NBER WORKING PAPER SERIES

CHOOSING WHO CHOOSES:
SELECTION-DRIVEN TARGETING IN ENERGY REBATE PROGRAMS

Takanori Ida
Takunori Ishihara
Koichiro Ito
Daido Kido
Toru Kitagawa
Shosei Sakaguchi
Shusaku Sasaki

Working Paper 30469
http://www.nber.org/papers/w30469

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
September 2022, Revised September 2023

We would like to thank Keisuke Ito for exceptional research assistance and Judson Boomhower, Severin Borenstein, Fiona Burlig, Lucas Davis, Mark Jacobsen, Ryan Kellogg, Louis Preonas, Wolfram Schlenker, FrankWolak, and seminar participants at UC Berkeley, the Global Energy Challenge Conference at University of Chicago, and the Center on Japanese Economy and Business (CJEB) at Columbia Business School, NBER Summer Institute Public Economics Meeting for their helpful comments. We thank the Japanese Ministry of Environment for their collaboration for this study. Kitagawa and Sakaguchi gratefully acknowledge financial support from ERC grant (number 715940) and the ESRC Centre for Microdata Methods and Practice (CeMMAP) (grant number RES-589-28-0001). The AEA RCT Registry number for our experiment is 6139. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

Choosing Who Chooses: Selection-Driven Targeting in Energy Rebate Programs
Takanori Ida, Takunori Ishihara, Koichiro Ito, Daido Kido, Toru Kitagawa, Shosei Sakaguchi, and Shusaku Sasaki
NBER Working Paper No. 30469
September 2022, Revised September 2023
JEL No. C01,Q4,Q48,Q5,Q58

ABSTRACT

We develop an optimal policy assignment rule that integrates two distinctive approaches commonly used in economics—targeting by observables and targeting through self-selection. Our method can be used with experimental or quasi-experimental data to identify who should be treated, be untreated, and self-select to achieve a policymaker's objective. Applying this method to a randomized controlled trial on a residential energy rebate program, we find that targeting that optimally exploits both observable data and self-selection outperforms conventional targeting. We highlight that the Local Average Treatment Effect (LATE) framework (Imbens and Angrist, 1994) can be used to investigate the mechanism in our approach. By estimating several key LATEs based on the random variation created by our experiment, we demonstrate how our method allows policymakers to identify whose self-selection would be valuable and harmful to social welfare.

Takanori Ida
Graduate School of Economics
Kyoto University
Yoshida, Sakyo
Kyoto 606-8501
Japan
ida@econ.kyoto-u.ac.jp

Takunori Ishihara
Kyoto University of Advanced Science
ishihara.takunori@kuas.ac.jp

Koichiro Ito
Harris School of Public Policy
University of Chicago
1155 East 60th St
Chicago, IL 60637
and NBER
ito@uchicago.edu

Daido Kido
Kyoto University
daido.kido@gmail.com

Toru Kitagawa
Department of Economics
Brown University
Box B
Providence, RI 02912
and Institute For Fiscal Studies - IFS
toru_kitagawa@brown.edu

Shosei Sakaguchi
Faculty of Economics
The University of Tokyo
7-3-1, Hongo
Bunkyo-ku
Tokyo 113-0033
Japan
sakaguchi@e.u-tokyo.ac.jp

Shusaku Sasaki
Osaka University
ssasaki.econ@gmail.com

1 Introduction

Targeting has become a central question in economics and policy design. When policymakers face budget constraints, identifying those who should be treated is critical to maximizing policy impacts. Advances in machine learning and econometric methods have led to a surge in research on targeting in many policy domains, including job training programs (Kitagawa and Tetenov, 2018), social safety net programs (Finkelstein and Notowidigdo, 2019; Deshpande and Li, 2019), energy efficiency programs (Burlig, Knittel, Rapson, Reguant, and Wolfram, 2020), behavioral nudges for electricity conservation (Knittel and Stolper, 2021), and dynamic electricity pricing (Ito, Ida, and Takana, forthcoming).

Economists generally consider two distinctive approaches to the design of effective targeting. The first approach is based on *observable characteristics*. In this approach, policymakers use individuals' observable data to explore optimal targeting (Kitagawa and Tetenov, 2018; Athey and Wager, 2021). The second approach is based on *self-selection*. In this approach, policymakers consider individuals' self-selection as valuable information to target certain individual types (Heckman and Vytlacil, 2005; Heckman, 2010; Alatas, Purnamasari, Wai-Poi, Banerjee, Olken, and Hanna, 2016; Ito, Ida, and Takana, forthcoming).

A priori, which approach is desirable for policymakers is unclear. For example, referring to the two distinctive approaches above as "planner's decisions" and "laissez-faire," Manski (2013) summarizes,

> *"The bottom line is that one should be skeptical of broad assertions that individuals are better informed than planners and hence make better decisions. Of course, skepticism of such assertions does not imply that planning is more effective than laissez-faire. Their relative merits depend on the particulars of the choice problem."*
>
> —Charles F. Manski, *Public Policy in an Uncertain World*

A common view in the literature, reflected in this quote, is that the appropriate approach depends on the context, and therefore, researchers and policymakers need to decide which to use on a case-by-case basis. In this study, we develop an optimal policy assignment rule that systematically integrates these two distinctive approaches commonly used in economics. Consider a treatment from which the social welfare gains are heterogeneous across individuals and can be positive, negative, or zero, depending on who takes the treatment. Our idea is that policymakers can leverage both of the *observable* and *unobservable* information by identifying three types of individuals based on their observable characteristics: i) individuals who should be untreated, ii) those who should be treated, and iii) those who should choose by themselves whether to receive the treatment. Once these individual types are identified, policymakers can design a targeting policy that takes advantage of observed and unobserved heterogeneity in the treatment effect.

We begin by formulating this idea by characterizing a social planner's optimal policy assignment problem following the statistical treatment choice literature (Manski, 2004). We highlight that the Local Average Treatment Effect (LATE) framework (Imbens and Angrist, 1994) can be used to investigate the mechanism in our approach. When individuals have an option to take a treatment, we can define two individual types. *Takers* are those who would take the treatment and *non-takers* are those who would not take the treatment. We demonstrate that the planner's decision rule can be characterized by the LATEs for takers and non-takers as well as the average treatment effect (ATE), all conditional on individuals' observable characteristics.

We then show that the optimal policy assignment rule, LATEs for takers and non-takers and the ATE can be identified and estimated by a randomized controlled trial (RCT) or a quasi-experiment with three randomly-assigned groups: an untreated group, a treated group, and a self-selection group. To estimate the optimal policy assignment, we use the empirical welfare maximization (EWM) method developed by Kitagawa and Tetenov (2018) with policy trees (Zhou, Athey, and Wager, 2023). Further, we demonstrate that the conventional estimation strategy for the LATE (Imbens and Angrist, 1994) can be applied to the three randomly-assigned groups to estimate the LATEs for takers and non-takers.

Our theoretical framework clarifies what variation has to be generated by an RCT or quasi-experiment to estimate the optimal policy assignment. With this insight, we designed an RCT on a residential electricity rebate program and implemented a field experiment in collaboration with the Japanese Ministry of Environment. The policy goal of the rebate program is to incentivize energy conservation in peak demand hours when the marginal cost of electricity tends to be substantially higher than the time-invariant residential electricity price. In our context, the social welfare gain from this rebate program can be heterogeneous across individuals and can be positive, negative, or zero given the existence of per-household implementation cost. This implies that optimal targeting could improve the social welfare gain from this program.

We randomly assigned households to an untreated group, a treated group, and a self-selection group to generate data for our empirical analysis. Using the data from this RCT, we estimate the optimal policy assignment, the ATE, and LATEs for takers and non-takers. We then use our framework to quantify the program's social welfare gain for each of the five policies: i) all consumers get untreated, ii) all consumers get treated, iii) all consumers self-select, iv) optimal targeting without self-selection (*selection-*

absent targeting), and v) optimal targeting with self-selection (*selection-driven targeting*). Our findings suggest that although the conventional targeting (selection-absent targeting) outperforms non-targeting policies, the selection-driven targeting substantially improves welfare relative to the selection-absent targeting. The optimal assignment suggests that 24% of households should be untreated, 31% should be treated, and 45% should self-select. The selection-driven targeting would provide an additional 42% of social welfare gain from the rebate program relative to the selection-absent targeting.

We then use the LATE framework described above to investigate the mechanism in our optimal policy assignment. Given the random assignment in our field experiment, we are able to estimate the LATEs for takers and non-takers conditional on observables. This implies that we can estimate these LATEs for each of the three groups obtained by the optimal assignment rule. Consider households who would be assigned to the self-selection group by the optimal assignment rule. For these households, we find that the LATE for takers is positive and large, and the LATE for non-takers is negative. Hence, self-selection is useful for the planner to sort customers in this group to get treated or untreated by their choice. In contrast, these two LATEs for those who are not assigned to the self-selection group suggest that allowing self-selection hurts social welfare because the planner can obtain higher social welfare gains by assigning them to either compulsory treatment or compulsory un-treatment.

Related literature and our contributions—Our study is related to three strands of the literature. First, many recent studies in economics have explored targeting based either on “observables" or “unobservables" through self-selection. Along with the papers cited earlier in this introduction, recent studies on targeting solely based on individuals’ observable characteristics include Johnson, Levine, and Toffel (forthcoming); Murakami, Shimada, Ushifusa, and Ida (2022); Cagala, Glogowsky, Rincke, and Strittmatter (2021); Christensen, Francisco, Myers, Shao, and Souza (2021); Gerarden and Yang (2023) and studies on targeting based on self-selection include Dynarski, Libassi, Michelmore, and Owen (2021); Lieber and Lockwood (2019); Unrath (2021); Waldinger (2021). However, to the best of knowledge, this is the first study to build an algorithm that systematically integrates these two distinctive targeting approaches to maximize a policy’s social welfare gain.[1]

Second, the medical statistics literature has studied hybrid sampling designs that combine randomization and treatment choice by patients. See, e.g., Janevic, Janz, Dodge, Lin, Pan, Sinco, and Clark (2003), Long,

[1] For example, Gerarden and Yang (2023) and other recent studies on residential electricity demand explore targeting based on the observable characteristics of customers without using self-selection. Our study differs from these studies because our objective is to develop an algorithm that systematically integrates targeting based on self-selection and observables.

Little, and Lin (2008), and references therein. In the medical literature, the sampling process used in our experiment is referred to as "a doubly randomized preference trial" (Rücker, 1989). An example of a clinical trial that implements a doubly randomized preference design is the Woman Take Pride study analyzed in Janevic, Janz, Dodge, Lin, Pan, Sinco, and Clark (2003). These studies focus on assessing whether letting patients choose their own treatment can have a direct causal effect on their health status beyond the causal effect of the treatment itself. See Knox, Yamamoto, Baum, and Berinsky (2019) for partial identification analysis in such a context and an application to political science. Doubly randomized preference trials have received less attention in economics. Bhattacharya (2013) is the only study that uses double randomization between randomized control trials and planner's allocation to assess the efficiency of the planner's treatment allocations. To our knowledge, no work has analyzed doubly randomized preference trial data to integrate targeting by observable characteristics and targeting through self-selection.

Third, our econometric framework builds on the growing statistical treatment choice literature. Generally assuming discrete characteristics, earlier studies in this literature (Manski, 2004; Dehejia, 2005; Hirano and Porter, 2009; Stoye, 2009, 2012; Chamberlain, 2011; Tetenov, 2012, among others) formulate estimation of a treatment assignment rule as a statistical decision problem. The empirical welfare maximization approach proposed by Kitagawa and Tetenov (2018) estimates a treatment assignment rule by maximizing the in-sample empirical welfare criterion over a class of assignment rules. As shown in Online Appendix of Kitagawa and Tetenov (2018) and Zhou, Athey, and Wager (2023), this approach can accommodate multi-armed treatment assignment and a rich set of household characteristics, including continuous characteristics, as in our empirical application. We employ a class of tree partitions considered in Athey and Wager (2021) and Zhou, Athey, and Wager (2023) as our class of policy rules. Finally, building on the LATE framework by Imbens and Angrist (1994), we demonstrate that the newly-defined estimators, the LATEs for *takers* and *non-takers*, can be used to investigate the mechanism in the optimal policy assignment in the presence of self-selection. These LATE estimands can be viewed as the complier's average treatment effects under a multi-valued discrete instrument, which indexes the three arms randomly assigned in the experiment.

2 Conceptual Framework

In this section, we present a theoretical framework of optimal policy assignment in the presence of self-selection. We begin by formulating an optimal policy assignment problem in Section 2.1. In Section 2.2, we present that the Local Average Treatment Effect (LATE) framework (Imbens and Angrist, 1994) can be used to investigate the mechanism in our approach. In Section 2.3, we describe how to empirically estimate the optimal policy assignment and LATEs using data from an RCT and the EWM method.

2.1 Optimal Policy Assignment in the Presence of Self-Selection

Consider a planner who wishes to introduce a policy intervention (program) to a population of interest. Instead of the uniform assignment over the entire population, the planner is interested in targeted assignment for heterogeneous individuals. A novel feature of our setting is that the planner can control not only who is compulsorily exposed to the program but also who is given an option to opt-in to the program. Interpreting an individual's take-up of the program as their exposure to the treatment, the planner's goal is therefore to assign each individual in the population to one of the three arms: *compulsorily treated* (indexed as T), *compulsorily untreated* (indexed as U), and *self-selection* (indexed as S). An individual assigned to T or U is exposed to or excluded from the program with no opt-out or opt-in option, whereas an individual assigned to S chooses whether to take it up by themselves. In our RCT, the treatment refers to participation in the energy rebate program. Hence, individuals assigned to T and U are those who are compulsorily exposed to and excluded from the rebate program, respectively. Individuals assigned to S are those who are given the choice to decide whether to participate in the program on their own.

The planner's goal is to optimize a social welfare criterion by assigning individuals to these three arms. Following the statistical treatment choice literature (Manski, 2004), we specify the planner's social welfare criterion to be the sum of individuals' welfare contributions. An individual's welfare contribution is a known function of the individual's response to being assigned to arm T, U, or S, and the per-person cost of the treatment. An individual's welfare contribution may not correspond to their utility. Hence, if an individual is assigned to S, their utility maximizing decision may not correspond to the choice that maximizes the planner's objective. For example, some individuals assigned to S participate in the energy rebate program to obtain monetary benefits but save less electricity consumption than that needed to compensate for the implementation cost of the program for the planner.

Let Y_T, Y_U, and Y_S denote the potential welfare contributions that would be realized if an individual were assigned to T, U, and S. We assume that the planner observes a pre-treatment characteristic vector for each individual $x \in \mathcal{X}$, where $\mathcal{X}$ denotes the support of the characteristics. Depending on these observable characteristics, the planner assigns each individual to one of the three arms. Let $G_T \subseteq \mathcal{X}$ denote a set of the pre-treatment characteristics x such that any individual whose x belongs to G_T is assigned to T. Similarly, let G_U and G_S denote sets of the pre-treatment characteristics x such that the individuals with $x \in G_U$ are assigned to U and individuals with $x \in G_S$ are assigned to S.

We call a partition $G := (G_T, G_U, G_S)$ an *assignment policy*. G describes how individuals are assigned to arms according to their observable characteristics x. The realized welfare contribution after assignment for an individual with characteristics x is either Y_T, Y_U, or Y_S depending on $x \in G_T$, $x \in G_U$, or $x \in G_S$. Hence, their welfare contribution under the policy G can be written as

$$\sum_{j \in \{T,U,S\}} Y_j \cdot 1\{x \in G_j\}. \tag{1}$$

Viewing individual characteristics and their potential welfare contributions as random variables, the average welfare contribution under assignment policy G can be written as

$$\mathcal{W}(G) \equiv E\left[\sum_{j \in \{T,U,S\}} Y_j \cdot 1\{X \in G_j\}\right], \tag{2}$$

where the expectation is with respect to (Y_T, Y_U, Y_S, X).

We define $\mathcal{W}(G)$ as our social welfare function. The social welfare function depends on the assignment policy G through the post-assignment distribution of individual welfare contributions, which can be manipulated by changing the individuals assigned to the different arms. This form of social welfare is standard in the statistical treatment choice literature. Y_j is not restricted to any specific functional form. Therefore, the planner can choose an appropriate social welfare function.

The planner's objective is to find the optimal assignment policy G^* that maximizes the social welfare $\mathcal{W}(G)$ over a set of possible assignment policies. If the planner can implement any assignment policy, this set of assignment policies corresponds to the set of measurable partitions of $\mathcal{X}$. G^* can be defined by

$$G^* \in \arg\max_{G \in \widetilde{\mathcal{G}}} \mathcal{W}(G), \tag{3}$$

where $\widetilde{\mathcal{G}} := \{G = (G_T, G_U, G_S) : G \text{ is a measurable partition of } \mathcal{X}\}$.

It is desirable that individuals with characteristics x be assigned to an arm that provides the largest conditional mean welfare contribution among $\{E[Y_j|x] : j \in \{T, U, S\}\}$. In the absence of a self-selection treatment arm, the planner's assignment policy is to allocate them to either T or U. The optimal choice is then determined by comparing $E[Y_T|x]$ and $E[Y_U|x]$. In other words, an optimal assignment policy exploits only heterogeneity in the average welfare contribution conditional on observable characteristics x, which can be assessed by the planner prior to assignment. We use $G^{\dagger}$ to denote this sub-optimal policy assignment and call it *the selection-absent targeting*.

Once individuals are permitted to self-select treatment, social welfare can be improved beyond the level attained by the selection-absent targeting. This is because an individual may possess private information, which drives or helps predict their response to the treatment, and choose whether to receive treatment based on it. Importantly, there can be significant heterogeneity in the usefulness of self-selection for the planner's objective. Individuals with some values of x choose by themselves the treatment that is optimal in terms of the social welfare. In contrast, individuals with other values of x may choose treatment that does not improve social welfare. Thus, an optimal assignment policy that identifies who should be assigned to S along with T and U could further improve welfare. We use G^* to denote this optimal policy assignment and call it *the selection-driven targeting*. In this case, the planner allocates individuals with x to either T, U, or S by comparing $E[Y_T|x]$, $E[Y_U|x]$, and $E[Y_S|x]$.[2]

2.2 Using the LATE Framework to Investigate the Mechanism

In this section, we present a simple model that clarifies how the optimal assignment policy G^* assigns T, U, and S to individuals in accordance with individual observable characteristics x. We highlight that the framework of the Local Average Treatment Effect (LATE) developed by Imbens and Angrist (1994) can be used to uncover the mechanism in our approach. Let $D_S \in \{0,1\}$ denote the individual's take-up of treatment when assigned to S. $D_S = 1$ means that the consumer would take the treatment if she is assigned to S, and $D_S = 0$ means that she would not take the treatment if she is assigned to S. The choice D_S may depend on both observable characteristics X and unobservable characteristics (i.e., private information). We define the LATEs for *takers* and *non-takers* as follows, which will be useful statistics to characterize the mechanism of optimal policy assignment.

Definition 2.1. *(The LATEs for takers and non-takers) Let $D_S \in \{0,1\}$ denote an individual's treatment take-up when they self-select into treatment and (Y_T, Y_U) denote the treated and untreated poten-*

[2]This also implies that comparing the sub-optimal assignment policies (such as the assignment policy with T and U only) against the optimal assignment policy allows us to estimate the welfare cost of eliminating an arm option or options.

tial outcomes. We define the LATE for takers by $E[Y_T - Y_U|D_S = 1]$ *and the LATE for non-takers by* $E[Y_T - Y_U|D_S = 0]$.[3]

In our RCT, the takers are individuals who voluntarily participate in the energy rebate program. The non-takers, on the other hand, are those who choose not to participate in the program when assigned to S.

Additionally, we make the following assumption, which is not required for the validity of our method in Section 2.1 but useful to investigate the mechanism.

Assumption 2.2. *The following holds:*

$$Y_S = Y_T \cdot 1\{D_S = 1\} + Y_U \cdot 1\{D_S = 0\}.$$

The meaning of Assumption 2.2 is that an individual's response to the treatment is the same irrespective of whether they self-select themselves or are assigned to it by the planner. That is, who chooses the treatment, either the individuals themselves or the planner, does not have causal impact on the individuals' outcomes, and this can be viewed as the exclusion restriction for instrumental variables, with an indicator for assignment to the self-selection treatment corresponding to an instrumental variable. This analogy to instrumental variable exclusion, combined with the random assignment of the self-selection treatment, provides a necessary testable implication for Assumption 2.2 as shown at the end of this section.

We use $p_1(x) = P(D_S = 1|x)$ and $p_0(x) = P(D_S = 0|x)$ to denote the probability of take-up conditional on x. Under Assumption 2.2, $E[Y_j|x]$ can be decomposed by,

$$E[Y_j|x] = \begin{cases} p_1(x) \cdot E[Y_T|D_S = 1, x] + p_0(x) \cdot E[Y_U|D_S = 0, x] & \text{if } j = S \\ p_1(x) \cdot E[Y_j|D_S = 1, x] + p_0(x) \cdot E[Y_j|D_S = 0, x] & \text{if } j \in \{T, U\}. \end{cases} \tag{4}$$

We can use equation (4) to investigate how the planner ranks the three assignments (T, U, S) for individuals with x. First, consider what condition makes the planner prefer S over U. Equation (4) implies that $E[Y_S - Y_U|x] = p_1(x) \cdot E[Y_T - Y_U|D_S = 1, x]$. Assuming $p_1(x) > 0$, $E[Y_S - Y_U|x] \geq 0$ if only if $E[Y_T - Y_U|D_S = 1, x] \geq 0$. That is, the LATE for takers has to be greater than or equal

[3] An alternative way to define $E[Y_T - Y_U|D_S = 1]$ and $E[Y_T - Y_U|D_S = 0]$ is to use the average treatment effects on the treated (ATT) and the average treatment effects on the untreated (ATU). $E[Y_T - Y_U|D_S = 1]$ is the ATT for individuals assigned to group S and $E[Y_T - Y_U|D_S = 0]$ is the ATU for individuals assigned to group S. In our context, these terms could create confusion because there is another ATT for those assigned to the compulsory treatment group (T) and another ATU for those assigned to the compulsory untreated group (U). To avoid this confusion, we use the terms defined in definition 2.1.

to 0. Second, consider what condition makes the planner prefer S over T. Equation (4) implies that $E[Y_S - Y_T|x] = p_0(x) \cdot E[Y_U - Y_T|D_S = 0, x]$. Assuming $p_0(x) > 0$, $E[Y_S - Y_T|x] \geq 0$ if only if $E[Y_T - Y_U|D_S = 0, x] \leq 0$. That is, the LATE for non-takers has to be less than or equal to 0.

Finally, the condition that makes the planner prefer T over U is trivial such that $E[Y_T - Y_U|x] \geq 0$. Combining the three conditions, we can characterize the optimal assignment policy G^* as defined in equation (3) that has the form $G^* = (G_T^*, G_U^*, G_S^*)$ with

$$
\begin{aligned}
G_T^* &= \{x \in \mathcal{X} : E[Y_T - Y_U|x] \geq 0 \text{ and } E[Y_T - Y_U|D_S = 0, x] > 0\}, \\
G_U^* &= \{x \in \mathcal{X} : E[Y_T - Y_U|x] < 0 \text{ and } E[Y_T - Y_U|D_S = 1, x] < 0\}, \\
G_S^* &= \{x \in \mathcal{X} : E[Y_T - Y_U|D_S = 1, x] \geq 0 \text{ and } E[Y_T - Y_U|D_S = 0, x] \leq 0\}.
\end{aligned}
\tag{5}
$$

Equation (5) implies that the key statistics that characterize the optimal assignment mechanism are the ATE ($E[Y_T - Y_U|x]$), the LATE for takers ($E[Y_T - Y_U|D_S = 1, x)$, and the LATE for non-takers ($E[Y_T - Y_U|D_S = 0, x]$), all conditional on observables.

2.3 Estimation

In this section, we describe how data from an RCT allows us to estimate the optimal policy assignment (G^*) presented in Section 2.1 and LATEs for takers and non-takers described in Section 2.2. To estimate G^*, we use the EWM method in Kitagawa and Tetenov (2018). Let the RCT data be a size n random sample of (Y_i, Z_i, X_i), where $Z_i \in \{T, U, S\}$ is individual i's randomly-assigned treatment arm, Y_i is their observed outcome (welfare contribution), and X_i are their observable pre-treatment characteristics. We use $\{Y_{T,i}, Y_{U,i}, Y_{S,i}\}$ to denote potential outcomes for individual i. The observed outcome Y_i is subject to $Y_i = \sum_{j \in \{T,U,S\}} Y_{j,i} \cdot 1\{Z_i = j\}$. We assume that $\{Y_{T,i}, Y_{U,i}, Y_{S,i}, X_i\}_{i=1,\dots,n}$ are independently and identically distributed as $\{Y_T, Y_U, Y_S, X\}$.

Using the RCT data and a class $\mathcal{G}$ of policies G, the EWM method estimates an optimal policy G^* by maximizing the empirical analogue of the social welfare function over $\mathcal{G}$:

$$
\begin{aligned}
\hat{G}^* &\in \arg\max_{G \in \mathcal{G}} \widehat{\mathcal{W}}(G), \\
\widehat{\mathcal{W}}(G) &\equiv \frac{1}{n} \sum_{i=1}^{n} \sum_{j \in \{T,U,S\}} \left(\frac{Y_i \cdot 1\{Z_i = j\}}{P(Z_i = j|X_i)} \cdot 1\{X_i \in G_j\} \right),
\end{aligned}
\tag{6}
$$

where $\widehat{\mathcal{W}}(G)$ is an empirical welfare function of G that produces an unbiased estimate of the population social welfare $\mathcal{W}(G)$. Observations are weighted by the inverse of the propensity scores, $P(Z_i = j|X_i)$, which are known from the RCT design.

The EWM approach is model-free: It does not require any assumptions or a functional form specification for the potential outcome distributions. However, the class of policies $\mathcal{G}$ must be specified, considering any feasibility constraints for assignment policies. If the class $\mathcal{G}$ is too rich, the EWM solution $\hat{G}_{EWM}$ overfits the RCT data, and the social welfare attained by the estimated policy falls. We use a class of decision trees (Breiman, Friedman, Olshen, and Stone, 2017) as $\mathcal{G}$ because of the ease of interpretation of the decision tree-based assignment policies and the availability of partition search algorithms from the classification tree literature.

We now demonstrate that the LATE for takers ($E[Y_T - Y_U|D_s = 1, x]$) and non-takers ($E[Y_T - Y_U|D_S = 0, x]$) can be also identified and estimated by the RCT data under Assumption 2.2. Supposing that the experimental assignment Z is randomly assigned and that Assumption 2.2 holds, our identification strategy is the same as that of Imbens and Angrist (1994).[4] We denote the observed take-up by $D \in \{0, 1\}$, which obeys $D = 1\{Z = T\} + 1\{Z = S, D_S = 1\}$. Furthermore, we suppress the dependence on x for ease of notation, although all expectations are taken conditional on x.

First, we discuss the identification and estimation of the LATE for takers. As illustrated in Section 2.2, the ITT between S and U (i.e., $E[Y_S - Y_U]$) equals to $p_1 \cdot E[Y_T - Y_U|D_S = 1]$ where $p_1 = P(D_S = 1)$. Then, the experimental variation of S and U allows us to identify the ITT and p_1 by $E[Y|Z = S] - E[Y|Z = U]$ and $P(D = 1|Z = S)$, respectively. Consequently, the LATE for takers can be identified by

$$E\left[Y_T - Y_U|D_S = 1\right] = \frac{E\left[Y|Z = S\right] - E\left[Y|Z = U\right]}{P(D = 1|Z = S)}. \tag{7}$$

This identification result is simply the application of the conventional LATE framework to experimental groups S and U. Thus, we can estimate this LATE by running the instrumental variable (IV) estimation using data from two groups ($Z \in \{S, U\}$) with the randomly-assigned Z as an instrument for take-up D.

Similarly, the ITT between T and S (i.e., $E[Y_T - Y_S]$) can be written as $p_0 \cdot E[Y_T - Y_U|D_S = 0]$ with $p_0 = P(D_S = 0)$. The experimental variation of T and S allows us to identify the ITT and p_0 by

[4] In general, the identification of LATE requires monotonicity assumption. In our application, this assumption is automatically satisfied by the nature of groups. In fact, if we define $D_T \in \{0, 1\}$ and $D_U \in \{0, 1\}$ as the individual's potential take-up when assigned to T and U, it always holds that $1 \equiv D_T \geq D_S \geq D_U \equiv 0$ since non-compliance is not allowed under T and U.

$E[Y|Z=T]-E[Y|Z=S]$ and $P(D=0|Z=S)$. Thus, the LATE for non-takers can be identified by

$$E\left[Y_T - Y_U | D_S = 0\right] = \frac{E\left[Y|Z=T\right] - E\left[Y|Z=S\right]}{P(D=0|Z=S)}. \tag{8}$$

This result can be regarded as the application of conventional LATE framework to experimental groups T and S. In our empirical application in Section 5.1, we estimate equations (7) and (8).

Identification of the LATE for takers and non-takers crucially relies on the exclusion restriction of Assumption 2.2, while validity of Assumption 2.2 can be arguable depending on the context. Similarly to the testable implications of the instrument validity assumption for LATE models shown by Balke and Pearl (1997), Imbens and Rubin (1997), and Heckman and Vytlacil (2005), non-negativity of the potential outcome distributions for takers and non-takers identified by Assumption 2.2 and the random assignment of Z requires the following inequalities on the distribution of observables:

$$\begin{aligned} f(y|Z=T) &\geq f(y|D=1, Z=S) \cdot \Pr(D=1|Z=S), \\ f(y|Z=U) &\geq f(y|D=0, Z=S) \cdot \Pr(D=0|Z=S) \end{aligned} \tag{9}$$

for all $y \in \mathbb{R}$, where $f(y|\cdot)$ denotes the probability density function of the observed outcome Y conditional on the corresponding event. The instrument validity test available in the literature such as the test of Kitagawa (2015) can be applied to empirically assess these inequalities and it can serve as a specification test for Assumption 2.2. We perform this test with our data in Section 5.1.

3 Field Experiment and Data

The framework in Section 2 highlighted that data from an RCT can be used to estimate the optimal policy assignment in the presence of self-selection. In this section, we describe how we designed and implemented such an RCT in the context of a residential energy rebate program in Japan. Section 3.1 provides an overview of the field experiment. Section 3.2 presents summary statistics and balance test.

3.1 Field Experiment

We conducted our field experiment in the summer of 2020 in collaboration with the Ministry of the Environment, Government of Japan in the Kansai (around Osaka) and Chubu (around Nagoya) regions of

Japan. To include a broad set of households, we invited customers in these regions both by letter and email with a participation reward with 2000 JPY ($\approx$ 20 USD, given 1 ¢ $\approx$ 1 JPY in the summer of 2020). A total of 4446 customers pre-registered for the experiment. Non-residential customers, those who canceled their electricity contracts in the middle of the experiment, and those who have incomplete high-frequency electricity usage data were excluded. This left us with 3870 residential customers. That is, our experiment was an RCT for households who agreed to participate in the experiment, which is common in the literature of residential electricity demand (Wolak, 2011; Ito, Ida, and Takana, forthcoming).[5]

We randomly assigned the 3870 households to one of the following three groups: untreated group (U), treated group (T), and self-selection group (S).[6]

Untreated group (U): 1577 customers did not participate in the rebate program.

Treated group (T): 1486 customers participated in the rebate program.

Selection group (S): 807 customers were asked to self-select into the rebate program.

The rebate program in our experiment is called the "peak-time rebate" (PTR) program (Wolak, 2011). The fundamental inefficiency in electricity markets in many countries is that residential electricity prices do not fully reflect the time-varying marginal cost of electricity. In peak hours, the time-invariant residential price tends to be too low relative to time-variant marginal cost. This creates a text-book example of short-run deadweight loss. The goal of peak-time rebate programs is to lower this deadweight loss by setting the rebate incentive close to the marginal cost.

The objective of our PTR was to reduce residential electricity consumption in the system peak hours (between 1 pm and 5 pm) during the week of August 24 to 30, 2020. To prevent customers from manipulating their baseline usage, we did not tell them how the baseline was calculated until August. The baseline usage is each customer's average electricity usage during the peak hours from July 1 to 31. During the treatment week (from August 24 to 30), customers who enrolled in the rebate program received a rebate that was equal to the energy conservation during the peak hours relative to the baseline (kWh) times 100 JPY per 1kWh. Customers who enrolled in the program were notified about the information about the treatment week, peak hours, and reward calculation procedure in the beginning of August.

[5]Because our experiment was an RCT for households who agreed to participate in the experiment, the external validity of the sample is an important question. To investigate this point, we collected data from a random sample of 2070 customers who resided in the experimental locations but did not participate in the experiment. We find that the experimental sample has slightly higher sample averages in their monthly electricity usage, number of people at home on weekdays, self-efficacy in energy conservation, and household income.

[6]The random assignment process was designed such that U: T: S= 2: 2: 1. A relatively large number of households were assigned to the U and T groups in consideration that the data for these groups was going to be used for other studies.

Customers in the selection group (S) were asked to send an email or a prepaid post card during the two-week period from July 31 to August 11 if they intended to participate in the rebate program. The take-up rate was 37.17%, which was rather higher than those for Critical Peak Pricing (CPP) in previous studies.[7] As mentioned above, the PTR never make consumers pay more, unlike the CPP treatment, which may have contributed to the higher take-up rate. At the same time, although the PTR would not make any participating household worse off financially, the take-up rate was lower than 100%, which could imply that there were non-financial reasons for a relatively low take-up, including inertia to participate in a new program.

3.2 Data and Summary Statistics

Our primary data is household-level electricity usage over a 30-minute interval. We collected this data in the pre-experimental period (from July 1 to 31, 2020) and the experimental period (from August 24 to 30, 2020). We also conducted a survey before the experiment to collect a variety of household characteristics.

[Table 1 about here]

Table 1 presents summary statistics and balance check. Columns 1, 2, and 3 present the sample averages by the randomly-assigned group ($Z = \{U, T, S\}$) with the standard deviations in brackets. Columns 4 to 6 report the difference in sample means with the standard error in parentheses. The first three variables are electricity usage (watt hour per 30-minute) in peak hours (from 1 pm to 5 pm), pre-peak hours (from 10 am to 1 pm), and post-peak hours (from 5 pm to 8 pm). The rest of the variables are from the survey. "Number of people at home" is the number of household members usually at home on weekdays. The survey also asked the self-efficacy in energy conservation using the 5-point Likert scale, in which higher scores imply higher self-efficacy. The household income is reported in 10000 JPY. "All electric" equals one if a customer has an all-electric service with no natural gas service. The survey also asked about the numbers of room air conditioners, electric fans, household members, and the total living area.

4 Optimal Assignment Policy and Welfare Gains

In this section, we apply the framework developed in Section 2 to our experimental data. In our framework, the planner's objective is to find the optimal policy assignment rule $G^* = (G_T^*, G_U^*, G_S^*)$ that maxi-

[7] The take-up rate for the CPP was 20% in Fowlie et al. (2021) and 16% (without a take-up incentive) in Ito et al. (forthcoming).

mizes the welfare gain $\mathcal{W}(G)$. We define $\mathcal{W}(G)$ in our empirical context in Section 4.1, describe exogenous parameters and estimation details in Section 4.2, and report the results in Section 4.3.

4.1 Construction of the Social Welfare Criterion

We use p and c to denote the price and marginal cost of electricity. In peak hours, the time-invariant residential price p tends to be too low relative to c. The goal of peak-time rebate programs is to reduce welfare loss from this economic inefficiency by setting the rebate incentive equal to c.

Consider a household that takes the rebate program. We use Q_U and Q_T to denote the potential untreated and treated outcomes of electricity consumption. We assume a locally-linear demand curve for electricity usage. Then, the short-run social welfare gain from this program can be written by $\frac{1}{2}(p-c)(Q_T-Q_U)$. Further, we consider that the reduction in consumption creates an additional long-run social welfare gain as it saves the cost of power plant investments. We denote this long-run gain by $\delta(Q_T-Q_U)$, where δ is the price per kW in the capacity market. Finally, the participation to the rebate program incurs an implementation cost per customer by a.

Then, for each $j \in \{T,U,S\}$, the social welfare gain from the rebate program can be written by,

$$\Delta Y_j := Y_j - Y_U = b \cdot (Q_j - Q_U) - a \cdot 1\{D_j = 1\}, \tag{10}$$

where Y_j is the potential outcome of social welfare for $j \in \{T,U,S\}$, Q_j is the potential outcome of electricity usage, D_j is the potential outcome of consumer's take-up of the program for $j \in \{T,U,S\}$, and $b = \frac{1}{2}(p-c)+\delta$. Note that $-b \cdot Q_U$ in equation (10) does not depend on policy assignment, and therefore, we can replace ΔY_j with $Y_j \equiv b \cdot Q_j - a \cdot 1\{D_j = 1\}$ and define an population optimal assignment policy G^* as a maximizer of the criterion of $\mathcal{W}(G) = E[Y_j \cdot 1\{X \in G_j\}]$. Using the sample, we estimate G^* by maximizing the following objective function with respect to G over a class of policies $\mathcal{G}$:

$$\widehat{\mathcal{W}}(G) = \frac{1}{n}\sum_{i=1}^{n}\sum_{j\in\{T,U,S\}} \frac{Y_i \cdot 1\{Z_i = j\}}{P(Z_i = j|X_i)} \cdot 1\{X_i \in G_j\},$$

where i indicates each household in the sample, n is the sample size, $Y_i = b \cdot Q_i - a \cdot 1\{D_i = 1\}$, Q_i is the observed electricity usage for household i, $D_i = 1$ if household i is treated, and $Z_i \in \{T,U,S\}$ is the randomly-assigned group.

4.2 Estimation Details

Equation (10) includes four exogenous parameters: p, c, a, and δ. We use data from the Japanese electricity market during our experimental period to set the values for these parameters. p is the unit price of electricity. We set to $p = 25$ JPY/kWh, approximately the regulated price of electricity in Japan, which is independent of the time of a day. c is the marginal cost of production for electricity. We specify $c = 125$ JPY/kWh, so that the difference between p and c is equal to the rebate per kWh, which is 100 JPY. The wholesale price of electricity sometimes soars during peak hours such as summer afternoons or winter evenings, reflecting supply constraints. In the past, the wholesale price has occasionally exceeded 100 JPY/kWh in summer afternoons. Parameter a represents the administrative cost of implementing our energy saving program. This cost comprises several items, including the installation cost of the Home Energy Management System (HEMS). In 2016, the Japanese government estimated the cost of implementing a demand reduction program, including the installation cost of HEMS, to be 291.1 JPY per household per season (Ida and Ushifusa, 2017). We use this as the value of the administrative cost.

Parameter δ represents the long-term benefits of a unit reduction in energy consumption. We consider the effect of a unit reduction on the capacity market, where future supply capacity is traded between the power generation and retail sectors. In Japan, the capacity market was established in 2020, with the first auction held at that time. In that auction, the Japanese government provided a reference price 9425 JPY/kW to bidders, which we use as the value for δ.

To estimate the optimal policy G^*, we need to solve the optimization problem with the objective function in Section 4.1. To do so, we specify the policy class to be the class of decision trees of depth 6. We select five variables among candidates to be used in constructing the decision trees. The first two variables are constructed by each household's hourly electricity usage data in the pre-experimental period: the average usage in peak hours relative to pre-peak hours and the average usage in peak hours relative to post-peak hours. The other three variables are from pre-experimental survey data: household income, the number of household members usually at home on weekdays, and a measure of the households' self-efficacy in energy conservation. These variables are selected based on their ability to predict electricity consumption and the conditional average treatment effects. Specifically, we select these variables by running two off-the-shelf machine learning algorithms, lasso and random forest, with all the available covariates and assessing the importance of each variable. When using lasso to assess importance, we regress Q_i on all the available covariates with a l_1-penalization term. We order variables in terms of importance by increasing the penalization parameter step-wise and checking which variables remain selected for large penalization parameter values. When using random forest, we estimate the conditional average treatment effects using the causal forest algorithm of (Wager and Athey, 2018) with all available covariates included. We use the frequency with which a variable is used to split nodes as a measure of its importance. These selected variables are those that appeared on the lists of important variables produced by both methods.

We use the decision tree at depth 6, and maximize the empirical welfare criterion by applying the exhaustive search algorithm of Zhou, Athey, and Wager (2023).[8] An important technical detail of the EWM estimation is that the optimized empirical welfare value from the estimation will be an upwardly biased estimate of the true welfare attained by the estimated policy. This is known as the winner's bias (see, e.g., Andrews, Kitagawa, and McCloskey, 2019), and is caused by using the same data twice: once to learn the policy and once to infer the policy's welfare.[9]

To mitigate the winner's bias in our point estimates and confidence intervals, we create an artificial test sample by running random forest regressions of the outcome onto all the covariates and generating outcome observations by the sum of regression fits and permuted regression residuals. We then estimate the optimal welfare by the welfare value in the test sample evaluated at the EWM optimal policy obtained in the original sample. One-sided $1-\alpha$ confidence intervals are constructed by applying the standard normal approximation to t-ratio centered at the point estimate and standard errors estimated with the artificial test sample.

Specifically, we construct test data $\{Y_i^{\text{test}}, Z_i, X_i\}_{i=1}^n$ by generating $(Q_i^{\text{test}}, D_i^{\text{test}})$ in the following procedure and plugging them into $Y_i^{\text{test}} = b \cdot Q_i^{\text{test}} - a \cdot 1\{D_i^{\text{test}} = 1\}$. For each $j \in \{T, U, S\}$, let $I_j := \{i : Z_i = j\}$ be the set of experimental units assigned to arm j.

1. For $i \in I_T$ and $i \in I_U$, set $D_i^{\text{test}} = 1$ and $D_i^{\text{test}} = 0$, respectively. For $i \in I_S$, we estimate $P(D_i = 1|X_i, Z_i = S)$ to obtain $\hat{P}(D_i = 1|X_i, Z_i = S)$ and sample $\{D_i^{\text{test}}\}_{i \in I_S}$ according to $D_i^{\text{test}} \sim \hat{P}(D_i =$

[8] For computational reasons, it is difficult to obtain a globally optimal tree of depth 6 that exactly maximizes the empirical welfare. To alleviate this difficulty, we employ a heuristic two-step procedure to approximate the globally optimal depth-6 tree. Specifically, we first optimize the parent tree of depth 3 that maximizes the empirical welfare in the entire sample; the resulting parent tree divides the entire sample into 8 subsamples. Then, for each subsample, we search the child depth-3 tree that maximizes the empirical welfare within the subsample. We finally graft the child depth-3 trees on the parent tree to construct the depth-6 tree. In the machine learning literature, this grafted-tree approach is common when constructing tree classifiers for computational feasibility (see, e.g., Chapter 2 of Breiman et al. (2017) and Section 9.2 in Hastie et al. (2009)).

[9] The estimation and inference procedures proposed by Andrews et al. (2019) cannot be directly applied to decision tree based policies because the number of candidate policies is infinite.

$1|X_i, Z_i = S)$.

2. For each $j \in \{T, U\}$ and using subsample I_j,

   (a) Estimate the conditional expectation function of electricity usage under arm j given the set of covariates X_i, $E[Q_i|X_i, Z_i = j]$, and calculate residuals $\hat{\epsilon}_i = Q_i - \hat{E}[Q_i|X_i, Z_i = j]$, where $\hat{E}[Q_i|X_i, Z_i = j]$ is the regression fitted value.

   (b) Estimate conditional variance of the regression residuals $E[\epsilon_i^2|X_i, Z_i = j]$ by regressing $\hat{\epsilon}_i^2$ on X_i, and calculate $\hat{\sigma}_i = \sqrt{\hat{E}[\epsilon_i^2|X_i, Z_i = j]}$ for $i \in I_j$. Sample $\{\tilde{\epsilon}_i\}_{i \in I_j}$ iid from the empirical distribution of the standardized residuals $\{\hat{\epsilon}_i/\hat{\sigma}_i\}_{i \in I_j}$, and calculate $\epsilon_i^{\text{test}} = \tilde{\epsilon}_i \cdot \hat{\sigma}_i$ for $i \in I_j$.

   (c) Construct $Q_i^{\text{test}} = \hat{E}[Q_i|X_i, Z_i = j] + \epsilon_i^{\text{test}}$ for $i \in I_j$.

3. For subsample I_S, we additionally include take-up status D_i in the regressions of Q_i and $\hat{\epsilon}_i^2$, and let $\hat{\sigma}_i(D_i) = \sqrt{\hat{E}[\epsilon_i^2|X_i, D_i, Z_i = S]}$. We generate ϵ_i^{test} by drawing $\tilde{\epsilon}_i$ from the standardized residual distribution conditional on take-up status, and setting $\epsilon_i^{\text{test}} = \tilde{\epsilon}_i \cdot \hat{\sigma}_i(D_i^{\text{test}})$. We then construct $Q_i^{\text{test}} = \hat{E}[Q_i|X_i, D_i^{\text{test}}, Z_i = S] + \epsilon_i^{\text{test}}$ for $i \in I_S$.

In this procedure, we estimate the conditional expectation functions of D_i and Q_i using random forests (Friedberg, Tibshirani, Athey, and Wager, 2021; Wager and Athey, 2018).

With these test data, we obtain a point estimator for the maximized welfare gain $\Delta\mathcal{W}(G^*) = \mathcal{W}(G^*) - \mathcal{W}_U$ relative to the welfare level $\mathcal{W}_U$ attained by the uniformly untreated policy by

$$\Delta\widehat{\mathcal{W}(G^*)} \equiv \frac{1}{n}\sum_{i=1}^{n}\sum_{j \in \{T,U,S\}}\left(\frac{Y_i^{\text{test}} \cdot 1\{Z_i = j\}}{P(Z_i = j|X_i)} \cdot 1\{X_i \in \hat{G}_j^*\}\right) - \frac{1}{n}\sum_{i=1}^{n}\frac{Y_i^{\text{test}} \cdot 1\{Z_i = U\}}{P(Z_i = U|X_i)}, \tag{11}$$

where $\hat{G}^*$ is an EWM policy defined in (6) constructed upon the original sample. We form one-sided confidence intervals for the maximal welfare gain $\Delta\mathcal{W}(G^*)$ with coverage $1-\alpha$ by $\left[\Delta\widehat{\mathcal{W}(G^*)} - z_{1-\alpha} \cdot \hat{\sigma}_{\mathcal{W}}/n^{1/2}, \infty\right]$, where $z_{1-\alpha}$ is the $(1-\alpha)$-th quantile of the standard normal distribution and $\hat{\sigma}_{\mathcal{W}}$ is a standard deviation estimator for the summands in equation (11) with $\hat{G}^*$ fixed.

Our approach mitigates winner's bias than the naive approach of reporting the maximized empirical welfare gain $(\widehat{\Delta\mathcal{W}}(\hat{G}^*))$ because in our approach the random forest estimation in Step 1 optimizes the regression fitness criterion that is different from the empirical welfare criterion used by the policy tree to obtain $\hat{G}^*$. This disagreement of the objective functions can significantly reduce the statistical dependence between the EWM policy $\hat{G}^*$ and the maximal welfare constructed upon the test sample $\Delta\widehat{\mathcal{W}(G^*)}$.[10]

As an alternative to our approach, sample splitting offers a simple way to construct an unbiased estimator and asymptotically valid confidence intervals for the welfare attained by a policy estimated by the training sample. However, sample splitting comes at a cost of precision loss in the estimation of both optimal policies and welfare. In addition, since our interest is to infer the welfare at a population optimal policy, an estimate by sample splitting underestimates the population maximal welfare. Our approach, in contrast, can utilize the whole sample to estimate an optimal policy.

4.3 Results of the Optimal Policy Assignment

We estimate the optimal policy assignment that maximizes social welfare based on equation (6) in Section 2. We compare five alternative policies: 1) assigning everyone to U, 2) assigning everyone to T, 3) assigning everyone to S, 4) the selection-absent targeting $G^\dagger$, and 5) the selection-driven targeting G^*.

[Table 2 about here]

In Table 2, we present the welfare performances of three benchmark policies without targeting (100% U, 100% T, and 100% S) followed by the suboptimal and optimal targeting policies ($G^\dagger$ and G^*). For each policy, we estimate the ITT of the welfare gain in JPY per household per season. We find that the 100% T policy induces a welfare gain of 120.7 per consumer, but the effect is not statistically significant. The 100% S policy results in a welfare gain by 180.6 per consumer and is marginally significant at a p-value of 0.107. These results suggest that without targeting, we cannot reject that the policy's net welfare gain can be zero.

Our policy intervention induces both cost (from the implementation cost) and benefit (from the energy conservation), and therefore, the net welfare gain from a consumer can be positive, negative, or zero. This implies that we could increase the policy performance by targeting policies, $G^\dagger$ and G^*. The results in Table 2 suggest that the selection-absent targeting ($G^\dagger$) attains a welfare gain by 387.8 per consumer. Our algorithm identifies that 52.4% of consumers should be treated, and 47.6% of them should be untreated. Furthermore, we find that the selection-driven targeting (G^*) results in a welfare gain of 551.4 per consumer. With this policy, our algorithm identifies that 31.4% of consumers should be treated, 23.9% of them should be untreated, and 44.7% of them should self-select.

[10]Based on extensive Monte Carlo simulations, we show that our procedure substantially reduces the bias in the welfare estimates relative to the naive approach. The results of our Monte Carlo studies are available in Appendix of Ida et al. (2023).

[Tables 3 and 4 about here]

In Table 3, we statistically test the null hypothesis that a policy's welfare gain is larger than another policy's welfare gain. The 100% S generates a larger welfare gain than the 100% T policy, but the difference is not statistically significant (p-value is 0.29). Both of our targeting policies ($G^\dagger$ and G^*) generate statistically larger welfare gains than non-targeting policies. Finally, we find that the selection-driven targeting (G^*) results in a 42% ($= 551.4/387.8 - 1$) larger welfare gain than the selection-absent targeting ($G^\dagger$), and the difference is statistically significant at a p-value of 0.004.

Table 4 presents the covariates distribution by the optimal policy assignment group $G^* = (G^*_U, G^*_T, G^*_S)$. Columns 1, 2, and 3 show the mean and standard deviation by group, and Columns 4, 5, and 6 show the difference between the means and its standard errors. For example, the means of household income indicate that higher-income households are more likely to be assigned to U rather than T or S. Similarly, the means of self-efficacy in energy conservation suggest that households with lower efficacy in energy conservation are more likely to be assigned to U.

5 Mechanism Behind the Optimal Policy Assignment

In this section, we investigate the mechanism in the optimal policy assignment G^*. To do so, we analyze the LATEs for takers and non-takers in Section 5.1 and the counterfactual ITTs in Section 5.2.

5.1 Using the LATE Framework to Uncover the Mechanism

As presented in Section 2.2, an advantage of our research design is that we can identify both of the LATE for *takers* ($E[Y_T - Y_U|D_S = 1]$) and the LATE for *non-takers* ($E[Y_T - Y_U|D_S = 0]$). In this section, we demonstrate that these two LATEs can be used to examine the mechanism in the selection-driven targeting.

In our empirical context, we define takers and non-takers in the following way with the notations used in Section 2.1. If a consumer is assigned to the self-selection group (S), the consumer has a binary choice between getting treated or untreated. We use $D_S = \{0, 1\}$ to denote this potential outcome. That is, $D_S = 1$ (takers) means that the consumer would take the treatment if she is assigned to S, and $D_S = 0$ (non-takers) means that she would not take the treatment if she is assigned to S.

As shown in equation (7) in Section 2.2, we can use the conventional LATE framework by Imbens and Angrist (1994) to demonstrate that $E[Y_T - Y_U|D_S = 1] = \frac{E[Y|Z=S]-E[Y|Z=U]}{P(D=1|Z=S)}$, where $Z = \{S, U\}$ is randomly assigned in our RCT, $Z = S$ is the selection group, $Z = U$ is the untreated group, and D is the observed treatment take-up for those who were assigned to $Z = S$. The numerator of the right-hand side of the equation is the difference in the ITTs between groups S and U, and the denominator is the take-up rate in groups S. Therefore, the sample analogue of this equation can be estimated from our experimental data.[11] A unique feature of our research design is that we have a randomly-assigned compulsory treatment group ($Z = T$) along with groups $Z = \{S, U\}$. As presented in equation (7) in Section 2.2, we can use two groups $Z = \{S, T\}$ to estimate the LATE for non-takers by $E[Y_T - Y_U | D_S = 0] = \frac{E[Y|Z=T]-E[Y|Z=S]}{P(D=0|Z=S)}$.

The LATEs for takers and non-takers can be estimated conditional on X because the randomization of $Z = (U, T, S)$ holds given X. This implies that we can estimate these LATEs by customer types based on X. Consider the optimal assignment rule with the selection-driven targeting policy $G^* = (G^*_U, G^*_T, G^*_S)$. This policy divides customers into three groups based on their observables: those who should be untreated ($X \in G^*_U$), those who should be treated ($X \in G^*_T$), and those who should self-select ($X \in G^*_S$).

[Figure 1 about here]

In Figure 1, we estimate equations (7) and (8) for these three groups, G^*_U, G^*_T, and G^*_S. For those who are assigned to the selection group (G^*_S), the LATE for takers is 2033 and the LATE for non-takers is -818. This implies that self-selection is a useful tool for this group to let customers sort into the treatment choice that is in line with the planner's objective. By contrast, if we allow self-selection for customers in G^*_U, it would decrease welfare because the LATE for takers in this group is -2012. Similarly, if we allow self-selection in G^*_T, it would lower welfare because self-selection would make non-takers out of the treatment even though their LATE is positive and large at 1124. Therefore, the LATE for takers and non-takers presented in Figure 1 highlights how our algorithm chooses who should get treated, untreated, and choose to get treated by themselves.

To empirically assess Assumption 2.2, we perform the test developed by Kitagawa (2015) for the null hypothesis of the inequalities (9). We find that the p-value of this test is 1.000, which provides supporting evidence for Assumption 2.2 with our data.[12]

[11]Equation (7) shows that the LATE for takers is equivalent to the LATE for compliers when we consider two groups with a binary instrument $Z = \{U, S\}$. This implies that we can use the conventional IV estimation to estimate equation (7) under the regular assumptions for identifying the LATE. In particular, a key assumption is the exclusion restriction in equation (2.2).

[12]We find that the p-value is not sensitive to various choices of tuning parameter values in the method.

5.2 Counterfactual Intention-to-Treat Analysis

Another way to investigate the mechanism in our algorithm is to estimate the counterfactual ITTs separately for each of the three groups G_U^*, G_T^*, and G_S^*. For instance, consider customers in G_U^*, who should be untreated according to the optimal assignment rule. In this group, our experiment provided a random variation of $Z = (U, T, S)$. Therefore, we can use this variation to estimate three ITTs as if they were assigned to U, T, and S. Similarly, we can estimate these three ITTs for customers in G_T^* and those in G_S^*.

[Table 5 about here]

Table 5 presents these counterfactual ITTs for each of G_U^*, G_T^*, and G_S^*. The results for customers in G_U^* imply that their ITTs would be negative (-905.4 and -900.6) if they were assigned to T and S, respectively. That is, the welfare gains are maximized if they are assigned to U. Similarly, the results for customers in G_T^* and G_S^* suggest that their welfare gains are maximized when they are assigned to T and S, respectively.

Hence, the policy assignment computed by G^* coincides with the arm that attains the highest welfare gain for every subgroup of $\{X \in G_j^*\}$, $j \in \{U, T, S\}$. In other words, the optimal policy G^* captures households' heterogeneous responses by using both observed and unobserved characteristics to maximize social welfare. While this result is true by construction because our algorithm finds the optimal assignment by maximizing the ITT of the welfare gain, Table 5 is useful to visualize the mechanism by observing that we indeed see the optimal ITTs in the diagonal line.

6 Conclusion

We develop an optimal policy assignment rule that systematically integrates two distinctive approaches commonly used in the literature—targeting by "observables" and targeting through "self-selection." Our method identifies those who should be treated, should be untreated, and should self-select into a treatment to maximize a policy's social welfare gain. We show that targeting that leverages information on both observables and self-selection outperforms conventional targeting. Finally, we highlight that the LATE framework (Imbens and Angrist, 1994) can be used to uncover the mechanism in our approach. We introduce new estimators, the LATEs for *takers* and *non-takers*, to demonstrate how our method identifies whose self-selection is useful and harmful for the planner to maximize social welfare.

References

ALATAS, V., R. PURNAMASARI, M. WAI-POI, A. BANERJEE, B. A. OLKEN, AND R. HANNA (2016): "Self-targeting: Evidence from a field experiment in Indonesia," *Journal of Political Economy*, 124, 371–427.

ANDREWS, I. S., T. KITAGAWA, AND A. MCCLOSKEY (2019): "Inference on Winners," *NBER working paper*.

ATHEY, S. AND S. WAGER (2021): "Efficient policy learning with observational data," *Econometrica*, 89, 133–161.

BALKE, A. AND J. PEARL (1997): "Bounds on treatment effects from studies with imperfect compliance," *Journal of the American Statistical Association*, 92, 1171–1176.

BHATTACHARYA, D. (2013): "Evaluating Treatment Protocols by Combining Experimental and Observational Data," *Journal of Econometrics*, 173, 160–174.

BREIMAN, L., J. H. FRIEDMAN, R. A. OLSHEN, AND C. J. STONE (2017): *Classification and regression trees*, Routledge.

BURLIG, F., C. KNITTEL, D. RAPSON, M. REGUANT, AND C. WOLFRAM (2020): "Machine learning from schools about energy efficiency," *Journal of the Association of Environmental and Resource Economists*, 7, 1181–1217.

CAGALA, T., U. GLOGOWSKY, J. RINCKE, AND A. STRITTMATTER (2021): "Optimal Targeting in Fundraising: A Causal Machine-Learning Approach," *arXiv preprint arXiv:2103.10251*.

CHAMBERLAIN, G. (2011): "Bayesian aspects of treatment choice," in *The Oxford Handbook of Bayesian Econometrics*, ed. by J. Geweke, G. Koop, and H. van Dijk, Oxford University Press, 11–39.

CHRISTENSEN, P., P. FRANCISCO, E. MYERS, H. SHAO, AND M. SOUZA (2021): "Energy Efficiency Can Deliver for Climate Policy: Evidence from Machine Learning-Based Targeting," *Working Paper*.

DEHEJIA (2005): "Program evaluation as a decision problem," *Journal of Econometrics*, 125, 141–173.

DESHPANDE, M. AND Y. LI (2019): "Who Is Screened Out? Application Costs and the Targeting of Disability Programs," *American Economic Journal: Economic Policy*, 11, 213–248.

DYNARSKI, S., C. LIBASSI, K. MICHELMORE, AND S. OWEN (2021): "Closing the gap: The effect of a targeted, tuition-free promise on college choices of high-achieving, low-income students," *American Economic Review*, 111, 1721–1756.

FINKELSTEIN, A. AND M. J. NOTOWIDIGDO (2019): "Take-up and Targeting: Experimental Evidence from SNAP," *Quarterly Journal of Economics*, 134, 1505–1556.

FOWLIE, M., C. WOLFRAM, P. BAYLIS, C. A. SPURLOCK, A. TODD-BLICK, AND P. CAPPERS (2021): "Default Effects And Follow-On Behaviour: Evidence From An Electricity Pricing Program," *The Review of Economic Studies*, 88, 2886–2934.

FRIEDBERG, R., J. TIBSHIRANI, S. ATHEY, AND S. WAGER (2021): "Local Linear Forests," *Journal of Computational and Graphical Statistics*, 30, 503–517.

GERARDEN, T. D. AND M. YANG (2023): "Using targeting to optimize program design: evidence from an energy conservation experiment," *Journal of the Association of Environmental and Resource Economists*, 10, 687–716.

HASTIE, T., R. TIBSHIRANI, AND J. FRIEDMAN (2009): *The Elements of Statistical Learning: Data Mining, Inference, and Prediction*, Springer Series in Statistics, Springer New York, 2 ed.

HECKMAN, J. J. (2010): "Building Bridges between Structural and Program Evaluation Approaches to Evaluating Policy," *Journal of Economic Literature*, 48, 356–398.

HECKMAN, J. J. AND E. VYTLACIL (2005): "Structural Equations, Treatment Effects, and Econometric Policy Evaluation," *Econometrica*, 73, 669–738.

HIRANO, K. AND J. R. PORTER (2009): "Asymptotics for statistical treatment rules," *Econometrica*, 77, 1683–1701.

IDA, T., T. ISHIHARA, K. ITO, D. KIDO, T. KITAGAWA, S. SAKAGUCHI, AND S. SASAKI (2023): "Choosing Who Chooses: Selection-driven targeting in energy rebate programs," *NBER working paper*.

IDA, T. AND Y. USHIFUSA (2017): "Cost-Benefit Analysis of Price-based Residential Demand Response," *Proceedings of the Japan Joint Automatic Control Conference*, 60, 304–307.

IMBENS, G. W. AND J. D. ANGRIST (1994): "Identification and Estimation of Local Average Treatment Effects," *Econometrica*, 62, 467–475.

IMBENS, G. W. AND D. B. RUBIN (1997): "Estimating outcome distributions for compliers in instrumental variables models," *The Review of Economic Studies*, 64, 555–574.

ITO, K., T. IDA, AND M. TAKANA (forthcoming): "Selection on Welfare Gains: Experimental Evidence from Electricity Plan Choice," *American Economic Review*.

JANEVIC, M. R., N. K. JANZ, J. A. DODGE, X. LIN, W. PAN, B. R. SINCO, AND N. M. CLARK (2003): "The role of choice in health education intervention trials: a review and case study," *Social Science and Medicine*, 56, 1581–1594.

JOHNSON, M. S., D. I. LEVINE, AND M. W. TOFFEL (forthcoming): "Improving regulatory effectiveness through better targeting: Evidence from OSHA," *American Economic Journal: Applied Economics*.

KITAGAWA, T. (2015): "A Test for Instrument Validity," *Econometrica*, 83, 2043–2063.

KITAGAWA, T. AND A. TETENOV (2018): "Who should be treated? Empirical welfare maximization methods for treatment choice," *Econometrica*, 86, 591–616.

KNITTEL, C. R. AND S. STOLPER (2021): "Machine Learning about Treatment Effect Heterogeneity: The Case of Household Energy Use," *AEA Papers and Proceedings*, 111, 440–44.

KNOX, D., T. YAMAMOTO, M. A. BAUM, AND A. J. BERINSKY (2019): "Design, Identification, and Sensitivity Analysis for Patient Preference Trials," *Journal of the American Statistical Association*, 114, 1532–1546.

LIEBER, E. M. AND L. M. LOCKWOOD (2019): "Targeting with in-kind transfers: Evidence from Medicaid home care," *American Economic Review*, 109, 1461–85.

LONG, Q., R. LITTLE, AND X. LIN (2008): "Causal Inference in Hybrid Intervention Trials Involving Treatment Choice," *Journal of the American Statistical Association*, 103, 474–484.

MANSKI, C. (2013): *Public Policy in an Uncertain World*, Cambridge, MA: Harvard University Press.

MANSKI, C. F. (2004): "Statistical treatment rules for heterogeneous populations," *Econometrica*, 72, 1221–1246.

MURAKAMI, K., H. SHIMADA, Y. USHIFUSA, AND T. IDA (2022): "Heterogeneous treatment effects of nudge and rebate: Causal machine learning in a field experiment on electricity conservation," *International Economic Review*, 63, 1779–1803.

RÜCKER, G. (1989): "A Two-Stage Trial Design for Testing Treatment, Self-Selection, and Treatment Preference Effects," *Statistics in Medicine*, 8, 477–485.

STOYE, J. (2009): "Minimax regret treatment choice with finite samples," *Journal of Econometrics*, 151, 70–81.

——— (2012): "Minimax regret treatment choice with covariates or with limited validity of experiments," *Journal of Econometrics*, 166, 138–156.

TETENOV, A. (2012): "Statistical treatment choice based on asymmetric minimax regret criteria," *Journal of Econometrics*, 166, 157–165.

UNRATH, M. (2021): "Targeting, Screening, and Retention: Evidence from California's Food Stamps Program," *Working paper*.

WAGER, S. AND S. ATHEY (2018): "Estimation and Inference of Heterogeneous Treatment Effects using Random Forests," *Journal of the American Statistical Association*, 113, 1228–1242.

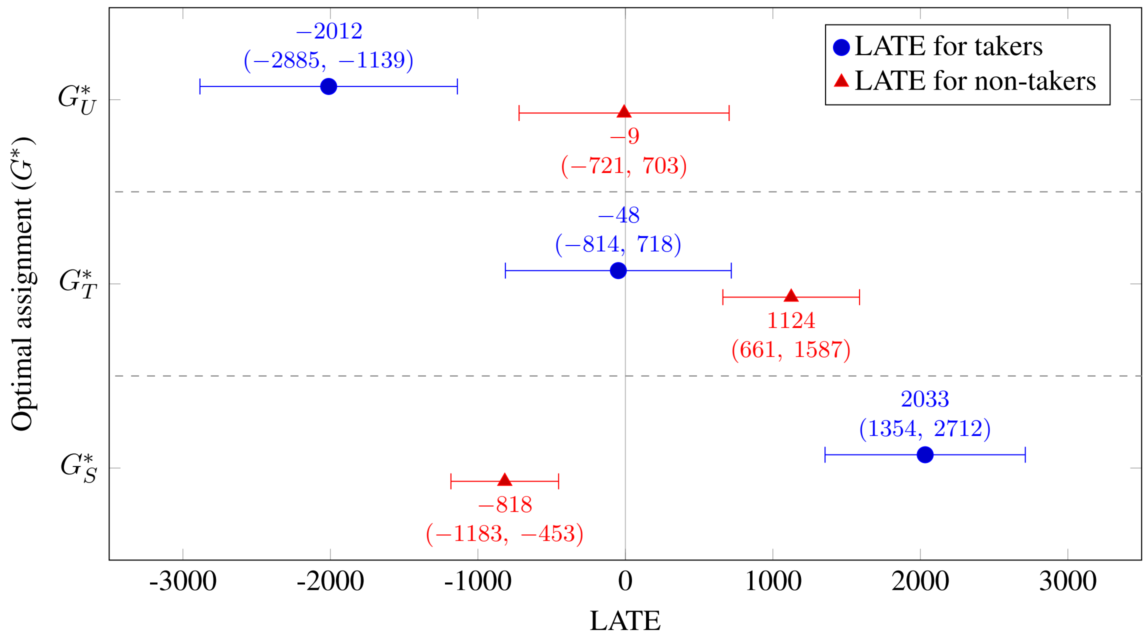
WALDINGER, D. (2021): "Targeting in-kind transfers through market design: A revealed preference analysis of public housing allocation," *American Economic Review*, 111, 2660–96.

WOLAK, F. A. (2011): "Do Residential Customers Respond to Hourly Prices? Evidence from a Dynamic Pricing Experiment," *The American Economic Review*, 101, 83–87.

ZHOU, Z., S. ATHEY, AND S. WAGER (2023): "Offline Multi-Action Policy Learning: Generalization and Optimization," *Operations Research*, 71, 148–183.

Figures

Figure 1: Mechanism Behind the Algorithm: The LATEs for Takers and Non-Takers



Notes: This figure shows the estimation results in Section 5.1. For each of the three groups in the optimal assignment ($x \in G_U^*, x \in G_T^*, x \in G_S^*$), we estimate the LATE for *takers* ($E[Y_T - Y_U|D_S = 1]$) and the LATE for *non-takers* ($E[Y_T - Y_U|D_S = 0]$) to investigate the mechanism in the optimal assignment. We show the point estimates with the 95% confidence intervals. For example, for those who are assigned to the selection group (G_S^*), the LATE for takers is 2033, and the LATE for non-takers is -818. This implies that self-selection is a useful tool for this group to let customers sort into the treatment choice that is in line with the planner's objective. The monetary unit is given as 1 ¢ = 1 JPY in the summer of 2020.

Tables

Table 1: Summary Statistics and Balance Check

	Sample mean by group [standard deviation]			Difference in sample means (standard error)		
	Untreated ($Z = U$)	Treated ($Z = T$)	Selection ($Z = S$)	U vs. T	U vs. S	T vs. S
Peak hour usage (Wh)	192	190	189	2.57	2.87	0.29
	[141]	[138]	[134]	(5.03)	(5.91)	(5.93)
Pre-peak hour usage (Wh)	179	176	180	3.79	-1.11	-4.89
	[137]	[135]	[142]	(4.92)	(6.07)	(6.11)
Post-peak hour usage (Wh)	299	297	293	1.94	6.02	4.08
	[175]	[171]	[174]	(6.26)	(7.54)	(7.56)
Number of people at home	2.48	2.44	2.47	0.04	0.01	-0.03
	[1.24]	[1.24]	[1.27]	(0.04)	(0.05)	(0.06)
Self-efficacy in energy conservation (1-5 scale)	3.45	3.46	3.49	-0.01	-0.04	-0.02
	[0.85]	[0.85]	[0.83]	(0.03)	(0.04)	(0.04)
Household income (JPY 10,000)	645	613	637	31.69	8.45	-23.23
	[399]	[362]	[391]	(13.75)	(17.06)	(16.67)
All electric	0.32	0.31	0.30	0.01	0.02	0.00
	[0.47]	[0.46]	[0.46]	(0.02)	(0.02)	(0.02)
Number of air conditioners	3.14	3.11	3.08	0.03	0.05	0.02
	[1.69]	[1.71]	[1.67]	(0.06)	(0.07)	(0.07)
Number of fans	2.80	2.73	2.77	0.07	0.04	-0.04
	[1.63]	[1.63]	[1.56]	(0.06)	(0.07)	(0.07)
Number of household members	2.76	2.73	2.75	0.04	0.01	-0.03
	[1.27]	[1.27]	[1.28]	(0.05)	(0.06)	(0.06)
Total living area (m^2)	107.29	105.51	103.42	1.78	3.87	2.09
	[48.57]	[49.61]	[46.14]	(1.78)	(2.03)	(2.07)

Notes: Columns 1-3 present the sample mean and standard deviations in blackets for the pre-experiment consumption data and demographic variables by randomly-assigned group: untreated ($Z = U$), treated ($Z = T$), and selection ($Z = S$). Columns 4-6 show the difference in the sample means with the standard error of the difference in parentheses. The number of households are 1,577 (U), 1,486 (T), and 807 (S). The monetary unit is given as 1 ¢ = 1 JPY in the summer of 2020.

Table 2: Welfare Gains from Each Policy

Policy	Welfare gain	Share of customers in each arm		
		G_U	G_T	G_S
100% untreated	0 (—)	100.0%	0.0%	0.0%
100% treated	120.7 (98.8)	0.0%	100.0%	0.0%
100% self-selection	180.6 (112.1)	0.0%	0.0%	100.0%
Selection-absent targeting ($G^{\dagger}$)	387.8 (55.7)	47.6%	52.4%	0.0%
Selection-driven targeting (G^{*})	551.4 (68.2)	23.9%	31.4%	44.7%

Notes: This table summarizes characteristics of three benchmark policies (100% untreated, 100% treated, and 100% self-selection), selection-absent targeting ($G^{\dagger}$), and selection-driven targeting (G^{*}). The column titled "Welfare Gain" shows the estimated ITT of welfare gain in JPY per household per season, with its standard error in parentheses. The monetary unit is given as 1 ¢ = 1 JPY in the summer of 2020.

Table 3: Comparisons of Alternative Policies

	Difference in welfare gains	p-value
100% self-selection vs. 100% treated	59.9 (110.0)	0.293
Selection-absent targeting ($G^{\dagger}$) vs. 100% treated	267.1 (99.7)	0.004
Selection-absent targeting ($G^{\dagger}$) vs. 100% self-selection	207.2 (116.9)	0.038
Selection-driven targeting (G^{*}) vs. 100% treated	430.7 (106.9)	0.000
Selection-driven targeting (G^{*}) vs. 100% self-selection	370.8 (113.9)	0.001
Selection-driven targeting (G^{*}) vs. Selection-absent targeting ($G^{\dagger}$)	163.6 (61.4)	0.004

Notes: This table compares welfare gains from each policy. For each row, the column "Difference in Welfare Gains" shows the estimated welfare gain of the policy on the left-hand side (W_L) relative to the policy on the right-hand side (W_R) in JPY per household per season, with its standard error in parenthesis. The column "p-value" gives the p-value for the null hypothesis: $H_0 : W_L \leq W_R$. The monetary unit is given as 1 ¢ = 1 JPY in the summer of 2020.

Table 4: Covariate Distribution by Optimally Assigned Group G^*

	Sample mean by group [standard deviation]			Difference in sample means (standard error)		
	G_U^*	G_T^*	G_S^*	G_U^* vs. G_T^*	G_U^* vs. G_S^*	G_T^* vs. G_S^*
Peak hour usage (Wh)	203	180	191	23.03	11.98	−11.05
	[146]	[136]	[135]	(6.18)	(5.79)	(5.08)
Pre-peak hour usage (Wh)	198	167	175	30.56	23.08	−7.48
	[150]	[133]	[132]	(6.23)	(5.86)	(4.97)
Post-peak hour usage (Wh)	329	255	310	73.22	18.82	−54.40
	[176]	[176]	[164]	(7.67)	(7.00)	(6.41)
Number of people at home	2.87	2.27	2.38	0.60	0.48	−0.11
	[1.34]	[1.32]	[1.08]	(0.06)	(0.05)	(0.05)
Self-efficacy in energy conservation (1-5 scale)	3.30	3.49	3.53	−0.19	−0.23	−0.04
	[1.02]	[0.82]	[0.75]	(0.04)	(0.04)	(0.03)
Household income (JPY 10,000)	787	597	572	190.12	215.11	25.00
	[433]	[397]	[318]	(18.23)	(16.15)	(13.73)
All electric	0.36	0.25	0.33	0.11	0.03	−0.08
	[0.48]	[0.43]	[0.47]	(0.02)	(0.02)	(0.02)
Number of air conditioners	3.41	2.82	3.16	0.58	0.24	−0.34
	[1.72]	[1.66]	[1.67]	(0.07)	(0.07)	(0.06)
Number of fans	2.99	2.58	2.78	0.41	0.20	−0.21
	[1.75]	[1.57]	[1.55]	(0.07)	(0.07)	(0.06)
Number of household members	3.17	2.54	2.67	0.63	0.50	−0.13
	[1.31]	[1.36]	[1.14]	(0.06)	(0.05)	(0.05)
Total living area (m^2)	115.41	97.16	106.73	18.25	8.68	−9.57
	[47.77]	[49.13]	[47.37]	(2.11)	(1.94)	(1.81)

Notes: This table shows the covariate distribution by group based on the optimal policy assignment G^*. The last column shows the difference in the sample means and its standard errors. The monetary unit is given as 1 ¢ = 1 JPY in the summer of 2020.

Table 5: Mechanism Behind the Algorithm: Counterfactual Analysis of the ITT

	Consumer types based on the optimal assignment rule G^*		
	G_U^*	G_T^*	G_S^*
Counterfactual ITT (if assigned to U)	0	0	0
	(——)	(——)	(——)
Counterfactual ITT (if assigned to T)	−905.4	662.5	257.8
	(157.8)	(131.4)	(117.5)
Counterfactual ITT (if assigned to S)	−900.6	−18.8	767.0
	(184.2)	(153.9)	(120.3)

Notes: This figure shows the estimation results in Section 5.2.

A Online Appendix of NBER Working Paper #30469

A.1 The External Validity of the Experimental Sample

We randomly sampled 2070 customers from the target population who did not participate in this experiment, and conducted a similar survey to the one for the experimental sample. The purpose of this was to investigate the external validity of our experimental sample by comparing the mean for each variable between the control group from our experimental sample and this random sample. Columns 1 and 2 of Table A.1 present summary statistics for the untreated group and the random sample. Column 3 presents differences in means, with the standard errors of these differences in parentheses. We observe larger means for four variables in the untreated group than in the random sample, and the differences are statistically significant. Our experimental sample has larger pre-experiment electricity usage per month, a larger number of people at home on weekdays, higher self-efficacy in energy conservation, and higher household income. This implies that our sample includes a larger number of customers who are willing and able to reduce their electricity consumption, which should be taken into consideration when discussing the generalizability of this study.

A.2 Heterogeneity in the Program's Impact on Peak-Hour Electricity Usage

The rebate program aimed at incentivizing energy conservation in peak hours. A key variable in our social welfare function is, therefore, electricity usage in peak hours. This section provides a simple analysis on heterogeneity in the rebate program's impact on peak-hour electricity usage.

Consider estimating the intention-to-treat (ITT) of the randomly-assigned groups $Z = \{T, S\}$ relative to $Z = U$ by the OLS with an estimating equation,

$$y_{it} = \beta_T T_{it} + \beta_S S_{it} + \lambda_i + \theta_t + \epsilon_{it}, \tag{1}$$

where y_{it} is the natural log of electricity usage for household i in a 30-minute interval t. We include data from the pre-experimental period and experimental period.[1] A dummy variable T_{it} equals one if household i is in group T and t is in the treatment period. A dummy variable S_{it} equals one if household i is in group S and t is in the treatment period. We include household fixed effects λ_i and time fixed effects θ_t for each 30-minute interval to control for time-specific shocks such as weather. Given that $Z = \{U, T, S\}$ is randomly

[1]Because of randomization, the pre-experimental data is not necessary for obtaining the consistent estimator. The primary benefit of including the pre-experimental data is that the inclusion of household fixed effects can substantially increase the precision of the estimates because residential electricity usage tends to form a significant part of household-specific time-invariant variation.

assigned, β_T and β_S provides the ITT of $Z = \{T, S\}$ relative to $Z = U$. Because there is no self-selection in group T, β_T is also the average treatment effect (ATE) of T relative to U. We cluster standard errors at the household level.

In Table A.2, we report the estimation results of equation (1). We begin by demonstrating the ITT for the entire sample in Column 1. The compulsory treatment (T) resulted in a reduction in peak-hour electricity usage by 0.097 log points (9.2%). The self-selection treatment (S) induced a reduction by 0.052 log points (5.1%). The p-value of the difference in these two ITTs is 0.088.[2]

Along with these overall program impacts, an important question for our analysis is whether there is substantial heterogeneity in the effects. If different household types respond to T and S differently, the optimal targeting policy could enhance the welfare gain from the policy. We investigate this question in the remaining columns of Table A.2.[3] Each pair of columns splits the sample into two groups: those with a below median value of a particular variable and those with an above median value.

We find evidence of rich heterogeneity in the program's impact. In columns 2 and 3, for instance, we split customers by peak-hour electricity usage relative to pre-peak hour usage, based on data in the pre-experimental period. For households with lower values of this variable, we find that $\hat{\beta}_T = -0.108$ and $\hat{\beta}_S = -0.022$, and the p-value for the difference is 0.013. In contrast, for households with higher values of this variable, $\hat{\beta}_T = -0.079$ and $\hat{\beta}_S = -0.073$, and the p-value for the difference is 0.88. That is, in terms of the ITT for peak-hour electricity usage, T provides a larger reduction than S for a subgroup, but this is not the case for other groups. We find similar heterogeneity when we split the sample based on the number of people at home, the self-efficacy in energy conservation, and household income.

This heterogeneity in the program's impact on peak-hour electricity usage implies that optimal targeting is likely to enhance the welfare gain from the policy. However, although the simple analysis in Table A.2 is useful, there are two caveats in this analysis. First, these ITTs are not equivalent to the social welfare gains, and therefore, do not necessarily provide full information to rank T and S. For example, these ITTs are related to but do not directly measure consumer surplus or social surplus. Furthermore, the cost of the policy (i.e., the implementation cost per participating household) is not included. Second, the true heterogeneity can be more complex as covariates may have nonlinear and interaction effects on the program's impact. For this reason, we conduct more comprehensive analysis based on a machine-learning method (the decision tree).

[2]For the self-selection group, we can use the ITT and take-up rate to obtain the local average treatment effect for takers (LATE), which is -0.14. This LATE is larger (in absolute value) than the ATE obtained by the T group, suggesting the possibility of selection on welfare gains similar to the findings in Ito, Ida, and Takana (forthcoming).

[3]This table presents results on the covariates selected for estimating the optimal policy.

A.3 Social Welfare Function with a Redistribution Goal

This section describes the derivation of each household's potential welfare contribution. First, we focus on the change in each household's utility resulting from the introduction of the rebate program. Under the assumption that the utility function of each household is quasi-linear and the electricity demand function is linear, assigning a household to arm $j \in \{T, U, S\}$ changes its utility as follows

$$CS_j = \frac{c-p}{2} \cdot (Y_j - Y_U) + R_j,$$

where $R_j = (c-p) \cdot \max\{Y_{\text{base}} - Y_j, 0\} \cdot 1\{D_j = 1\}$. The first term represents the change in consumer surplus due to an increase in the price of electricity from p to c, while the second term represents the rebate received by reducing electricity consumption below the baseline consumption Y_{base}.

Then, given the Pareto weights w for a household, it is natural to define the weighted potential welfare contribution by

$$W_j = w \cdot CS_j + \Delta PS_j - a - R_j + \delta \cdot (Y_j - Y_U).$$

The welfare contribution is the sum of five terms. $w \cdot CS_j$ is the consumer surplus weighted with the Pareto weight. ΔPS_j is change of producer surplus, and hence we have $\Delta PS_j = (p-c)(Y_j - Y_U)$. The constant a denotes the cost taken to implement rebate program. Note that this cost does not include the rebate payment, which is instead reflected in $-R_j$. Finally, the term $\delta \cdot (Y_j - Y_U)$ is the long-run gain. Using the concrete expression of CS_j, we can rewrite the welfare contribution as follows

$$W_j = \left(\frac{(2-w)(p-c)}{2} + \delta \right) \cdot (Y_j - Y_U) - (1-w) \cdot R_j - a,$$

which is equation (2). Further, when $w = 1$, the welfare contribution boils down to

$$W_j = \left(\frac{p-c}{2} + \delta \right) (Y_j - Y_U) - a.$$

A.4 Welfare Maximization with Redistribution

In the main text, we presented the results based on the utilitarian welfare function. However, our method is not necessarily restricted to a conventional utilitarian framework. Rather, one can apply our method to any welfare function most appropriate for a policy goal. In this section, we shed light on this point by considering a policy goal that balances the equity-efficiency trade-off.

Table A.3 presents the redistribution implications of the optimal utilitarian policy. The efficiency gain

(551.4 in the table) is the ITT of the welfare gain of the optimal policy (the selection-driven targeting) based on the utilitarian welfare function. As presented in the main text, this targeting policy ($\hat{G}^*$) maximizes the utilitarian welfare gain compared to other policy options.

However, Table A.3 suggests that this policy may create a concern for equity. We compare the average rebate that would be distributed to consumers across the household income distribution. We find that the optimal targeting policy would distribute more rebates to higher income households. That is, although this targeting maximizes the efficiency gain from the policy, it may not be appealing to policymakers who weigh on redistribution implications.

To address this equity concern, we consider a social welfare function that balances the equity-efficiency trade-off by using a framework developed by Saez (2002) and used by Allcott, Lockwood, and Taubinsky (2019) and Lockwood (2020). Consider Pareto weights for a household: $w = h^{-\nu}$ where h is household income and ν is a scalar parameter that represents a policymaker's preference for redistribution. With this specification, a higher ν implies a stronger preference for redistribution, $\nu = \infty$ corresponds to the Rawlsian criterion, and $\nu = 0$ corresponds to utilitarianism. When $\nu = 1$, it approximately corresponds to the weight that would arise under logarithmic utility from income. We modify our utilitarian social welfare function by using this weight for each consumer's surplus from the policy intervention. In Appendix A.3, we show that the social welfare function with this Pareto weight can be written by:

$$W_j = \tilde{b} \cdot (Y_j - Y_U) - (1 - w)R_j - a \cdot 1\{D_j = 1\}, \tag{2}$$

for $j \in \{T, U, S\}$, where $\tilde{b} = \frac{2-w}{2}(p - c) + \delta$, $w = h^{-\nu}/H$ is the normalized weight, and H is the sum of $h^{-\nu}$ over all households. R_j is the potential amount of rebate for arm j and it is defined by $R_j = (c - p) \cdot \max\{Y_{\text{base}} - Y_j, 0\} \cdot 1\{D_j = 1\}$, where Y_{base} denotes the baseline consumption of rebate payment for the household. When $\nu = 0$, w is equal to 1 for all households, and hence, the equation (2) becomes the utilitarian welfare function.[4]

In Table A.3, we present the results with $\nu = 1$ and $\nu = 2$ below the result for the utilitarian welfare function, which is equivalent to the case with $\nu = 0$. We find that the welfare function with $\nu = 2$ is able to roughly equalize the average rebate distributed to households across the income distribution. However, as we expect, the efficiency gain (i.e., the welfare gain evaluated based on the utilitarian welfare function) is compromised as we increase the weight on the preference for redistribution.

[4]The potential amount of rebate R_j does not appear in the utilitarian weight because it is just a lump-sum transfer between consumers and producers. However, once we allow differential weight for each consumer's surplus, R_j is not cancelled out in the welfare function.

These results indicate that policymakers can choose the welfare function that most appropriately balances the equity-efficiency trade-off and apply our method to such welfare functions. We also present in Table A.4 that the selection-driven targeting maximizes the welfare function with all values of ν that are considered in our analysis.

A.5 Monte Carlo Simulations: Compare the Artificial Data Approach and Other Approaches

Sample splitting offers a simple way to construct unbiased estimator and asymptotically valid confidence intervals for welfare attained by a policy estimated by the training sample. However, sample splitting comes at a cost of precision loss in the estimation of both optimal policies and welfare. In addition, if our interest is to infer the welfare at a population optimal policy, an estimate by sample splitting underestimates the population maximal welfare. Our approach in the paper (the artificial data approach), in contrast, can utilize the whole sample for estimating an optimal policy. At the same time, we expect that it mitigates winner's bias than the naive approach of reporting the in-sample maximized empirical welfare. This is because in our approach we estimate the data generating process (from which we generate test samples) by optimizing the mean squared error criterion for predicting Y, which is quite different from the empirical welfare criterion that the policy tree is maximizing. This disagreement of the objective functions can significantly reduce the statistical dependence between the EWM policy and the test sample used to perform point-estimation and inference for the maximal welfare. We do not have an analytical claim but for this reason, it is expected to perform better than the naive approach in terms of winner's bias.

To assess this point, we perform Monte Carlo simulations to examine the statistical performance of our approach relative to sample splitting and the naive approach. Our specifications of data generating processes mimic the setting of our empirical analysis. We consider three treatment arms $A \in \{0, 1, 2\}$, which are randomly assigned according to $P(A = 0) = P(A = 1) = 2/5$ and $P(A = 2) = 1/5$. Covariate vector X has 10 independent variables $(X_1, X_2, \cdots, X_{10})$, among which the first two follow the uniform distribution on $[0, 1]$, and the other eight are Bernoulli random variables with parameter $1/2$. The observed outcome Y is generated according to $Y = \mu_A(X) + \epsilon$ with $\epsilon \sim N(0, \sigma^2)$, where $\mu_A(X)$ is the conditional expectation function $E[Y|A, X]$. To assess the robustness of this analysis, we report results with three different data generating processes (DGP). Our specifications resemble the Monte Carlo specifications in Zhou, Athey, and Wager (2023); let $f_1(x) = -\frac{(x_1-1)^2}{0.4^2} - \frac{(x_2-1)^2}{0.3^2} + 1$, $f_2(x) = -\frac{x_1^2}{0.6^2} - \frac{x_2^2}{0.4^2} + 1$, $\mu_0(x) = 0$, and

DGP 1 $\mu_1(x) = 2 \cdot 1\{f_1(x) \geq 0\} - 1$, and $\mu_2(x) = 2 \cdot 1\{f_2(x) \geq 0\} - 1$

DGP 2 $\mu_1(x) = 0.2 f_1(x)$ and $\mu_2(x) = 0.2 f_2(x)$

DGP 3 $\mu_1(x) = 0.03f_1(x)$ and $\mu_2(x) = 0.09f_2(x)$

The variance σ^2 is chosen so that it matches the ratio of the variance of regression fit to the residual variance in our empirical data. In DGP 1, the conditional mean functions are piece-wise constants with discontinuities at the margin of $CATE(x) = \mu_1(x) - \mu_0(x) = 0$. In contrast, DGPs 2 and 3 have continuous $CATE(x)$. DGP 3 has a thicker margin around $CATE(x) = 0$ than DGP 2, i.e., DGP 3 corresponds to a harder problem than DGP 2 in terms of classifying positive vs negative CATEs. We chose the multiplying constants in DGP 3 to replicate the distribution of the absolute value of the estimated conditional welfare gains in our empirical data.

For each DGP, we draw 500 samples of the size of 3,870. In each sample, we estimate the optimal 2-arm (selection-absent targeting) and 3-arm trees (selection-driven targeting) of depth 6 using the two-step procedure (see footnote 15 in the manuscript). Then, we construct the point estimates and 95% one-sided confidence intervals (CIs) for the welfare levels and the welfare gain of the 3-arm relative to the 2-arm trees. We compare three different ways to construct point estimates and CIs. The first is the naive method that treats the optimized empirical welfare contrast as an estimator for the population welfare gain and constructs CI by assuming the asymptotic normality of the point estimate. The second is the artificial test data approach implemented in our manuscript (see Section 4.2 of the manuscript for implementation details). The third is based on sample splitting in which we randomly split the full sample into training and test samples and use the training one to estimate an optimal tree and the test one to infer the welfare gain. We label each method as Naive, AT, and SS, respectively.

For each of these approaches, we assess the biases and standard errors of the point estimators and the cove rages of one-sided confidence intervals for the welfare levels and differences at the *population optimal policies*. In particular, for the main conclusion of our paper, it is of interest the coverage of CIs for the welfare difference between the selection-driven targeting policy (3-arm tree) and the selection-absent targeting policy (2-arm tree).

Table A.5 shows the performances of Naive, AT, and SS. We have three main findings. First, the naive method performs quite poorly; its point estimates are heavily upward-biased, and the coverage of CI is nearly zero due to winner's curse bias. Second, the AT performs much better than Naive, and the coverage probability does not deviate much from 95%, supporting our discussion given above. The SS has a larger downward bias than AT for the optimal welfare since what SS unbiasedly estimates is the welfare at a policy estimated in the training sample, which is lower than the population optimal welfare. In addition, due to a smaller size of the test sample, SS suffers from the precision losses compared with the other methods.

A.6 Appendix Tables

Table A.1: The external validity of the experimental sample

	Experimental sample in the untreated group	Random sample of population	Difference between sample and population
Monthly electricity usage in July (kWh)	356	304	51.86
	[205]	[177]	(6.34)
Number of people at home	2.48	2.31	0.17
	[1.24]	[1.20]	(0.04)
Self-efficacy in energy conservation (1-5 Likert scale)	3.45	3.32	0.13
	[0.85]	[0.97]	(0.03)
Household income (JPY10,000)	645	581	63.83
	[399]	[384]	(13.06)
Number of households	1,577	2,070	

Notes: We randomly sampled 2070 customers from the target population who did not participate in this experiment, and conducted a similar survey to the one for the experimental sample. The purpose of this survey was to investigate the external validity of our experimental sample by comparing the mean for each variable between the control group from our experimental sample and this random sample. Columns 1 and 2 show summary statistics for the untreated group and the random sample. Column 3 presents differences in means, with the standard errors of these differences in parentheses. We observe larger means for four variables in the untreated group than in the random sample, and the differences are statistically significant. Our experimental sample has larger pre-experiment electricity usage per month, a larger number of people at home on weekdays, higher self-efficacy in energy conservation, and higher household income. This implies that our sample includes a larger number of customers who are willing and able to reduce their electricity consumption, which should be taken into consideration when discussing this study's generalizability. The monetary unit is given as 1 ¢ = 1 JPY in the summer of 2020.

Table A.2: Intention-to-Treat Estimates

		Peak hour usage − Pre-peak hour usage (in pre-experiment)		Peak hour usage − Post-peak hour usage (in pre-experiment)	
	All	Low	High	Low	High
Treated group ($Z = T$)	-0.097	-0.108	-0.079	-0.089	-0.094
	(0.021)	(0.028)	(0.031)	(0.030)	(0.028)
Selection group ($Z = S$)	-0.052	-0.022	-0.073	-0.070	-0.023
	(0.027)	(0.034)	(0.041)	(0.037)	(0.037)
Number of customers	3,870	1,935	1,935	1,937	1,933
Number of observations	1,176,480	588,240	588,240	589,152	587,328
p-value (T = S)	0.088	0.013	0.880	0.595	0.047
Take-up rate in group S	37.2%	36.9%	37.4%	39.9%	34.7%

	Number of people at home		Self-efficacy		Household income	
	Low	High	Low	High	Low	High
Treated group ($Z = T$)	-0.096	-0.098	-0.134	-0.057	-0.071	-0.125
	(0.027)	(0.034)	(0.028)	(0.031)	(0.028)	(0.031)
Selection group ($Z = S$)	-0.022	-0.094	-0.036	-0.072	-0.036	-0.060
	(0.034)	(0.042)	(0.035)	(0.040)	(0.038)	(0.037)
Number of customers	2,245	1,625	1,967	1,903	2,036	1,834
Number of observations	682,480	494,000	597,968	578,512	618,944	557,536
p-value (T = S)	0.020	0.934	0.004	0.715	0.336	0.094
Take-up rate in group S	37.6%	36.6%	33.8%	40.6%	34.8%	39.7%

Notes: This table shows the estimation results for equation (1) using the full-sample (the first column of the upper panel) or sub-samples (the remaining columns). The dependent variable is the log of household-level electricity consumption over a 30-minute interval. We include household fixed effects and time fixed effects for each 30-minute interval. The standard errors are clustered at the household level to adjust for serial correlation. To investigate the heterogeneity of the treatment effects, we focused on the five variables selected for estimating the optimal policy in Section 4.2 and divided the sample into five sets of two sub-groups. For the five different variables, the first sub-group includes households who are below the median of this variable and the second includes those who are above the median. The monetary unit is given as 1 ¢ = 1 JPY in the summer of 2020.

Table A.3: Incorporating Equity-Efficiency Trade-off

	Efficiency gain	Average rebate by the quartiles of household income			
		[0%,25%]	(25%,50%]	(50%,75%]	(75%,100%]
Utilitarian ($\nu = 0$)	551.4	79.1	88.7	144.7	140.1
	(68.2)	(11.6)	(12.4)	(18.1)	(20.7)
With a redistribution goal ($\nu = 1$)	446.4	87.7	120.1	141.7	117.0
	(70.5)	(15.4)	(15.9)	(17.7)	(20.6)
With a redistribution goal ($\nu = 2$)	388.4	113.8	106.8	116.5	106.1
	(70.6)	(16.8)	(15.3)	(16.6)	(20.2)

Notes: The first column "Efficiency gain" shows the welfare gain from the policy measured by the utilitarian welfare function. Other columns present the average rebate amount in each of the quartile of the income distribution. The utilitarian policy maximizes the efficiency gain but its rebate distributions are regressive. In Section A.4, we consider a welfare function with a redistribution goal with a Pareto parameter ν. The policies with $\nu = 1$ and 2 reduce regressivity at the cost of sacrificing the efficiency gain. The monetary unit is given as 1 ¢ = 1 JPY in the summer of 2020.

Table A.4: Comparisons of Selection-absent and Selection-driven Targeting in Welfare Function with Redistributional Goal

	Difference in Welfare Gains	p-value
G^* vs. $G^\dagger$ (with $\nu = 1$)	145.1	0.012
	(64.4)	
G^* vs. $G^\dagger$ (with $\nu = 2$)	114.8	0.039
	(65.3)	

Notes: This table compares welfare gains with G^* (selection-driven targeting) and those with $G^\dagger$ (selection-absent targeting) with a redistributional goal. The column "Difference in Welfare Gains" shows the estimated welfare gain of the selection-driven targeting relative to selection-absent targeting in terms of welfare function weighted Pareto weight with parameter $\nu \in \{1, 2\}$, with the standard errors in parentheses. For each row, the column "Difference in Welfare Gains" shows the estimated welfare gain of the policy on the left-hand side (W_L) relative to the policy on the right-hand side (W_R) in JPY per household per season, with its standard error in parenthesis. The column "p-value" gives the p-value for the null hypothesis: $H_0 : W_L \geq W_R$. The monetary unit is given as 1 ¢ = 1 JPY in the summer of 2020.

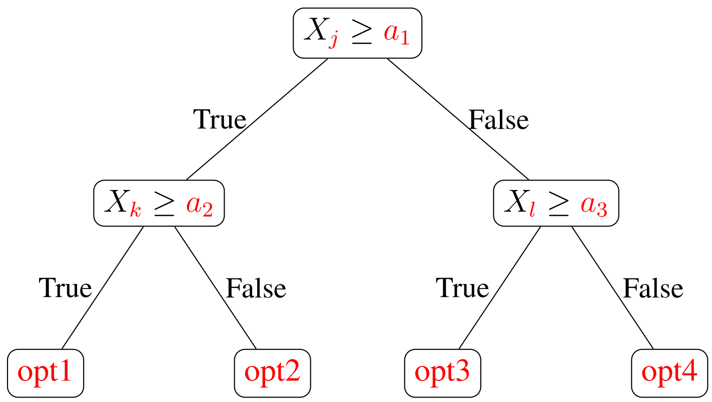
Table A.5: Comparison of the Three Different Ways to Construct Point Estimates and Confidence Intervals

	2-arm tree			3-arm tree			3-arm tree vs. 2-arm tree		
Method	Bias	Std. Err.	Coverage	Bias	Std. Err.	Coverage	Bias	Std. Err.	Coverage
DGP 1									
Naive	0.127	0.034	0.018	0.198	0.041	0.000	0.071	0.033	0.268
AT	-0.054	0.032	0.988	-0.123	0.038	1.000	-0.069	0.030	1.000
SS	-0.105	0.049	1.000	-0.192	0.058	1.000	-0.087	0.050	0.996
DGP 2									
Naive	0.180	0.035	0.000	0.338	0.041	0.000	0.158	0.035	0.000
AT	-0.031	0.034	0.953	-0.055	0.039	0.968	-0.023	0.033	0.965
SS	-0.077	0.051	1.000	-0.106	0.058	1.000	-0.028	0.053	0.975
DGP 3									
Naive	0.324	0.035	0.000	0.462	0.041	0.000	0.138	0.038	0.006
AT	-0.015	0.033	0.936	-0.025	0.038	0.939	-0.011	0.036	0.943
SS	-0.050	0.050	0.996	-0.081	0.058	1.000	-0.031	0.057	0.982

Notes: Naive, AT, and SS refer to the naive method, the method based on artificial test data, and the method based on sample splitting, respectively. The nominal coverage is 95%.

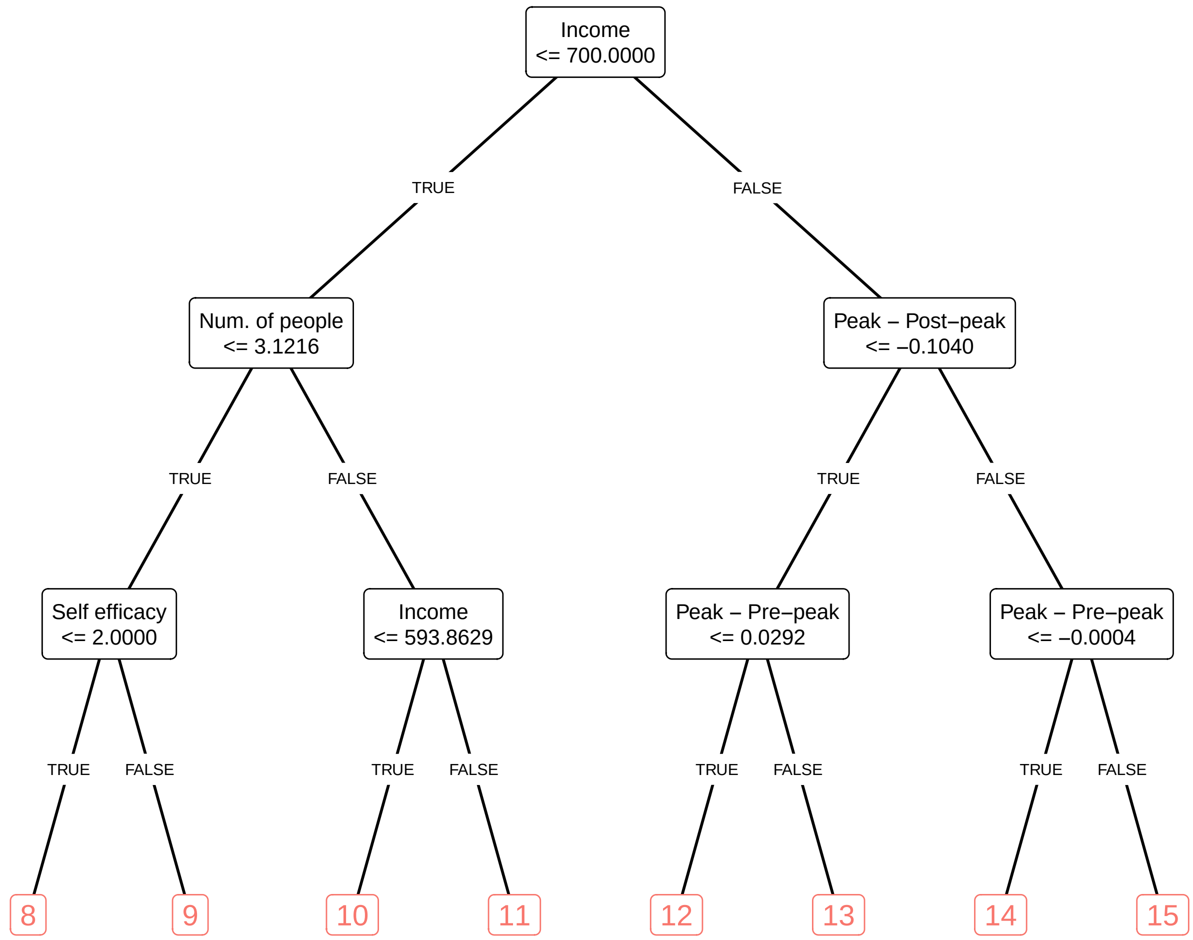
A.7 Appendix Figures

Figure A.1: Decision tree of depth 2



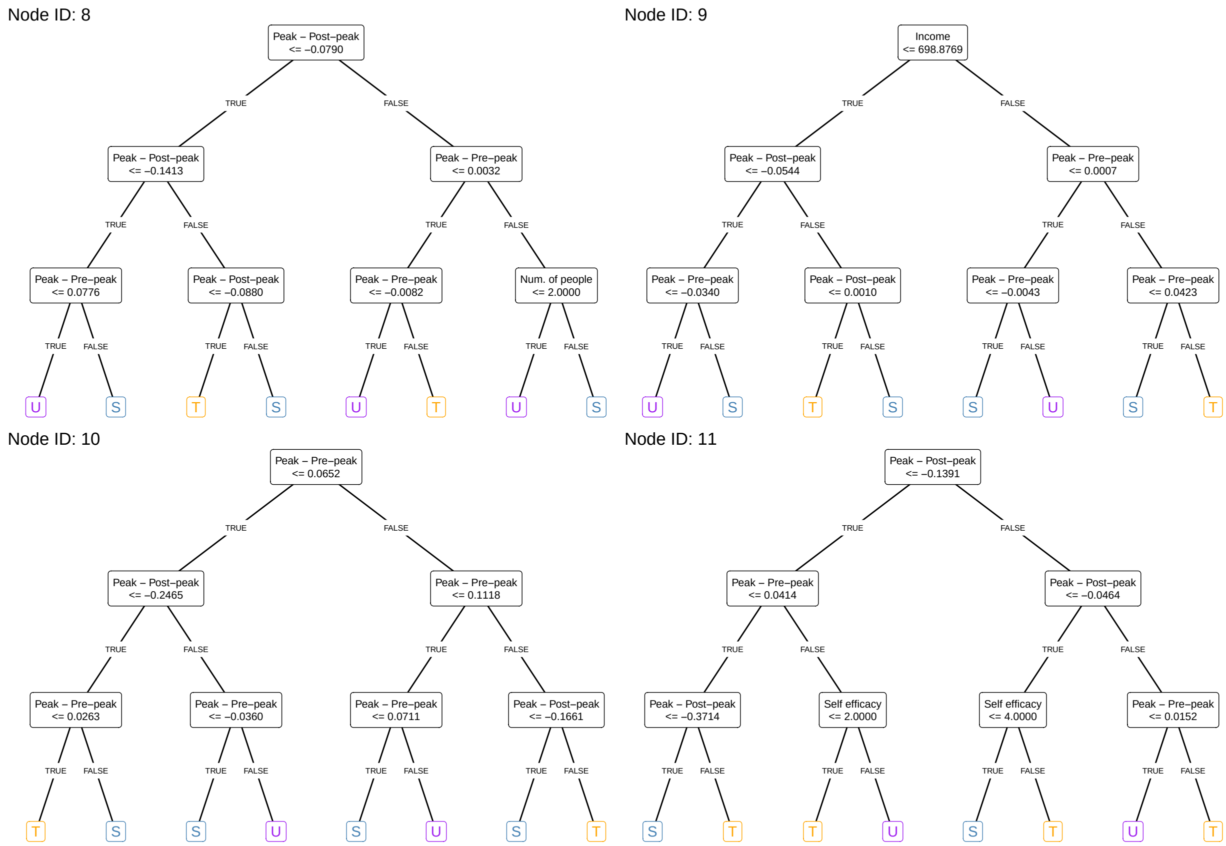
Notes: $(j, k, l) \in \{1, \ldots, K\}^3$, $(a_1, a_2, a_3) \in \mathbb{R}^3$, and $(\text{opt1}, \ldots, \text{opt4}) \in \{T, U, S\}^4$. Searching for the optimal decision tree of depth 2 is equivalent to finding the best combination of indices $(j, k, l) \in \{1, \ldots, K\}^3$ of X and threshold values $(a_1, a_2, a_3) \in \mathbb{R}^3$ in the top 2 layers, and options $(\text{opt1}, \ldots, \text{opt4}) \in \{T, U, S\}^4$ in the bottom layer.

Figure A.2: Optimal Assignment $\hat{G}^*$ (Panel A: From Depth 1 to 3)



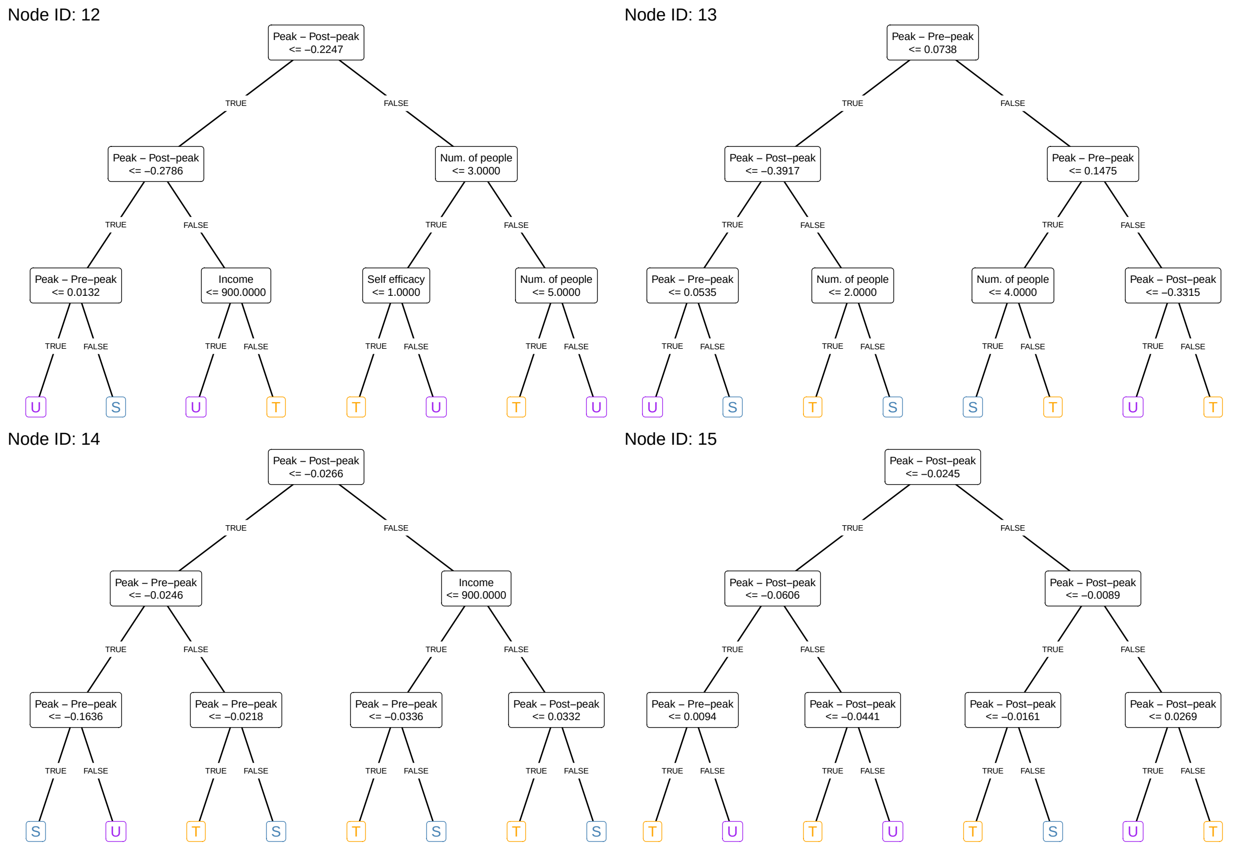
Notes: This figure presents the depth 1 to 3 of the optimal assignment $\hat{G}^*$. Each household answers yes-no questions from its top, and is given one number from 8 to 15. Households given a number less than or equal to 11 go to Panel B below. Other households go to Panel C below. The monetary unit is given as 1 ¢ = 1 JPY in the summer of 2020.

Figure A.2: Optimal Assignment $\hat{G}^*$ (Panel B: From Depth 4 to 6, Left Side)



Notes: This figure presents the left side of depth 4 to 6 of the optimal assignment $\hat{G}^*$. Each household given a number at most 11 in Panel A refers to tree with the same node id. Then, following the yes-no questions from its top, each household assigned to one of U, T, and S.

Figure A.2: Optimal Assignment $\hat{G}^*$ (Panel C: From Depth 4 to 6, Right Side)



Notes: This figure presents the right side of depth 4 to 6 of the optimal assignment $\hat{G}^*$. Each household given a number at least 12 in Panel A refers to tree with the same node id. Then, following the yes-no questions from its top, each household assigned to one of U, T, and S.

References

ALLCOTT, H., B. B. LOCKWOOD, AND D. TAUBINSKY (2019): "Regressive sin taxes, with an application to the optimal soda tax," *The Quarterly Journal of Economics*, 134, 1557–1626.

ITO, K., T. IDA, AND M. TAKANA (forthcoming): "Selection on Welfare Gains: Experimental Evidence from Electricity Plan Choice," *American Economic Review*.

LOCKWOOD, B. B. (2020): "Optimal income taxation with present bias," *American Economic Journal: Economic Policy*, 12, 298–327.

SAEZ, E. (2002): "Optimal income transfer programs: intensive versus extensive labor supply responses," *The Quarterly Journal of Economics*, 117, 1039–1073.

ZHOU, Z., S. ATHEY, AND S. WAGER (2023): "Offline Multi-Action Policy Learning: Generalization and Optimization," *Operations Research*, 71, 148–183.