potential outcomes for each agent i.

Potential treatment status. The potential treatment status $W_i(z)$ denotes the Medicaid enrollment status that agent i would exhibit if the lottery assignment were $Z_i = z$, for $z \in \{0, 1\}$:

$$W_i(1) \quad \text{is the enrollment status if } i \text{ is selected in the lottery (offered coverage),}$$

$$W_i(0) \quad \text{is the enrollment status if } i \text{ is not selected in the lottery.}$$

The observed treatment is

$$W_i = Z_i W_i(1) + (1 - Z_i) W_i(0).$$

Potential outcomes. The potential outcome $Y_i(w)$ denotes the financial outcome that agent i would exhibit if their coverage status were $W_i = w$, for $w \in \{0, 1\}$:

$$Y_i(1) \quad \text{is the outcome if } i \text{ is enrolled in Medicaid,}$$

$$Y_i(0) \quad \text{is the outcome if } i \text{ is not enrolled in Medicaid.}$$

The observed outcome is

$$Y_i = W_i Y_i(1) + (1 - W_i) Y_i(0).$$

This framework partitions the population into four latent principal strata, based on the pair $(W_i(0), W_i(1))$:

- **Compliers (c):** $(W_i(0), W_i(1)) = (0, 1)$. These agents enroll in Medicaid if and only if they are selected in the lottery. Under the monotonicity assumption (no defiers), compliers are the only principal stratum for whom the instrument Z_i causally changes the treatment status W_i.

- **Never-takers (n):** $(W_i(0), W_i(1)) = (0, 0)$. These agents do not enroll in Medicaid, regardless of the lottery outcome. In the OHIE context, a substantial share of individuals offered coverage behave as never-takers with respect to the experimental Medicaid program.

- **Always-takers (a):** $(W_i(0), W_i(1)) = (1, 1)$. These agents enroll in Medicaid regardless of the lottery outcome (for example, because they qualify through other eligibility pathways such as disability or pregnancy). In the specific OHIE setting, this group is expected to be small, but the stratum is included for conceptual completeness.
- **Defiers (d):** $(W_i(0), W_i(1)) = (1, 0)$. These agents enroll only if they were not selected in the lottery. The standard LATE analysis of OHIE imposes the monotonicity assumption, which rules out defiers, so this stratum is assumed to be empty in what follows.

2.1.ii Identification Assumptions

To identify a causal effect in this setting, I make the standard LATE assumptions.

Independence (random assignment). Let S_i denote the pre-randomization strata (defined by household size). In the OHIE, the lottery assignment Z_i was randomized at the household level with the probability of selection depending on S_i by design. Conditional on S_i, the instrument is therefore independent of potential outcomes and potential treatment statuses:

$$(Y_i(1), Y_i(0), W_i(1), W_i(0), X_i) \perp\!\!\!\perp Z_i \,\big|\, S_i.$$

In estimation, I additionally condition on pre-randomization covariates X_i to improve precision (and mitigate chance covariate imbalance in finite samples), which is consistent with

$$(Y_i(1), Y_i(0), W_i(1), W_i(0)) \perp\!\!\!\perp Z_i \,\big|\, S_i, X_i.$$

Exclusion restriction. The lottery affects the financial outcome only through its effect on Medicaid enrollment:

$$Y_i(z, w) = Y_i(w) \quad \text{for all } z, w \in \{0, 1\}.$$

The offer itself is assumed not to directly change financial outcomes except insofar as it induces enrollment.

Monotonicity (no defiers). For all i,

$$W_i(1) \geq W_i(0).$$

Winning the lottery does not cause anyone to forgo enrollment who would have enrolled in its absence. This rules out the defier stratum and is highly plausible in the OHIE context.

First-stage relevance. For the subpopulation in which I seek to estimate the heterogeneous treatment effect $\tau(x)$, the lottery assignment must have a strictly positive effect on Medicaid enrollment. At the conditional level,

$$E[W_i \mid Z_i = 1, S_i = s, X_i = x] \; > \; E[W_i \mid Z_i = 0, S_i = s, X_i = x]$$

for all (s, x) in the region of interest. At a global level, this implies

$$E\big[W_i(1) - W_i(0)\big] > 0,$$

i.e., the lottery induces enrollment for at least some individuals.

SUTVA (household-level Stable Unit Treatment Value Assumption). Let h index households and i index agents within households. Let $Z_{\mathcal{A}}$ denote the vector of lottery assignments for all households and Z_h the assignment for household h. I assume that the potential outcomes of agent i in household h depend only on their own household's assignment:

$$Y_{hi}(Z_{\mathcal{A}}) = Y_{hi}(Z_h), \qquad W_{hi}(Z_{\mathcal{A}}) = W_{hi}(Z_h),$$

and not on the assignment of other households. That is, there is no cross-household interference (no general equilibrium congestion or spillover effects across households). Because assignment is at the household level, I impose SUTVA at the household level: each individual's potential outcomes may depend on the household's assignment (and, implicitly, on within-household coverage decisions), but not on other households' assignments. For expositional simplicity, I suppress this within-household dependence in the notation and treat outcomes as functions of the household's assignment.

2.2 The LATE and C-LATE Estimands

Under the assumptions in Section 2.1, I can now define the causal parameters of interest.

The scalar LATE. Following Imbens and Angrist (1994) [11] and the seminal OHIE analyses (Finkelstein et al. 2012 [8]; Baicker et al. 2013 [4]), the Local Average Treatment Effect (LATE) is defined as the average causal effect of Medicaid coverage for the complier subpopulation:

$$\tau_{\text{LATE}} = E\big[Y_i(1) - Y_i(0) \,\big|\, i \in C\big],$$

where $C = \{i : (W_i(0), W_i(1)) = (0, 1)\}$ denotes the set of compliers. Under independence, exclusion, monotonicity, SUTVA, and a non-zero first-stage, this parameter is identified by the Wald ratio of the Intent-to-Treat (ITT) effect of the instrument on the outcome to the ITT effect of the instrument on treatment:

$$\tau_{\text{LATE}} = \frac{\text{ITT}_Y}{\text{ITT}_W} = \frac{E[Y_i \mid Z_i = 1] - E[Y_i \mid Z_i = 0]}{E[W_i \mid Z_i = 1] - E[W_i \mid Z_i = 0]}.$$

As discussed above, this scalar LATE is a causally well-defined reduced-form parameter, but it is "policy-incomplete" for my purposes: it is a single average over a heterogeneous complier population and therefore bundles together the Rothschild-Stiglitz and Nyman mechanisms into one number.

The Conditional LATE (C-LATE): the parameter of interest. To test the microeconomic hypotheses (H1–H3) derived in Section 1.4, I require the LATE conditional on pre-randomization covariates $X_i = x$. This is the Conditional Local Average Treatment Effect (C-LATE):

$$\tau(x) = E\big[Y_i(1) - Y_i(0) \,\big|\, i \in C,\, X_i = x\big].$$

The function $\tau(x)$ is the empirical counterpart of the theoretical value of Medicaid $\tau(p_i, L_i)$ derived in the microeconomic framework, with components of x serving as proxies for health risk p_i and liquidity L_i. Because in the main application Y_i is an indicator for having any medical debt (a "bad" outcome), more negative values of $\tau(x)$ correspond to higher welfare value from coverage in terms of financial protection. This mapping is most direct when x contains rich proxies for p_i and L_i; otherwise, $\tau(x)$ represents the (possibly nonparametric) projection of the theoretical value onto the observable covariate space. The hypotheses in Section 1.4 are statements about the shape of $\tau(x)$ with respect to these coordinates.

Under the same IV assumptions, $\tau(x)$ is identified by the conditional Wald ratio:

$$\tau(x) = \frac{E[Y_i \mid Z_i = 1, X_i = x] - E[Y_i \mid Z_i = 0, X_i = x]}{E[W_i \mid Z_i = 1, X_i = x] - E[W_i \mid Z_i = 0, X_i = x]} = \frac{\text{C-ITT}_Y(x)}{\text{C-ITT}_W(x)},$$

where $\text{C-ITT}_Y(x)$ denotes the conditional intent-to-treat effect of the lottery assignment Z on the outcome Y for agents with covariate profile $X_i = x$, and $\text{C-ITT}_W(x)$ denotes the conditional intent-to-treat effect of the lottery assignment Z on Medicaid enrollment W for agents with covariate profile $X_i = x$. Throughout, I treat the lottery strata indicators S_i as included in X_i (or, equivalently, condition on S_i in all conditional moments); I suppress S_i in the notation for brevity.

In other words, $\tau(x)$ is the complier-specific effect of Medicaid coverage for individuals with covariate profile $X_i = x$, constructed as the ratio of the conditional ITT of the lottery on the outcome to the conditional ITT of the lottery on enrollment. The econometric task in the next subsection is to estimate this function nonparametrically using instrumental forests.

2.3 The Instrumental Forest (IV-Forest) Estimator

Estimating the function $\tau(x)$ nonparametrically from the data requires a method that can (i) handle the IV structure and (ii) flexibly uncover heterogeneity along high-dimensional covariates X. This is precisely what the instrumental forest estimator is designed for.

2.3.i The Problem: C-ITT vs. C-LATE

It is critical to distinguish the C-LATE from the parameter estimated by a standard causal forest (Wager & Athey, 2018 [18]).

Causal forest (CF). A standard causal forest is designed to estimate the Conditional Average Treatment Effect (CATE) of a treatment W on an outcome Y:

$$\tau_{\text{CATE}}(x) = E\big[Y_i(1) - Y_i(0) \,\big|\, X_i = x\big].$$

This estimand is identified under the assumption that, conditional on covariates X_i, assignment to the treatment W is as good as random (unconfounded). In the OHIE context, one could in principle apply a standard CF in two ways, neither of which recovers the parameter of interest $\tau(x)$:

- *Estimating the CATE of W (biased).* A standard CF applied with Medicaid coverage W as the treatment does not, in general, identify the causal effect of W on Y because W is endogenous. Even conditional on covariates X_i, the decision to enroll is plausibly related to unobservable determinants of the outcome, so the unconfoundedness condition is not credible in this setting. A CF applied to W_i would therefore typically recover a selection-biased association between W and Y, rather than the causal effect.

- *Estimating the CATE of Z (identified, but wrong parameter).* One can instead use a standard CF to estimate the effect of the randomized lottery offer Z on Y. Because Z is randomly assigned (conditional on strata S_i), this yields a valid causal parameter: the Conditional Intent-to-Treat (C-ITT) effect,

  $$\tau_{\text{ITT}}(x) = E[Y_i \mid Z_i = 1, X_i = x] - E[Y_i \mid Z_i = 0, X_i = x].$$

  However, this parameter is insufficient for testing the microeconomic theory. The C-ITT conflates the structural value of coverage $\tau(x)$ with the compliance rate,

  $$\text{C-ITT}_W(x) = E[W_i \mid Z_i = 1, X_i = x] - E[W_i \mid Z_i = 0, X_i = x] = E\big[W_i(1) - W_i(0) \,\big|\, X_i = x\big],$$

  via the relationship

  $$\tau_{\text{ITT}}(x) = \tau(x) \cdot \text{C-ITT}_W(x).$$

  If a low $|\tau_{\text{ITT}}(x)|$ is observed for a specific group (e.g., the healthy poor), we cannot tell from $\tau_{\text{ITT}}(x)$ alone whether Medicaid has little value for that group ($\tau(x) \approx 0$) or whether they simply do not comply with the offer (compliance $\text{C-ITT}_W(x) \approx 0$). To disentangle these mechanisms and recover $\tau(x)$, we require an estimator that directly targets the C-LATE, namely the instrumental forest.

2.3.ii The Solution: The Instrumental Forest (IV-Forest)

The instrumental forest (Athey et al., 2019) [3] is a member of the Generalized Random Forest (GRF) family that is explicitly designed to estimate heterogeneous treatment effects in an IV setting. In this application, its target is the C-LATE function $\tau(x)$ defined in Section 2.2.

Formally, the IV-forest estimates $\tau(x)$ by solving, for each covariate value x, a localized version of an orthogonalized IV moment condition. This approach uses a Neyman–orthogonal score, which reduces sensitivity to small errors in estimating nuisance functions (a principle also central to Double Machine Learning; see Chernozhukov et al., 2018) [7]. One convenient orthogonal moment condition can be written as

$$E\Big[(Z_i - m(X_i))\Big((Y_i - q(X_i)) - \tau(x)\big(W_i - e(X_i)\big)\Big) \,\Big|\, X_i = x\Big] = 0,$$

where $m(X_i) = E[Z_i \mid X_i]$, $e(X_i) = E[W_i \mid X_i]$, and $q(X_i) = E[Y_i \mid X_i]$ represent the conditional means of the instrument, treatment, and outcome, respectively.

When the instrument is binary, $Z_i \in \{0, 1\}$, the orthogonal moment condition above reduces to (and therefore identifies) the conditional Wald ratio under the IV assumptions in Section 2.1:

$$\tau(x) = \frac{E[Y_i \mid Z_i = 1, X_i = x] - E[Y_i \mid Z_i = 0, X_i = x]}{E[W_i \mid Z_i = 1, X_i = x] - E[W_i \mid Z_i = 0, X_i = x]}.$$

The GRF mechanism. In general, GRF methods search for solutions to conditional moment equations of the form

$$E\big[\Psi_{\theta(x),v(x)}(O_i) \,\big|\, X_i = x\big] = 0,$$

where $O_i = (Y_i, W_i, Z_i, X_i)$, $\theta(x)$ is the parameter of interest, and $v(x)$ collects the nuisance functions. For the IV-forest, $\theta(x) = \tau(x)$ and the score function is

$$\Psi_{\theta(x),v(x)}(O_i) = (Z_i - m(X_i))\Big((Y_i - q(X_i)) - \tau(x)(W_i - e(X_i))\Big),$$

which encodes the standard IV orthogonality condition with the instrument, the outcome, and the endogenous regressor residualized on X_i.

Trees are grown using a splitting rule that selects splits which are most informative for estimating the local IV parameter $\tau(x)$ (i.e., splits that improve the local fit of the orthogonal IV moment / reduce expected error), often operationalized via pseudo-outcomes. This splitting criterion is designed so that the forest adaptively partitions the covariate space into regions with different values of the structural parameter $\tau(x)$, thereby encouraging splits that reveal heterogeneity in the C-LATE.

Weighting and estimation. To estimate $\tau(x)$ at a specific target point x, the forest aggregates information from the ensemble of trees to generate adaptive weights. The algorithm assigns a weight $\alpha_i(x)$ to each training observation i, equal to the average (across trees) of the inverse leaf size for leaves where observation i and the target x co-occupy the same terminal node. The estimator $\hat{\tau}(x)$ is then obtained by solving the sample analogue of the weighted moment condition:

$$\sum_{i=1}^{n} \alpha_i(x)\, \Psi_{\hat{\tau}(x),\hat{v}(x)}(O_i) = 0.$$

Honest trees. A key ingredient for the statistical properties of GRF estimators is "honesty" in tree construction. Following Athey et al. (2019) [3], I implement "honest" tree construction: within each tree, the data used to choose splits are separated from the data used to form within-leaf estimates/weights (i.e., split selection and estimation are performed on disjoint subsamples). This ensures the tree structure is independent of the estimation sample's outcomes, which under regularity conditions supports valid asymptotic inference for $\hat{\tau}(x)$.

3 From Estimation to Policy: Optimal Targeting Under Noncompliance

The framework developed so far provides (i) a microeconomic model that explains why heterogeneity in $\tau(x)$ should arise (Section 1) and (ii) an econometric procedure to estimate the C-LATE function $\tau(x)$ (Section 2.3). This section connects those estimates to counterfactual policy design.

A central feature of the OHIE is noncompliance: the policymaker cannot directly assign Medicaid coverage W_i, but can only assign the opportunity to apply, Z_i (the lottery offer). As a result, a targeting rule for offers must depend not only on the welfare value of coverage among those induced to enroll (captured by the conditional LATE/C-LATE $\tau(x)$) but also on the likelihood of take-up if offered coverage, i.e. the conditional first stage. Following the logic in Athey et al. (2025) [2], I therefore frame optimal targeting in terms of the conditional intent-to-treat (ITT) effect of an offer and its decomposition into a "value" component and a "compliance" component. In the empirical implementation (and in Figure 4), I operationalize this decomposition by combining IV-forest estimates of $\tau(x)$ with an estimated conditional first stage $\pi(x)$ constructed from survey-weighted take-up differences within risk–liquidity quantile cells.

3.1 Policy Objective: Maximizing Welfare Gains from Lottery Offers

For each covariate profile x, define the conditional intent-to-treat effect of a lottery offer on outcome Y_i as

$$\tau_{\mathrm{ITT}}(x) = \mathbb{E}[Y_i \mid Z_i = 1, X_i = x] - \mathbb{E}[Y_i \mid Z_i = 0, X_i = x].$$

If Y_i is coded so that higher values correspond to better financial protection, then higher $\tau_{\mathrm{ITT}}(x)$ is better. In the empirical implementation below, I instead take Y_i to be an indicator for having any medical debt, so lower values correspond to improved financial protection; in that case the policy-relevant *gain* is

$$g(x) \equiv -\tau_{\mathrm{ITT}}(x),$$

so that larger $g(x)$ corresponds to larger expected debt reductions from an offer.

Consider a policymaker who faces a simple budget constraint: each lottery offer (setting $Z_i = 1$) has the same cost, and the policymaker can offer access to at most a fraction b of the eligible population. I model the budget constraint as limiting the share of offers, rather than realized expenditure on coverage. This reflects the institutional design of the OHIE, where administrative capacity and a fixed number of lottery slots constituted the binding constraints.

A targeting rule is a function $d(x) \in \{0, 1\}$ that assigns an offer ($Z_i = 1$) when $d(X_i) = 1$ and no offer when $d(X_i) = 0$. Under this setup, expected outcomes under a policy $d(\cdot)$ can be written as

$$\mathbb{E}[Y_i(d)] = \mathbb{E}[Y_i(0)] + \mathbb{E}\big[\tau_{\mathrm{ITT}}(X_i)\, d(X_i)\big],$$

where $\mathbb{E}[Y_i(0)]$ is the expected outcome under no lottery offers. When Y_i is an adverse outcome (as in the application), maximizing financial protection is equivalent to maximizing

$$\mathbb{E}\big[g(X_i)\, d(X_i)\big] \quad \text{subject to} \quad \mathbb{E}[d(X_i)] \le b$$

Under this constant-cost budget and binary decision rule, the optimal policy takes a threshold form: rank individuals by $g(x)$ (equivalently, by the most negative $\tau_{\mathrm{ITT}}(x)$) and offer the lottery to those with the highest gains until the budget is exhausted.

3.2 Decomposition: Value of Coverage x Compliance

The conditional ITT effect $\tau_{\mathrm{ITT}}(x)$ is the policy-relevant object for targeting lottery offers Z_i, but it combines (i) the value of coverage for those induced to enroll and (ii) the propensity to enroll when offered.

Let

$$\text{CFS}(x) \equiv \mathbb{E}[W_i \mid Z_i = 1, X_i = x] - \mathbb{E}[W_i \mid Z_i = 0, X_i = x]$$

denote the conditional first stage. Under the standard IV assumptions (random assignment of Z_i, exclusion, and monotonicity), the conditional ITT admits the LATE decomposition

$$\tau_{\text{ITT}}(x) = \text{CFS}(x) \cdot \tau(x),$$

where

$$\tau(x) \equiv \mathbb{E}[Y_i(1) - Y_i(0) \,|\, W_i(1) > W_i(0),\, X_i = x]$$

is the Conditional LATE (C-LATE), i.e., the effect of coverage among compliers with characteristics x.

In the empirical analysis, the IV forest delivers observation-level estimates $\hat{\tau}_i$ of this complier-specific effect. To form a policy-relevant predicted ITT for offers, I pair $\hat{\tau}_i$ with an estimate of the conditional first stage, yielding the observation-level targeting score

$$\widehat{\tau}_{\text{ITT},i} = \hat{\pi}_i \, \hat{\tau}_i,$$

where $\hat{\pi}_i$ is an estimate of $\text{CFS}(X_i)$.

This decomposition clarifies the targeting trade-off. A policy that targets only on $\tau(x)$ may overlook individuals with slightly smaller coverage value but substantially higher enrollment. Conversely, a policy that targets only on compliance may prioritize individuals who readily enroll but derive limited benefit from coverage. The optimal rule in this framework targets those with the largest product $\tau(x) \times \text{CFS}(x)$.

In practice, I estimate $\text{CFS}(x)$ using survey-weighted take-up differences within quantile cells of the baseline risk and liquidity indices. Specifically, for each risk–liquidity cell I compute weighted take-up rates by lottery status and difference them:

$$\hat{\pi}(\text{cell}) \equiv \overline{W}^{\,w}_{1,\text{cell}} - \overline{W}^{\,w}_{0,\text{cell}}, \qquad \overline{W}^{\,w}_{z,\text{cell}} \equiv \frac{\sum_{i:\, Z_i = z,\, i \in \text{cell}} \omega_i W_i}{\sum_{i:\, Z_i = z,\, i \in \text{cell}} \omega_i},$$

where ω_i is the survey weight. I then assign $\hat{\pi}_i = \hat{\pi}(\text{cell}(i))$, use the pooled (overall) survey-weighted first stage as a fallback when a cell-specific estimate is unavailable (e.g., when a cell contains only $Z_i = 0$ or only $Z_i = 1$ observations), and impose $\hat{\pi}_i \geq 0$ by clipping at zero. This yields the targeting score $\widehat{\tau}_{\text{ITT},i}$ used in Figure 4.

3.3 Empirical Strategy for Counterfactual Policy Simulation

To translate the decomposition into policy-relevant estimates, I implement a three-step procedure.

1. **Estimate the C-LATE function.** First, I estimate the conditional LATE function $\tau(x)$ using an instrumental forest, yielding observation-level complier-effect estimates $\hat{\tau}_i \approx \tau(X_i)$ for each outcome. In the empirical implementation, Y_i is an adverse indicator (any medical debt), so more negative values of $\hat{\tau}_i$ correspond to larger reductions in debt incidence among compliers.

2. **Estimate heterogeneous compliance (the conditional first stage).** Second, I estimate heterogeneity in compliance via the conditional first stage

$$\mathrm{CFS}(x) \equiv \mathbb{E}[W_i \mid Z_i = 1, X_i = x] - \mathbb{E}[W_i \mid Z_i = 0, X_i = x] .$$

   In practice, I approximate CFS(x) nonparametrically using survey-weighted take-up differences within quantile cells of the baseline risk and liquidity indices. For each risk–liquidity cell I compute

$$\hat{\pi}(\text{cell}) = \overline{W}^{w}_{1,\text{cell}} - \overline{W}^{w}_{0,\text{cell}},$$

   assign $\hat{\pi}_i = \hat{\pi}(\text{cell}(i))$, use the pooled (overall) survey-weighted first stage as a fallback when a cell-specific estimate is unavailable, and impose $\hat{\pi}_i \geq 0$ by clipping at zero.

3. **Construct targeting scores and simulate policies.** Third, I construct the observation-level predicted ITT score for offers,

$$\widehat{\tau}_{\text{ITT},i} = \hat{\pi}_i \, \hat{\tau}_i,$$

   and rank individuals by predicted ITT gain (in percentage points) $-100\,\widehat{\tau}_{\text{ITT},i}$. I summarize counterfactual targeting policies with a policy gain curve that traces the expected reduction in the population-average probability of any medical debt as the share of the population offered Medicaid increases, comparing targeting to a random-offer (homogeneous ITT) benchmark. This gain curve is an in-sample, descriptive simulation based on predicted effects.

This strategy aligns the estimated complier-specific coverage effects with a feasible targeting rule for lottery offers that accounts for both value heterogeneity and compliance.

4 Results

This paper uses data from the Oregon Health Insurance Experiment, accessed through the National Bureau of Economic Research (NBER) [1]

Code and replication materials are available publicly [2]

4.1 Sample Construction, Survey Response, and First Stage

I begin by describing the construction of the analysis sample and documenting survey response, attrition, and the strength of the randomized lottery as an instrument for Medicaid coverage. The experimental universe consists of 74,922 adults who signed up for the Oregon Health Plan Standard waiting list and are observed in the administrative descriptive file that records lottery assignment, household identifiers, and basic pre-lottery characteristics. I use this file as the sampling frame and merge in three additional sources: the baseline (0-month) survey, which records detailed pre-treatment health, utilization, and financial variables; the 12-month

[1] `https://www.nber.org/research/data/oregon-health-insurance-experiment-data`

[2] `https://github.com/Suzanna-Ayash/OHIE_Causal_ML/tree/main`

survey, which records financial outcomes; and administrative enrollment data that track Medicaid coverage through September 2009. All merges are done by `person_id` and validated as one-to-one, and I verify that there are no duplicate identifiers or missing instrument or treatment variables in the merged universe.

The 12-month survey was administered to a subset of the randomized universe. Among individuals sampled into this 12-month survey frame (58,405 adults), 41.5% of lottery losers and 39.9% of lottery winners returned the survey, a difference of -1.6 percentage points that is statistically significant with household-clustered standard errors ($p \approx 0.0003$). Although response is not identical by lottery status, attriters within the survey frame are well balanced on a range of pre-treatment characteristics: among non-respondents, standardized differences in a selection of baseline demographics, health, and financial variables between winners and losers are all below 0.06 in absolute value. Throughout the analysis of outcomes, I use the survey's nonresponse-adjusted 12-month weight (`weight_12m`). When applied to the sample, this weighting yields a design-adjusted response rate of 50%, compared with the unweighted response rate of 41%.

For the main C-LATE analysis, I further restrict attention to individuals who completed both the baseline (0-month) and 12-month surveys, so that all baseline covariates and financial outcomes are available (subject to item non-response discussed below). This yields an analysis sample of 16,579 adults (8,432 lottery losers and 8,147 lottery winners). Within this sample, the instrument and treatment are fully observed (no missing values in lottery assignment or Medicaid enrollment). I examine balance on a rich set of pre-treatment covariates (including birth year, sex, baseline self-reported physical and mental health, chronic conditions, household income, baseline medical debt and out-of-pocket spending, insurance history, and pre-lottery geographic indicators) and find that randomization achieves good balance: standardized differences between winners and losers are generally small, typically below 0.10 in absolute value. The only notable imbalance is in the pre-lottery variable `numhh_list` (the number of household members listed on the lottery sign-up card), which reflects the fact that households with more listed members had a higher probability of receiving at least one offer under the experimental design. All linear regressions cluster standard errors at the household level to account for this grouping.

As depicted in Table 1, the lottery generates a large and statistically precise first stage for Medicaid coverage in the analysis sample. Among lottery losers, 13.1% ever enroll in Medicaid by September 2009, reflecting a combination of alternative eligibility channels and always-takers. Among lottery winners, the enrollment rate rises to 44.9%. The difference of 31.8 percentage points represents a very strong first-stage effect of the lottery on coverage and, under the monotonicity assumption (no defiers), can be interpreted as the share of compliers in the sample. A simple linear probability model of Medicaid enrollment on lottery assignment, with standard errors clustered at the household level, yields a coefficient on the lottery indicator of 0.318 (standard error ≈ 0.007), corresponding to a cluster-robust F-statistic of about 2,022. When `numhh_list` is included as an additional control, the first stage is essentially unchanged: the coefficient on the lottery rises slightly to about 0.325 and the cluster-robust F-statistic remains above 2,100. These values are far above conventional weak-instrument thresholds and confirm that the lottery is an exceptionally strong instrument for Medicaid coverage in the population used to estimate the IV-forest and C-LATE functions.

Table 1: First Stage Effect of Lottery on Medicaid Enrollment

	(1)	(2)
Dependent Variable:	Enrolled in Medicaid	
Specification:	Unadjusted	**Design-Adjusted**
Lottery Win (Z)	0.318***	0.325***
	(0.007)	(0.007)
Household Size Control	No	Yes
Control Mean ($Z = 0$)	0.131	0.131
F-Statistic (Cluster)	2021.8	2140.9
Observations	16,579	16,579

Notes: Standard errors clustered at the household level in parentheses. Column (2) controls for `numhh_list` (household size on the lottery list), which is mechanically related to the probability that at least one household member receives an offer. *** p< 0.01.

4.2 Data Harmonization, Missingness, and Imputation

Prior to estimation, I harmonize variable definitions and address missing data through a two-stage process: (i) deterministic, logic-based corrections where the implied value is fully pinned down by internal consistency, and (ii) machine-learning–based imputation applied only to baseline covariates for residual item non-response.

First, I reconcile demographic inconsistencies by prioritizing baseline survey responses over administrative records and encoding race as a set of non–mutually exclusive indicators. I enforce deterministic consistency rules on financial variables: extensive-margin indicators are aligned with continuous totals, and missing components of out-of-pocket (OOP) spending are recovered residually whenever algebraic identities allow. Indicators for financial distress (such as borrowing to pay medical bills) are set to zero when reported medical debt and OOP spending are both zero (and the distress item is missing). Crucially, I do not statistically impute outcome variables; beyond these logically forced cases, remaining missing values in outcomes are left missing, and estimation for each outcome proceeds on the corresponding available subsample.

Second, I address residual item non-response in baseline covariates using an iterative Extra-Trees regressor ensemble. To preserve causal validity, this procedure excludes lottery assignment, treatment status, and all outcome variables from both the predictors and the imputation targets. For every imputed covariate, I generate a binary missingness flag that is included as an additional control to capture non-random missingness patterns. Post-imputation, variables are constrained to their natural support, and continuous financial covariates are winsorized at the 1st and 99th percentiles to mitigate the influence of outliers.

Finally, I construct key heterogeneity variables. 12-month household income is converted from categorical bins to numeric midpoints (in 2008 dollars). I define catastrophic health expenditure as a binary indicator equal to one if total OOP spending exceeds 30% of household income (treating income = 0 as catastrophic if $OOP > 0$). Respondent age is calculated from baseline birth year relative to the lottery year and used in place of year of birth in the covariate vector.

4.3 Baseline IV Results: Medical Debt and Financial Distress

Table 2 presents the primary results, summarizing the relationship between lottery assignment, Medicaid coverage, and the incidence of medical debt. I report both unweighted and weighted estimates to assess robustness to survey nonresponse, but focus the discussion on the weighted specifications, which incorporate the 12-month sampling weights.

Panel B shows that the lottery provides an exceptionally strong instrument for Medicaid coverage. In the analysis sample for the "any medical debt" outcome, being selected by the lottery increases the probability of Medicaid enrollment by 32.0 percentage points (s.e. 0.008). The corresponding cluster-robust first-stage F-statistic is 1,752, effectively ruling out weak-instrument concerns. The implied complier share is also about 32%, so the LATEs reported below apply to a quantitatively important segment of the low-income population whose coverage status is marginally affected by the lottery.

Panel A reports the reduced-form ITT effect of lottery assignment on medical debt. Winning the lottery reduces the probability of holding any medical debt at 12 months by 5.6 percentage points (s.e. 0.009) in the weighted specification. Dividing this ITT by the first-stage effect yields the Wald estimate in Panel C: among compliers, Medicaid coverage reduces the incidence of medical debt by 17.4 percentage points (s.e. 0.028). This effect is both statistically precise and economically large. In the control group, 58.1% of respondents have any medical debt at 12 months, so the estimated LATE corresponds to roughly a 30% reduction in the prevalence of medical debt relative to the counterfactual mean. For comparison, the instrumental-forest C-LATE estimates discussed below have a survey-weighted mean of about -12.3 percentage points; the difference from the Wald LATE reflects the fact that the forest adaptively reweights compliers across covariate profiles, placing relatively more weight on groups with somewhat smaller effects. Outcome-specific samples are slightly smaller than the full analysis sample in Table 1 due to specific outcome non-response in the 12-month medical debt outcome and the exclusion of zero-weight observations in the weighted specifications.

Although Table 2 focuses on the extensive margin of medical debt, additional outcomes (not shown here) indicate that Medicaid also delivers substantial relief on the intensive and liquidity margins. Coverage reduces the total stock of medical debt by approximately $1,261 (s.e. 375) among compliers, even after winsorizing extreme values at the 99th percentile. Consistent with the program relaxing short-run budget constraints, Medicaid coverage lowers the probability that households borrow or skip other bills to pay for healthcare by about 18.6 percentage points, nearly a 50% reduction relative to the control mean. Finally, coverage almost eliminates the risk of catastrophic out-of-pocket expenditure: the probability of spending more than 30% of income out of pocket falls by 7.6 percentage points from a baseline of roughly 8%.

Table 2: Lottery, Medicaid Enrollment, and Any Medical Debt at 12 Months

	Unweighted	Weighted
Panel A: Intent-to-Treat Effect on Any Medical Debt		
Coefficient on Z	-0.059 (0.008)	-0.056 (0.009)
Control Mean ($Z = 0$)	0.576	0.581
N	16,377	16,364
Panel B: First Stage for Medicaid Coverage		
Coefficient on Z	0.319 (0.007)	0.320 (0.008)
Robust first-stage F	2012.9	1752.1
Complier rate	0.319	0.320
Panel C: Local Average Treatment Effect of Coverage on Any Medical Debt		
LATE (W on Any Medical Debt)	-0.187*** (0.026)	-0.174*** (0.028)

Notes: Panel A reports ITT effects of lottery (Z) on 12-month medical debt. Panel B reports the first stage (Z on Medicaid W). Panel C reports LATE (Wald) estimates. Household-clustered standard errors in parentheses. Weighted specifications use 12-month survey weights. Control mean is unweighted for $Z = 0$. *** p<0.01, ** p<0.05, * p<0.10.

4.4 Risk and Liquidity Indices

To explore whether the aggregate reductions in financial distress are driven by the alleviation of health risk or the relaxation of budget constraints, I construct two latent indices that map into the theoretical objects p_i (health risk) and L_i (liquidity). Crucially, these indices are constructed exclusively using pre-randomization baseline covariates (X_{0m}); no treatment, lottery, or post-treatment outcomes are used, ensuring that these measures capture ex-ante heterogeneity rather than ex-post realizations.

The risk index p_i aggregates the multidimensional health profile into a single scalar, where higher values correspond to greater baseline morbidity and expected need for care. I utilize the full set of baseline health covariates, including indicators for unmet medical and prescription needs, utilization intensity (doctor, ED, and hospital visits in the six months prior to the lottery), self-reported health status, and diagnoses of chronic conditions. To ensure a consistent scale, I reverse qualitative measures so that higher values uniformly indicate worse health. I standardize each component to mean zero and unit variance and define p_i as their simple average. The resulting index has a mean of zero, a standard deviation of 0.47, and a median of -0.042.

The liquidity index L_i captures the tightness of the household's short-run budget constraint independent of ex-ante health risk. This index combines measures of resources and financial strain, again drawing only from baseline data. On the resource side, I construct equivalized household income using pre-lottery income bins and household size, alongside labor market variables such as education and hours worked. On the strain side, I include pre-existing measures of medical debt, out-of-pocket spending, and indicators for borrowing or being refused care prior to the lottery. I reverse the sign of all strain variables so that higher values of L_i correspond to looser constraints (more liquidity). The resulting liquidity index L_i has a mean of zero, a standard deviation of 0.44, and a median of 0.013.

As a pre-diagnostic step, I dichotomize the sample at the median of each index to form four mutually exclusive groups. Individuals with $p_i > -0.042$ are classified as *High Risk*, and those with $L_i < 0.013$ are classified as *High Constraint* (Low Liquidity). The joint distribution reveals a positive association between health risk and financial strain: approximately 34% of the sample falls into the *High Risk, High Constraint* cell, while 34% falls into the *Low Risk, Low Constraint* cell. The off-diagonal groups (those who are healthy but constrained, or sick but unconstrained) each account for roughly 16% of the sample.

4.5 Stratified LATEs by Risk-Liquidity Cells

I re-estimate the causal effect of Medicaid coverage on medical debt within each of these four cells. The lottery instrument remains powerful across all subsamples, with high cluster-robust first-stage F-statistics, ensuring that differences in estimates are not driven by weak identification.

As Table 3 reveals, the results reveal that the aggregate reduction in medical debt is highly heterogeneous. Among *Low Risk, Low Constraint* households, the weighted LATE is small and statistically indistinguishable from zero (-0.040, s.e. 0.052). For this group, Medicaid coverage provides little realized financial benefit on the extensive margin because their baseline probability of incurring debt is already low. In contrast, estimates are large and significant for households facing elevated baseline risk or tight budgets. For *Low Risk but High Constraint* households, the effect size more than doubles to -0.105 (s.e. 0.058), which is marginally significant at the 10% level, suggesting that even for healthy individuals, tight pre-existing liquidity constraints can convert moderate health shocks into debt. The effects are largest for the high-risk groups. For *High Risk, Low Constraint* households, the LATE is -0.181 (s.e. 0.076). Finally, for the *High Risk, High Constraint* group, Medicaid reduces the probability of medical debt by 20.1 percentage points (s.e. 0.036).

These stratified results clarify the mechanism behind the aggregate treatment effect. The reduction in financial distress is not a uniform "income effect." Rather, it is concentrated precisely where theory predicts the highest welfare gains: among households who face both a high probability of adverse health shocks p_i and a binding inability to self-finance care. The aggregate LATE of -0.174 is thus driven primarily by the protection Medicaid provides to the most vulnerable segments of the population.

Table 3: LATE on Any Medical Debt by Risk-Liquidity Group

LATE by risk × liquidity group - Any Medical Debt (Coefficient (SE); stars from Wald test; *** p<0.01, ** p<0.05, * p<0.10)		
Risk-Liquidity Segment	unweighted	weighted
Low risk, low constraint	-0.059 (0.048)	-0.040 (0.052)
Low risk, high constraint	-0.104* (0.055)	-0.105* (0.058)
High risk, low constraint	-0.194*** (0.071)	-0.181** (0.076)
High risk, high constraint	-0.216*** (0.032)	-0.201*** (0.036)

4.6 Instrumental Forest Results

To relax the parametric assumptions of the stratified Wald estimates and fully exploit the high-dimensional baseline covariates, I estimate a Causal IV Forest (Athey et al., 2019 [3]) with the lottery assignment Z_i as an instrument for Medicaid coverage W_i and the outcome

$$Y_i = \mathbf{1}\{\text{any medical debt at 12 months}\}.$$

The forest uses 76 pre-lottery covariates X_i (57 binary, 19 non-binary covariates standardized) and incorporates the 12-month survey weights. For each individual, it produces an observation-level Local Average Treatment Effect $\hat{\tau}_i = \hat{\tau}(X_i)$, interpreted as the C-LATE of Medicaid on the probability of having any medical debt for compliers with covariates X_i. Pointwise intervals from the forest are used descriptively; all formal inference relies on second-stage regressions that treat forest predictions of $\hat{\tau}_i$ as known covariates, ignoring sampling uncertainty from the first-stage estimation.

The distribution of these C-LATEs reveals substantial heterogeneity in the value of coverage. The survey-weighted mean of $\hat{\tau}_i$ is approximately -0.123, implying that, on average across compliers, Medicaid reduces the probability of having any medical debt by about 12.3 percentage points. Because the IV-forest C-LATEs are averaged over the analysis sample using survey weights (and not reweighted by compliance), their mean does not mechanically coincide with the scalar Wald LATE of -0.174; I use the forest primarily to study relative heterogeneity rather than to replace the benchmark global estimate. Aggregating the individual C-LATEs back to the four diagnostic risk-liquidity groups yields a clear gradient consistent with the stratified Wald estimates in Table 3, with the largest predicted reductions for the *High Risk, High Constraint* group. Among *Low Risk, Low Constraint* households, those in relatively good health with strong balance sheets, the average predicted effect is modest (around -6.8 percentage points). For *High Risk, High Constraint* households, who face high expected medical needs and tight budgets, the mean effect is much larger, about -16.5 percentage points, with the two intermediate groups around -13.6 percentage points. Figure 1 summarizes this distribution and highlights the subgroup averages.

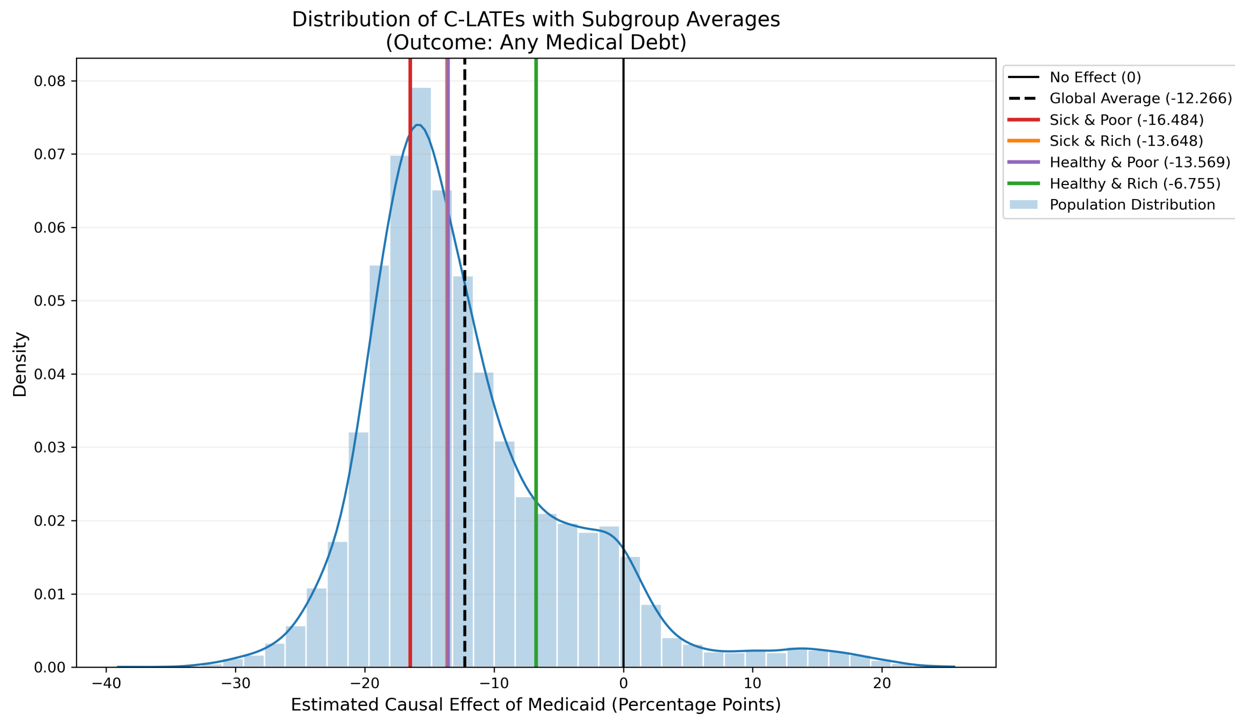
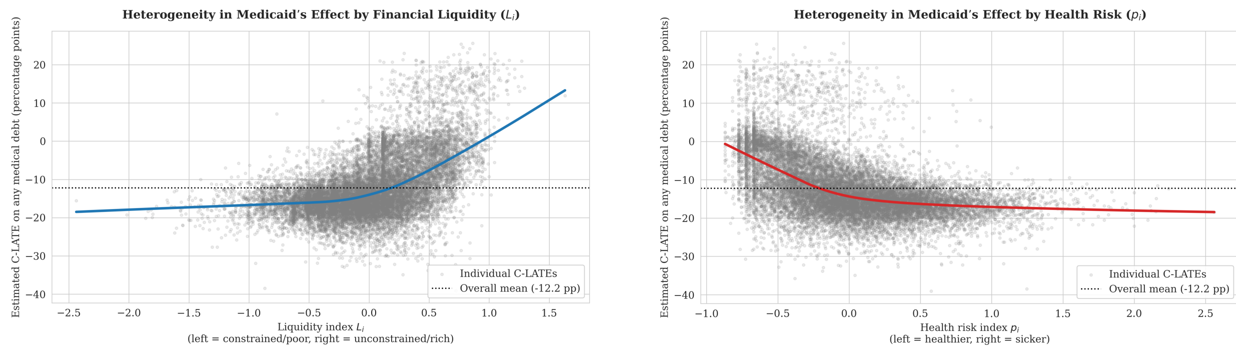


Figure 1: Distribution of estimated C-LATEs on any medical debt with subgroup averages.

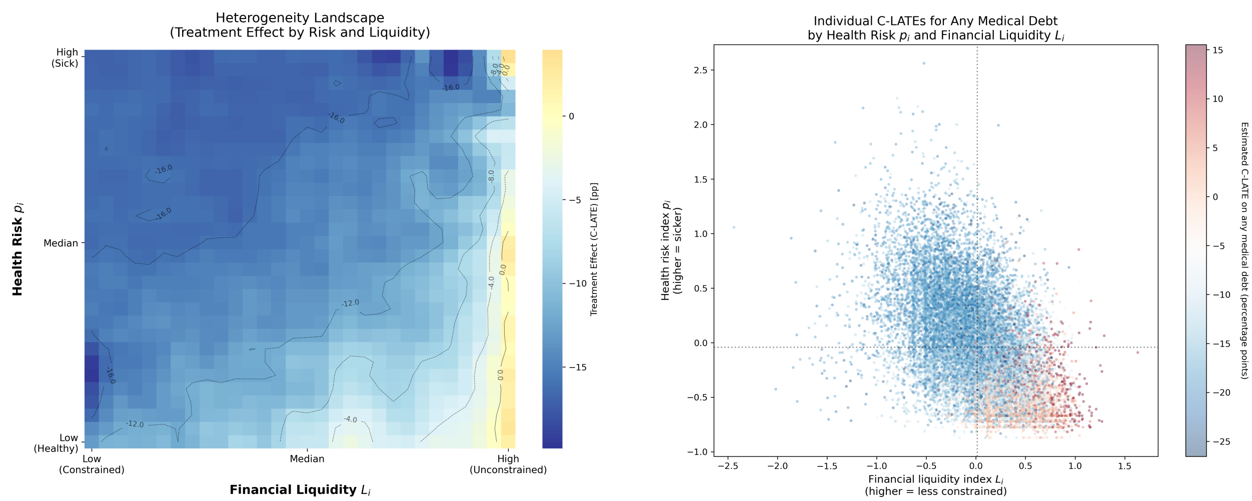
To connect the nonparametric estimates more directly to the model's two key dimensions, Figures 2a and 2b plot smooth projections of $\hat{\tau}_i$ onto the continuous liquidity and risk indices. In Figure 2a, the C-LATE becomes substantially more negative as liquidity tightens (moving left on the L_i axis), consistent with the Nyman liquidity channel: households with tighter budget constraints experience larger debt relief from coverage. In Figure 2b, the C-LATE becomes more negative as baseline morbidity increases, consistent with the Rothschild-Stiglitz risk-smoothing channel. Figures 3a and 3b combine these dimensions in a two-dimensional heterogeneity landscape, 3a shows that the largest average C-LATEs (reductions) on medical debt are concentrated in the more liquidity constrained, higher health risk agents. Analogously, 3b depicts that individual C-LATEs (reductions) on any medical debt are prevalently among the more liquid constrained sick individuals.



(a) Heterogeneity by L_i

(b) Heterogeneity by p_i.

Figure 2: Heterogeneity in Medicaid's effect on any medical debt by financial liquidity L_i (panel a) and health risk p_i (panel b). The solid line shows a descriptive LOWESS smooth of $\hat{\tau}_i$ as a function of the index; the dotted line marks the overall mean effect.



(a) Average C-LATEs

(b) Individual C-LATEs

Figure 3: Average and individual C-LATEs on any medical debt across financial liquidity L_i and health risk p_i. Panel (a) reports survey-weighted mean C-LATEs within quantile bins (smoothed for visualization); panel (b) plots individual C-LATE estimates colored by magnitude.

Finally, Figure 4 summarizes a counterfactual targeting exercise that explicitly accounts for noncompliance. Because the policymaker cannot directly assign Medicaid coverage W_i but can only assign a lottery offer Z_i, the relevant object for targeting offers is the conditional intent-to-treat (ITT) effect of an offer. I therefore construct an observation-level targeting score

$$\widehat{\tau}_{ITT,i} = \hat{\pi}_i \, \hat{\tau}_i,$$

where $\hat{\tau}_i$ is the IV-forest estimate of the C-LATE of coverage on the outcome and $\hat{\pi}_i$ is an estimate of the conditional first stage. In the implementation, $\hat{\pi}_i$ is computed as the survey-weighted take-up difference $E[W_i \mid Z_i = 1] - E[W_i \mid Z_i = 0]$ within quantile cells of the baseline risk and liquidity indices, with the overall survey-weighted first stage used as a fallback for sparse cells; I also impose $\hat{\pi}_i \geq 0$ by clipping at zero.

The solid line plots the resulting policy gain curve: the expected reduction in the population-average probability of having any medical debt as the share of the population offered Medicaid expands, when individuals are ranked by the predicted ITT gain (in percentage points) $-100\,\widehat{\tau}_{ITT,i}$. The dashed line plots the corresponding benchmark under random offers, which increases linearly with the offer share and has slope equal to the survey-weighted mean predicted ITT gain (in percentage points), i.e. the weighted mean of $-100\,\widehat{\tau}_{ITT,i}$ (a homogeneous ITT benchmark). By construction, both curves coincide at 100% offers, but the targeted curve lies above the random-offer benchmark over intermediate offer shares, indicating that targeting by the compliance-weighted C-LATE $\widehat{\tau}_{ITT,i} = \hat{\pi}_i \hat{\tau}_i$ yields larger gains per offer than uniform expansion.

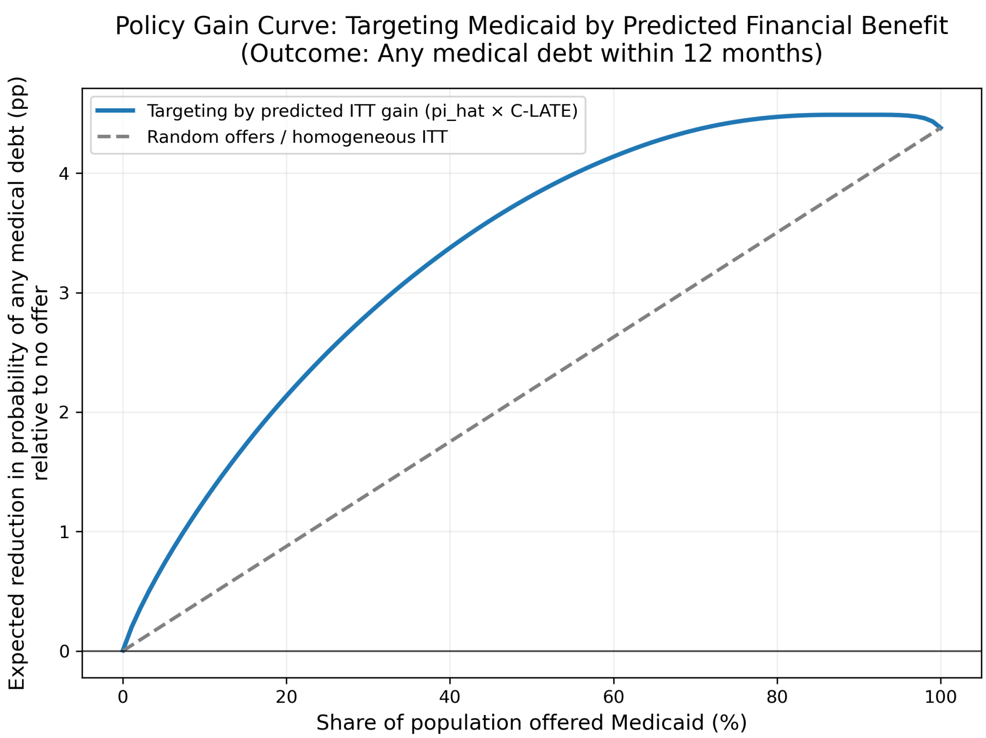


Figure 4: Policy gain curve for targeting Medicaid offers by predicted ITT effects on any medical debt within 12 months. The targeting score is $\widehat{\tau}_{ITT,i} = \hat{\pi}_i \hat{\tau}_i$, where $\hat{\tau}_i$ is the IV-forest C-LATE and $\hat{\pi}_i$ is the estimated conditional first stage (survey-weighted take-up within risk–liquidity quantile bins). The solid line shows expected debt reductions when targeting by predicted ITT; the dashed line shows the benchmark under random offers.

To summarize this heterogeneity in a parsimonious way, I project the observation-level

C-LATEs onto the continuous risk and liquidity indices (p_i, L_i) via the weighted linear projection

$$\hat{\tau}_i = \alpha + \beta_1 p_i + \beta_2 L_i + \beta_3 (p_i \times L_i) + \varepsilon_i,$$

using survey weights and standard errors clustered at the household level. This projection accounts for roughly 37% of the *in-sample* variation in $\hat{\tau}_i$. This magnitude is unsurprising given that the indices aggregate baseline covariates that also inform the forest's heterogeneity splits; accordingly, the R^2 should be interpreted as a descriptive measure of how much of the estimated heterogeneity is captured by these two summary indices rather than as evidence that risk and liquidity are sufficient statistics. The estimated coefficients are reported in Table 4.

The estimated main effects align with the model's qualitative channels. The coefficient on the risk index is $\beta_1 \approx -0.058$ (s.e. 0.001): a one-unit increase in the risk index is associated with roughly a 5.8 percentage point more negative C-LATE, consistent with larger debt relief among sicker compliers. The coefficient on the liquidity index is $\beta_2 \approx 0.062$ (s.e. 0.002); since higher L_i denotes looser liquidity constraints, this positive sign implies that stronger financial position attenuates the debt-relief effect of Medicaid. The interaction term is large in magnitude and negative, $\beta_3 \approx -0.091$ (s.e. 0.003), indicating that risk and liquidity do not operate additively: the marginal association between health risk and the C-LATE varies systematically with liquidity, and vice versa.

Because the dependent variable in this projection is the C-LATE of Medicaid on an indicator for having any medical debt (an adverse outcome) more negative values of $\hat{\tau}_i$ represent greater financial-protection value. For this reason, the theoretical predictions in Section 2 about the sign of derivatives of the underlying value function do not translate mechanically into the sign of the linear interaction coefficient β_3 in this projection. I therefore treat Table 4 as a descriptive linear approximation to the heterogeneity surface, with the primary evidence on the risk–liquidity interaction coming from the stratified LATEs in Table 3 and the two-dimensional heterogeneity plots in Figures 3a and 3b, which show that the largest reductions in medical debt are concentrated among high-risk, high-constraint households.

If heterogeneity aligned purely along liquidity, one would expect the liquidity main effect to dominate and the remaining terms to contribute comparatively little; conversely, if heterogeneity aligned purely along risk, the risk main effect would dominate. Instead, all three coefficients are quantitatively large in this projection, consistent with multi-dimensional and non-additive heterogeneity. In economic terms, and consistent with the stratified LATEs and heterogeneity plots, the debt-reduction gains from Medicaid are largest where high expected medical costs coincide with tight liquidity constraints: for these households, coverage not only reduces out-of-pocket spending but also reduces the likelihood of accumulating medical debt.

Standard errors in this projection are clustered at the household level but should be viewed as descriptive, as they do not incorporate estimation uncertainty from the instrumental forest. The reported significance levels should therefore be interpreted as suggestive rather than fully calibrated. Formal inference on these coefficients would require a household-level block bootstrap that re-estimates the forest and reapplies the projection in each replication.

Table 4: Weighted Linear Projection of C-LATEs on Risk and Liquidity Indices

Weighted least squares regression results

Dep. Variable:	tau_hat_cost_any_owe	R-squared:	0.371
Model:	WLS	Adj. R-squared:	0.371
Method:	Least Squares	F-statistic:	2125.
No. Observations:	16364	Prob(F-statistic):	0.00
Df Residuals:	16360	Log-Likelihood:	21490.
Df Model:	3	AIC:	-4.297e+04
Covariance Type:	cluster	BIC:	-4.294e+04

	coef	std err	z	P> $\|z\|$	[0.025	0.975]
Intercept	-0.1318	0.001	-213.927	0.000	-0.133	-0.131
risk_index_0m	-0.0584	0.001	-42.347	0.000	-0.061	-0.056
liquidity_index_0m	0.0620	0.002	36.146	0.000	0.059	0.065
risk_liq_interaction	-0.0908	0.003	-34.059	0.000	-0.096	-0.086

5 Conclusion

This paper develops a simple two-channel model of the value of public health insurance and brings it to the Oregon Health Insurance Experiment (OHIE). Building on Rothschild and Stiglitz's (1976) [15] theory of risk smoothing and Nyman's (2003) [14] theory of access value, I formalize how Medicaid can generate welfare gains either by reducing the dispersion of consumption across health states for unconstrained households or by relaxing binding liquidity constraints for constrained households. The model delivers three sharp predictions: the value of coverage should increase with ex-ante health risk; it should (weakly) decline with financial liquidity; and the risk gradient should be steepest at low levels of liquidity, implying especially large gains for households that are both high-risk and liquidity-constrained.

The empirical analysis uses the OHIE lottery as a strong instrument for Medicaid enrollment in a 16,579-person survey subsample with rich baseline covariates. The first-stage effect of the lottery on coverage is about 32 percentage points, with cluster-robust F-statistics well above conventional weak-instrument thresholds. In this setting, Medicaid generates substantial average financial protection: for compliers, coverage reduces the probability of having any medical debt at 12 months by 17.4 percentage points (s.e. 0.028) from a control mean of 58.1%, lowers total medical debt by roughly $1,261 (s.e. 375), reduces borrowing or bill-skipping to pay medical expenses by 18.6 percentage points, and nearly eliminates catastrophic out-of-pocket spending above 30% of income. These results reaffirm the benchmark OHIE finding that Medicaid provides large financial benefits to low-income adults.

At the same time, the results show that these gains are far from homogeneous. Stratified LATEs by jointly defined risk and liquidity cells reveal that the extensive-margin effect on medical debt is close to zero for low-risk, low-constraint households, grows in magnitude for either high-risk or high-constraint households, and is largest for those who are both sick and financially constrained: the weighted LATE in this group is -20.1 percentage points (s.e.

0.036). Instrumental-forest estimates based on 76 pre-lottery covariates yield a distribution of observation-level C-LATEs with a survey-weighted mean of about -12.3 percentage points, and show that the average predicted reduction in medical debt is only about -6.8 percentage points for low-risk, low-constraint households but roughly -16.5 percentage points for high-risk, high-constraint households. A simple regression of these C-LATEs on the continuous risk and liquidity indices explains about 37% of their variation and produces large, precisely estimated coefficients: a one-unit increase in the risk index is associated with roughly a 5.8 percentage point larger reduction in medical debt, while a one-unit increase in the liquidity index attenuates the reduction by about 6.2 percentage points; the negative interaction term indicates that risk and liquidity do not operate additively, so the effect of health risk on the C-LATE depends systematically on an individual's liquidity position. Taken together with the stratified LATEs and the two-dimensional forest plots, these patterns are consistent with the model's prediction that the largest welfare gains arise for households that are both high-risk and liquidity-constrained.

Finally, the paper links these heterogeneous estimates to counterfactual policy design. Recognizing that policymakers in the OHIE setting control lottery offers rather than direct enrollment, I move beyond simplified direct-coverage simulations to implement a compliance-aware targeting rule. I rank individuals by their predicted Conditional Intent-to-Treat (ITT) effect, which explicitly combines the welfare value of coverage (captured by the C-LATE) with heterogeneity in take-up (captured by the conditional first stage). The resulting policy gain curve demonstrates that targeting offers based on this composite score yields substantially larger aggregate debt reductions per offer than random allocation. These results highlight the practical value of combining theory-driven heterogeneity analysis with causal machine learning to design social insurance policies that maximize protective efficiency under fixed budgets.

Several limitations remain. The analysis focuses on a single state, a single episode of Medicaid expansion, and a 12-month horizon, so external validity to other contexts and longer-run outcomes is not automatic. The second-stage regression treats the instrumental forest estimates as data and does not propagate first-stage uncertainty, so the reported standard errors for the heterogeneity regressions should be interpreted as approximate. A natural extension for future work would be to implement a household-level block bootstrap that re-estimates both the instrumental forest and the second-stage projection in each replication, thereby fully propagating uncertainty in the C-LATEs into the estimated risk and liquidity coefficients. Moreover, the risk and liquidity indices, while grounded in theory, are constructed proxies rather than direct measurements of the latent primitives. Future work could extend the framework to multiple outcomes (e.g., health), embed general-equilibrium effects, or integrate richer behavioral models of take-up. Nonetheless, the evidence here shows that the financial value of Medicaid is highly concentrated among households facing both high ex-ante health risk and tight liquidity constraints, and that modern IV-based machine learning tools can be used to make this heterogeneity both visible and policy-relevant.

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