

Incentive Design in Human-in-the-Loop Cyber-Physical Systems: A Case Study on Demand Response in California

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Abstract—We analyze a human-in-the-loop cyber-physical system in which a principal pays users to induce a desired behavior. Specifically, we estimate the causal effect of hour-ahead price interventions on the reduction of residential electricity consumption, using a large-scale Randomized Controlled Trial on 5,000 households in California. In addition to this *experimental* approach, we also develop a *non-experimental* estimation technique that allows for an estimation of the desired treatment effect on an *individual level* by estimating user-level counterfactuals using time-series prediction. This approach crucially eliminates the need for a randomized experiment, which in many settings is often hard to conduct due to financial and ethical constraints. Both approaches estimate a reduction of ≈ 0.13 kWh (14%) per Demand Response event and household. Since both estimates are close to each other, we claim that this methodology can be applied to predict treatment effects of any future Demand Response program. Using different incentive levels, we find a weak price elasticity of reduction. We also evaluate the effect of an adaptive targeting scheme, which discriminates users based on their estimated responses in order to increase the per-dollar reduction ratio by 43%. Lastly, we find that households with smart home automation devices reduce significantly more, namely about 0.33 kWh (38%).

I. INTRODUCTION

Cyber-Physical Systems (CPS) utilize intelligent mechanisms to improve the efficiency and usability of the interacting elements and are often augmented by human capabilities to achieve a desired goal. However, recent advances in CPS emerging from societal-scale infrastructure have been limited by the lack of good models for human interaction with systems. Significant efforts to introduce such models exist in transportation or energy, which is the case studied here. We study the effect of incentivizing people to participate in Demand Response to reduce electricity consumption with a Randomized Controlled Trial (RCT). While our application is specific, our techniques employed in this paper are generalizable to any societal-scale CPS which seeks to incentivize users to achieve a desired human behavior.

Following the 1970s energy crisis, programs for demand-side management (DSM) [4] were introduced on a global scale. Such programs seek to temporarily reduce consumers' electricity usage through financial incentive schemes during periods of electricity supply shortage. These programs are enabled by the integration of information and communications technology in the electric grid, which has inspired a large body of research aiming at better understanding the interaction between the consumption habits of consumers,

load-serving entities and the electric wholesale market. Energy supply is highly inelastic due to the slowness of power plants' output adjustment, which causes small increases and decreases in demand to result in a price boom or bust, respectively. Despite the fact that electric utilities and generating companies hedge against such price fluctuations through long-term contracts, a large portion of electricity remains to be procured through the wholesale electricity market. Since utilities are obligated to provide end-users with electricity at a quasi-fixed tariff at all times [1], e.g. Time-of-Use pricing, they have to bear price risks. Therefore, DSM is an attempt to protect utilities against such price risks by partially relaying them to end-users, which increases market efficiency according to the economic consensus [2], [3].

Previous studies in residential DSM have mainly focused on critical-peak pricing and real-time pricing, using *day-ahead* notifications that were sent often only during selected seasonal periods of the year [5], [6], [7]. In contrast, this paper focuses on extremely short-term, *hour-ahead* behavioral interventions when demand reductions can counteract high electricity prices [8]. Since electric wholesale markets tend to fluctuate more on an hour-ahead rather than day-ahead level, such *hour-ahead* notifications could have more leverage to deliver welfare effects compared to *day-ahead* interventions.

The California Public Utilities Commission (CPUC) has launched a Demand Response Auction Mechanism (DRAM) in July 2015 [9] which requires utilities to procure a minimum monthly amount of reduction capacity from Demand Response (DR) aggregators. The real-time market determines electricity prices by matching demand and utilities' supply curves subject to the procured capacity. A utility whose bid is cleared then asks the DR provider to incentivize its customers to temporarily reduce their consumption relative to their projected consumption without intervention. This is the counterfactual, referred to in this context as *baseline*, based on which compensations for (non-)fulfilled reductions are settled: If the consumer uses less (more) energy than the baseline, she receives a reward (incurs a penalty). In a similar fashion, if the aggregator falls short of delivering the promised load reduction, it incurs a penalty.

The estimation of the actually delivered reduction arguably is the most critical component of the DR bidding process. If the reductions are estimated with a biased counterfactual, either the DR provider or the utility clearing the bids is systematically discriminated against. If the baseline is unbiased but plagued by high variance, the profit settlement is highly volatile. Existing baselines employed by major power grid operators in the United States (e.g. California Independent

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System Operator (CAISO), New York ISO) are calculated with simple arithmetic averages of previous observations [10] and therefore are inaccurate. Estimating more accurate baselines with non-experimental, unbiased estimators on the one hand and an experimental control group within a RCT on the other hand, is a significant contribution of this paper.

A. Contributions

We estimate the *average treatment effect* (ATE) of hour-ahead notifications on the reduction of electricity consumption by evaluating a Randomized Controlled Trial (RCT) on ≈ 5000 residential households in California serviced by the three main electric utilities (PG&E, SDG&E, SCE). This experiment is funded by the California Energy Commission, which – to the best of our knowledge – is the first one to experiment with hour-ahead notifications on a residential household level. We estimate an ATE of -0.13 kWh per DR Event and user and further discover notable geographic and temporal heterogeneity among users, as the largest estimated reductions occur in summer months as well as in regions with warmer climate, suggesting that air conditioning units play a decisive role in DR programs.

In addition to this experimental approach, we also develop a *non-experimental method* for estimating this causal effect on an *individual* user level, which is easily aggregated into an ATE. Importantly, we utilize experimental observations and the existence of a control group as a benchmark for the non-experimental ATE estimate and find that the results in both cases are close to each other. Interestingly, the non-experimental approach even achieves tighter confidence intervals of the estimated causal effect. Motivated by these results, we claim that our method is applicable to estimating treatment effects in any setting with high-frequency time-series data whenever an RCT is hard to conduct, for example due to budget or ethical constraints.

Furthermore, we design an *adaptive targeting* method, which exploits heterogeneity in users’ responses to incentive signals to assign differing price levels to different subsets of the treatment population. Specifically, we separate users based on their previous responses into two distinct groups, each of which either only receives low or high incentives. Using this partitioning method, we observe an increase of the per-dollar yield of $\approx 43\%$.

This paper unfolds as follows: In Section II, we describe the market setting for Residential Demand Response. Section III explains the experimental setup and provides summary statistics on the RCT data. We then develop the non-experimental estimation framework in Section IV, where we pay particular attention to estimation bias and empirical de-biasing methods (Section IV-C). The non-experimental estimation results are provided in Section V. Next, in Section VI we estimate the ATE using a classical Fixed-Effects Estimator from econometrics [11]. By using varying regression specifications, we estimate the demand curve of Demand Response (Section VI-A) and conditional ATEs by hour of the day (Section VI-B), month of the year (Section VI-C), or smart home automation status of users (Section VI-D).

Section VII compares the estimates obtained by both approaches. Lastly, the effect of adaptive targeting is discussed in Section VIII. Section IX concludes. Supplemental figures and numeric data are relegated to the Appendix.

B. Related Work

Given the rapid growth of collected user data, non-experimental estimates become more and more valuable. Moreover, there are situations where RCTs, the experimental standard, are infeasible to conduct, e.g. due to budget or ethical constraints. These facts have spurred research at the intersection of machine learning and economics, whose general idea is to partition observations under treatment and control in order to fit a nominal model on the latter set, which, when applied on the treatment set, yields the treatment effect of interest. Examples for such nominal models are found in [12], who evaluates welfare effects of home automation by calculating the Kolmogorov-Smirnov Statistic between users, which are then used as weights for kernel-based non-parametric regression. In [13], a convex combination of US states is computed as the counterfactual estimate for tobacco consumption to estimate the effect of a tobacco control program in California on tobacco consumption. In [14], [15], the estimators are random forests trained by recursive partitioning of the feature space and novel cross-validation criteria. [16] develops Bayesian structural time series models combined with a Monte-Carlo sampling method for treatment effect inference of market interventions.

Fitting an estimator on smart meter time-series is essentially a short-term load forecasting (STLF) problem, whose goal is to fit estimators on observed data to predict future consumption with the highest possible accuracy. Within STLF, tools employed are ARIMA models with a seasonal component [17] and classic regression models where support vector regression (SVR) and neural networks yield the highest accuracy [18], [19]. A comprehensive comparison between ML techniques for forecasting and differing levels of load aggregation is provided in [20].

In the context of smart meter data mining, much of the existing work focuses on disaggregation of energy consumption to identify contributions of discrete appliances from the total observed consumption [21] and to learn consumption patterns [22], [23]. Studies in applied economics typically emphasize the estimation of ATEs of experimental interventions. To increase precision of the estimates, the employed regression models often employ unit-level fixed effects [7], [24], which is an implicit way of training models for the consumption of individual consumers. In this work, we make these user-level models explicit, allowing for more general ML techniques. Importantly, our approach is original as it permits to perform causal inference on the level of *individual* treatment effects in a straightforward fashion by employing estimators from STLF. To the best of our knowledge, this paper is the first of its kind to analyze the potential of Demand Response interventions on a residential level, combining ideas at the intersection of causal inference from econometrics and Machine Learning for estimation.

II. DEMAND RESPONSE MECHANISM

According to DRAM [9], electric utilities are obligated to offer “demand flexibility” through Demand Response Providers (DRPs). Utilities solicit bids from DRPs and accept the highest ones up to a monthly target capacity. In the real-time wholesale electricity market, the utility submits supply bids including these acquired capacities, which, when cleared, have to be delivered by the DRP under contract over a contractually specified period of time. The DRP does so by eliciting reductions among a suitably chosen subset of its residential end-use customers by offering them a monetary incentive. Such an aggregation of users is also known as a *Proxy Demand Resource* (PDR) [10] product. The DRP receives a payment from the wholesale market for each unit of reduction up to its original capacity bid, but incurs a shortfall penalty for each unit of unfulfilled obligation. Figure 1 illustrates the interaction between all agents.

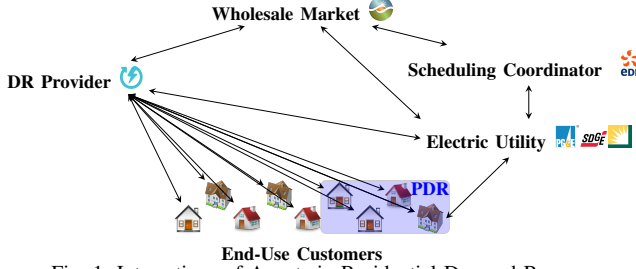


Fig. 1: Interactions of Agents in Residential Demand Response

We focus on the DRP-User interaction and in particular answer the question of how to measure and quantify reductions of end-users’ electricity consumption in response to monetary incentives. The regulatory standard in California measures reductions with the CAISO 10-in-10 baseline [10], which computes the estimated reduction for a particular user at a particular time. During a DR event, the materialized consumption is compared to the estimated baseline. The user is rewarded (penalized) the difference between those two multiplied with a reward level. Due to the inherent noisiness of the CAISO baseline, we present alternative approaches to estimate reductions (namely the individual level non-experimental estimates and the fixed effects regression, which implicitly uses the control group as a counterfactual).

Our data set consists of DR events of length one hour. Specifically, users receive notifications of a DR event up to 20 minutes into an hour, which lasts until the end of the hour. The notifications sent during the RCT communicate the incentive level to the user, which was chosen from the set $\mathcal{R} = \{0.05, 0.10, 0.25, 1.00, 3.00\} \frac{\text{USD}}{\text{kWh}}$.

III. EXPERIMENTAL SETUP AND DATA CHARACTERISTICS

A. Setup of the Experiment

The experiment is carried out by OhmConnect, Inc., using funds provided by the California Energy Commission. Figure 2 draws a flowchart of the experimental setup.

Over the course of the experimental time period (Nov. 2016 - Dec. 2017), each consumer that signs up for the study is *randomly* assigned to one of the following groups:

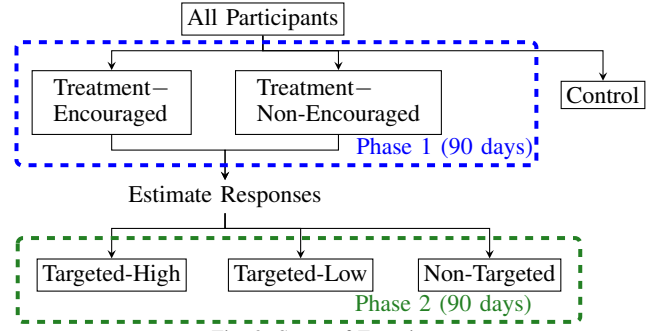


Fig. 2: Setup of Experiment

- **Treatment-Encouraged:** The user receives an average number of 25 DR events in the 90 days following the signup, with incentive levels being randomly chosen from the set $\mathcal{R} = \{0.05, 0.25, 0.50, 1.00, 3.00\} \frac{\text{USD}}{\text{kWh}}$. Additionally, the user is given a rebate for purchasing a smart home automation device.
- **Treatment-Non-Encouraged:** Same as in Treatment-Encouraged, but without smart home automation rebate.
- **Control:** Users do not receive any notifications for DR events for a period of 90 days after signup.

Taken together, these three groups form Phase 1 of the experiment. Users in the control group that have reached 90 days of age are removed from the study. Users in either the Treatment-Encouraged or Treatment-Non-Encouraged groups that have reached 90 days of age are pooled and systematically assigned to one of the following groups for Phase 2 interventions:

- **Targeted-High:** The user receives an average number of 25 DR events for a period of 90 days after being rolled over into Phase 2. Each reward level is randomly drawn from the set $\{1.00, 3.00\} \frac{\text{USD}}{\text{kWh}}$.
- **Targeted-Low:** Same as in Targeted-High, but rewards are randomly drawn from $\{0.05, 0.25, 0.50\} \frac{\text{USD}}{\text{kWh}}$.
- **Non-Targeted:** Same as in targeted groups, with rewards drawn from $\{0.05, 0.25, 0.50, 1.00, 3.00\} \frac{\text{USD}}{\text{kWh}}$.

Users who have completed Phase 2 are then removed from the study. In Sections IV-VII, we evaluate Phase 1 of the experiment whereas Section VIII is dedicated to adaptive targeting (Phase 2). In the remainder of this paper, we use the term “treatment users” to refer to users in the “Treatment-Encouraged” and “Treatment-Non-Encouraged” group.

B. Summary Statistics

Table I reports the number of users by experiment group and proportion of users for which we were able to scrape historical smart meter reading data (as of September 28, 2017). The table shows that the randomized assignment of users to groups roughly follows a 1:2:2 ratio (Control vs. Treatment-Encouraged vs. Treatment-Non-Encouraged).

Historical Smart Meter Data Availability by Group			
Group	# Enrolled	# With Data	# With DR
Control	3266	1311	—
Treatment-Encouraged	6735	2873	2389
Treatment-Non-Enc.	6689	2910	2402

TABLE I: Number of Total Users Enrolled by Group, Data Availability, and Users with DR Events as of September 28, 2017.

Users without DR events or for which we were unable to scrape historical data are omitted from the study. Since the assignment of users into the different experimental groups was randomized (see Section III-D), dropping such users does not affect the evaluation of the experiment.

Figure 3 shows the geographic distribution of the remaining users across California. More than half of all study participants are serviced by Pacific Gas & Electric. The remaining users are covered by Southern California Edison (Los Angeles area) and San Diego Gas & Electric.

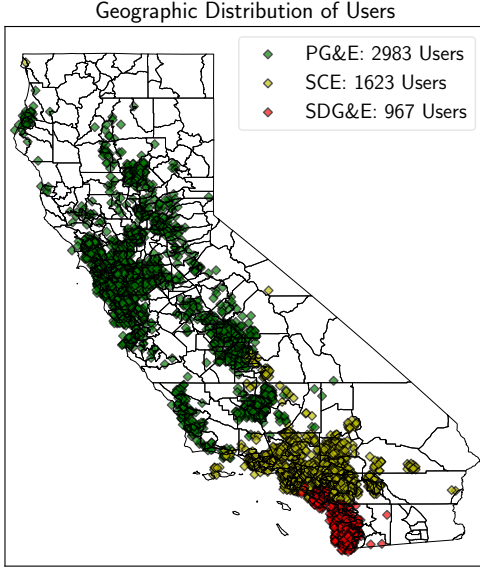


Fig. 3: Geographic Distribution of Users

C. Weather Data

Hourly measurements of ambient air temperature are scraped from the publicly accessible California Irrigation Management Information System [25]. As there are fewer weather stations than distinct user ZIP codes, we linearly interpolate user-specific temperatures at their ZIP codes from the two closest weather stations in latitude and longitude by calculating geodesic distances with Vincenty's formulae [26].

D. Balance Checks

To verify that users were randomly assigned to control and treatment groups, we perform a balance check on the distribution of observed air temperatures and electricity consumptions across both groups. Notice that the relatively large sample size renders a classical differences-in-means t -test inappropriate. Therefore, we utilize Cohen's d to estimate the effect size based on the differences between means, which is insensitive to the large sample size. Given two discrete distributions P and Q with sample sizes n_1/n_2 and means \bar{x}_1/\bar{x}_2 , Cohen's d is defined as

$$d = \frac{\bar{x}_1 - \bar{x}_2}{s}, \quad s = \left(\frac{(n_1 - 1)s_1^2 + (n_2 - 1)s_2^2}{n_1 + n_2 - 2} \right)^{1/2}, \quad (1)$$

where s_1 and s_2 are the sample standard deviations for distributions P and Q , respectively. In addition, we use the Hellinger Distance H as a nonparametric comparison

to quantify the similarity between the distributions [27]:

$$H(P, Q) = \frac{1}{\sqrt{2}} \left(\sum_{i=1}^k (\sqrt{p_i} - \sqrt{q_i})^2 \right)^{1/2} \quad (2)$$

where $P = \{p_1, \dots, p_k\}$ and $Q = \{q_1, \dots, q_k\}$. To compute (1) and (2), we discretize the temperature and consumption distributions appropriately. Table VII in the Appendix provides these metrics together with the differences in means for a selected subset of hours of the day, which was chosen to coincide with those hours of the day for which DR events were observed (see Figure 10). We omit the metrics for the remaining hours of the day as they are very similar to the listed ones. As the Hellinger Distance $H \in [0, 1]$, with 0 corresponding to a perfect similarity and 1 to total dissimilarity, we can assume that the assignment of users into treatment and control group is as good as random.

IV. NONEXPERIMENTAL TREATMENT EFFECT ESTIMATION

A. Potential Outcomes Framework

To estimate the effect of the DR intervention program, we adopt the *potential outcomes* framework introduced by Rubin (1974) [28]. Let $\mathcal{I} = \{1, \dots, n\}$ denote the set of users. The indicator $D_{it} \in \{0, 1\}$ encodes the fact whether or not user i received DR treatment at time t . Each user is endowed with a consumption time series $\mathbf{y}_i = \{y_{i1}, \dots, y_{i\tau}\}$ and associated covariates $\mathbf{X}_i = \{\mathbf{x}_{i1}, \dots, \mathbf{x}_{i\tau}\} \in \times_{i=1}^{\tau} \mathcal{X}_i$, $\mathcal{X}_i \subset \mathbb{R}^{n_x}$, where time is indexed by $t \in \mathbb{T} = \{1, \dots, \tau\}$ and n_x is the dimension of the covariate space \mathcal{X}_i . Let y_{it}^0 and y_{it}^1 denote user i 's electricity consumption at time t for $D_{it} = 0$ and $D_{it} = 1$, respectively. Let \mathcal{C}_i and \mathcal{T}_i denote the set of control and treatment times for user i . That is,

$$\mathcal{C}_i = \{t \in \mathbb{T} \mid D_{it} = 0\}, \quad \mathcal{T}_i = \{t \in \mathbb{T} \mid D_{it} = 1\}. \quad (3)$$

The number of treatment hours is much smaller than the number of non-treatment hours. Thus $0 < |\mathcal{T}_i|/|\mathcal{C}_i| \ll 1$.

Further, let $\mathcal{D}_{i,t}$ and $\mathcal{D}_{i,c}$ denote user i 's covariate-outcome pairs of treatment and control times, respectively. That is,

$$\mathcal{D}_{i,t} = \{(\mathbf{x}_{it}, y_{it}) \mid t \in \mathcal{T}_i\}, \quad \mathcal{D}_{i,c} = \{(\mathbf{x}_{it}, y_{it}) \mid t \in \mathcal{C}_i\}. \quad (4)$$

The one-sample estimate of the treatment effect on user i at time t , given the covariates $\mathbf{x}_{it} \in \mathbb{R}^{n_x}$, is

$$\beta_{it}(\mathbf{x}_{it}) := y_{it}^1(\mathbf{x}_{it}) - y_{it}^0(\mathbf{x}_{it}) \quad \forall i \in \mathcal{I}, t \in \mathbb{T}, \quad (5)$$

which varies across time, the covariate space, and the user population. Marginalizing this one-sample estimate over the set of treatment times \mathcal{T}_i and the covariate space \mathcal{X}_i yields the user-specific Individual Treatment Effect (ITE) β_i

$$\beta_i := \mathbb{E}_{\mathcal{X}_i} \mathbb{E}_{t \in \mathcal{T}_i} [y_{it}^1 - y_{it}^0 \mid \mathbf{x}_{it}] = \frac{1}{|\mathcal{T}_i|} \sum_{t \in \mathcal{T}_i} y_{it}^1 - y_{it}^0. \quad (6)$$

The average treatment effect on the treated (ATT) follows from (6):

$$\text{ATT} := \mathbb{E}_{i \in \mathcal{I}} [\beta_i] = \frac{1}{|\mathcal{I}|} \sum_{i \in \mathcal{I}} \frac{1}{|\mathcal{T}_i|} \sum_{t \in \mathcal{T}_i} (y_{it}^1 - y_{it}^0). \quad (7)$$

Since users were put into different experimental groups in a *randomized* fashion, the ATT and the average treatment effect (ATE) are identical [29]. Lastly, the conditional average treatment effect (CATE) on $\tilde{\mathbf{x}}$ is obtained by marginalizing the conditional distribution of one-sample estimates (5) on $\tilde{\mathbf{x}}$ over all users and treatment times, where $\tilde{\mathbf{x}} \in \mathbb{R}^{\tilde{n}_x}$ is a subvector of $\mathbf{x} \in \mathbb{R}^{n_x}$, $0 < \tilde{n}_x < n_x$:

$$\text{CATE}(\tilde{\mathbf{x}}) := \mathbb{E}_{i \in \mathcal{I}} \mathbb{E}_{t \in \mathcal{T}_i} \left[(y_{it}^1 - y_{it}^0) \mid \tilde{\mathbf{x}}_{it} = \tilde{\mathbf{x}} \right]. \quad (8)$$

The CATE captures heterogeneity among users, e.g. with respect to specific hours of the day, the geographic distribution of users, the extent to which a user possesses “smart home” appliances, group or peer effects, etc. To rule out the existence of unobserved factors that could influence the assignment mechanism generating the complete observed data set $\{(\mathbf{x}_{it}, y_{it}, D_{it}) \mid i \in \mathcal{I}, t \in \mathbb{T}\}$, we make the following standard assumptions:

Assumption 1 (Unconfoundedness of Treatment Assignment). *Given the covariates $\{\mathbf{x}_{it}\}_{t \in \mathbb{T}}$, the potential outcomes are independent of treatment assignment:*

$$(y_{it}^0, y_{it}^1) \perp D_{it} \mid \mathbf{x}_{it} \quad \forall i \in \mathcal{I}, t \in \mathbb{T}. \quad (9)$$

Assumption 2 (Stationarity of Potential Outcomes). *Given the covariates $\{\mathbf{x}_{it}\}_{t \in \mathbb{T}}$, the potential outcomes are independent of time, that is,*

$$(y_{it}^0, y_{it}^1) \perp t \mid \mathbf{x}_{it} \quad \forall i \in \mathcal{I}, t \in \mathbb{T}. \quad (10)$$

Assumption 1 is the “ignorable treatment assignment” assumption introduced by Rosenbaum and Rubin [30]. Under this assumption, the assignment of DR treatment to users is implemented in a *randomized* fashion, which allows the calculation of unbiased ATEs (7) and CATEs (8). Assumption 2, motivated by the time-series nature of the observational data, ensures that the set of observable covariates $\{\mathbf{x}_{it} \mid t \in \mathbb{T}\}$ can capture seasonality effects in the estimation of the potential outcomes. That is, the conditional distribution of the potential outcomes, given covariates, remains constant.

The *fundamental problem of causal inference* [31] refers to the fact that either the treatment or the control outcome can be observed, but never both (granted there are no missing observations). That is,

$$y_{it} = y_{it}^0 + D_{it} \cdot (y_{it}^1 - y_{it}^0) \quad \forall t \in \mathbb{T}. \quad (11)$$

Thus, the ITE (6) is not identified, because one and only one of both potential outcomes is observed, namely $\{y_{it}^1 \mid t \in \mathcal{T}_i\}$ for the treatment times and $\{y_{it}^0 \mid t \in \mathcal{C}_i\}$ for the control times. It therefore becomes necessary to estimate counterfactuals.

B. Non-Experimental Estimation of Counterfactuals

Consider the following model for the estimation of such counterfactuals:

$$y_{it} = f_i(\mathbf{x}_{it}) + D_{it} \cdot \beta_{it}(\mathbf{x}_{it}) + \varepsilon_{it}, \quad (12)$$

where ε_{it} denotes noise uncorrelated with covariates and treatment assignment. $f_i(\cdot) : \mathbb{R}^{n_x} \mapsto \mathbb{R}$ is the conditional

mean function and pertains to $D_{it} = 0$. To obtain an estimate for $f_i(\cdot)$, denoted with $\hat{f}_i(\cdot)$, control outcomes $\{y_{it}^0 \mid t \in \mathcal{C}_i\}$ are first regressed on $\{\mathbf{x}_{it} \mid t \in \mathcal{C}_i\}$, namely their observable covariates. In a second step, the counterfactual \hat{y}_{it}^0 for any $t \in \mathcal{T}_i$ can be estimated by evaluating $\hat{f}_i(\cdot)$ on its associated covariate vector \mathbf{x}_{it} . Finally, subtracting \hat{y}_{it}^0 from y_{it}^1 isolates the one-sample estimate $\beta_{it}(\mathbf{x}_{it})$, from which the user-specific ITE (6) can be estimated. Figure 4 illustrates this process of estimating the reduction during a DR event by subtracting the actual consumption y_{it}^1 from the predicted counterfactual $\hat{y}_{it}^0 = \hat{f}_i(\mathbf{x}_{it})$. Despite the fact that consumption can be predicted for horizons longer than a single hour, we restrict our estimators $f_i(\cdot)$ to a single hour prediction horizon as DR events are at most one hour long.

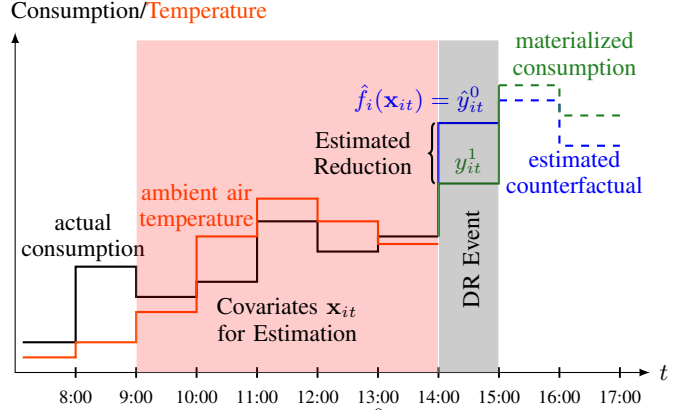


Fig. 4: Estimation of the Counterfactual \hat{y}_{it}^0 using Treatment Covariates \mathbf{x}_{it} and Predicted Reduction $\hat{y}_{it}^0 - y_{it}^1$

To estimate $f_i(\cdot)$, we use the following classical regression methods [32], referred to as *estimators*:

- E1:** Ordinary Least Squares Regression (OLS)
- E2:** L1 Regularized (LASSO) Linear Regression (L1)
- E3:** L2 Regularized (Ridge) Linear Regression (L2)
- E4:** k -Nearest Neighbors Regression (KNN)
- E5:** Decision Tree Regression (DT)
- E6:** Random Forest Regression (RF)

DT (E5) and RF (E6) follow the procedure of Classification and Regression Trees [33]. We compare estimators (E1)-(E6) to the CAISO 10-in-10 Baseline (BL) [10], which, for any given hour on a weekday, is calculated as the mean of the hourly consumptions on the 10 most recent business days during the selected hour. For weekend days and holidays, the mean of the 4 most recent observations is calculated. This BL is further adjusted with a *Load Point Adjustment*, which corrects the BL by a factor proportional to the consumption three hours prior to a DR event [10].

Since users tend to exhibit a temporary increase in consumption in the hours following the DR intervention [4], we remove $n_r = 8$ hourly observations following each DR event in order to prevent estimators (E1)-(E6) from learning from such spillover effects. This process is illustrated in Figure 5.

Hence the training data $\mathcal{D}_{i,tr} \subset \mathcal{D}_{i,t}$ used to estimate the conditional mean function $\hat{f}_i(\cdot)$ (12) consists of all observations leading up to a DR event, excluding those that are within 8 hours of any DR event. To estimate user i 's

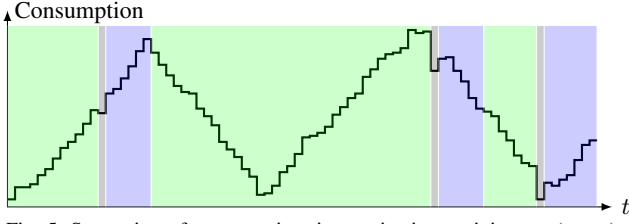


Fig. 5: Separation of consumption time series into training set (green), DR Events (grey), and Periods of Spillover Effects (blue)

counterfactual outcome \hat{y}_{it}^0 during a DR event $t \in \mathcal{T}_i$, we use the following covariates:

- 5 hourly consumption values preceding time t
- Air temperature at time t and 4 preceding measurements
- Hour of the day, an indicator variable for (non-)business days, and month of the year as categorical variables

Thus, the covariate vector writes

$$\mathbf{x}_{it} = [y_{it-1}^0 \quad \dots \quad y_{it-5}^0 \quad T_{it} \quad \dots \quad T_{it-4} \quad \text{C(HoD}_{it}) : \text{C(is_Bday}_{it}) \quad \text{C(MoY}_{it})]. \quad (13)$$

In (13), T_{it} denotes temperature, HoD_{it} hour of day, is_Bday_{it} an indicator variable for business days, and MoY_{it} the month of year (all for user i at time t). “C” denotes dummy variables and “:” their interaction.

C. Placebo Treatments and De-biasing of Estimators

As previously mentioned, a crucial element of an estimator is unbiasedness. If an estimator systematically predicts counterfactuals that are too large (small), users receive an excess reward (are paid less) proportional to the amount of prediction bias. For a fair economic settlement, it is thus desirable to minimize the amount of bias. In our application, such prediction bias is caused by the following two factors:

- Inherent bias of estimators: With the exception of OLS (E1), (E2)-(E6) are inherently biased, which is justified due to the well-known bias-variance tradeoff.
- Seasonal and temporal bias: Due to the experimental design, DR events for a particular user are concentrated within a period of 180 days after signing up. Further, DR events are called only in the afternoon and early evening (see Figure 10). Thus, fitting an estimator on all available historical data is likely to introduce bias during these time periods of interest.

To deal with these challenges, we use the de-biasing procedure presented in Algorithm 1, which was first introduced in [34]. We first separate a subset of non-DR events from user i ’s control data $\mathcal{D}_{i,c}$, which we call the placebo set $\mathcal{D}_{i,pl}$ with associated placebo treatment times $\mathcal{C}_{i,pl}$ (we chose $\mathcal{D}_{i,pl}$ to be of size 25). This placebo set is drawn according to user i ’s empirical distribution of Phase 1 DR events by hour of day and month of year. Next, the non-experimental estimator of choice is fitted (using cross-validation to find hyperparameters to minimize the mean squared prediction error) on the training set $\mathcal{D}_{i,tr}$. Importantly, to account for temporal bias, we assign weights to the training samples, ensuring that samples in “similar” hours or seasons as actual DR events are assigned larger weights. Specifically, the

Algorithm 1 Unbiased Estimation of Counterfactuals

Input: Treatment data $\mathcal{D}_{i,t}$, control data $\mathcal{D}_{i,c}$, Estimator, η

- 1: Split $\mathcal{D}_{i,c}$ into training data $\mathcal{D}_{i,tr}$ and placebo data $\mathcal{D}_{i,pl}$ according to empirical distribution of \mathcal{T}_i . Split control times \mathcal{C}_i into training times $\mathcal{C}_{i,tr}$ and placebo times $\mathcal{C}_{i,pl}$
- 2: Compute weights for $\mathcal{D}_{i,tr} = \{(x_{it}, y_{it}) \mid t \in \mathcal{C}_{i,tr}\}$ according to (15a)-(15c), using η
- 3: Fit conditional mean function \hat{f}_i on $\mathcal{D}_{i,tr}$ with weights
- 4: Estimate placebo counterfactuals $\{\hat{y}_{it}^0 \mid t \in \mathcal{C}_{i,pl}\}$
- 5: Compute bias on placebo treatment set
- 6: Estimate treatment counterfactuals $\{\hat{y}_{it}^0 \mid t \in \mathcal{T}_i\}$
- 7: Subtract placebo treatment bias from estimated treatment counterfactuals:

$$\hat{y}_{it}^0 \leftarrow \hat{y}_{it}^0 - \frac{1}{|\mathcal{C}_{i,pl}|} \sum_{\tau \in \mathcal{C}_{i,pl}} (\hat{y}_{i\tau}^0 - y_{i\tau}^0) \quad \forall t \in \mathcal{T}_i \quad (14)$$

weights w_{it} are determined as follows:

$$w_{it} \propto w_{it}^{\text{HoD}} w_{it}^{\text{MoY}}, \quad (15a)$$

$$w_{it}^{\text{HoD}} = \eta + \sum_{\tau \in \mathcal{C}_{i,tr}} \mathbf{1}(\text{HoD}_{it} = \text{HoD}_{i\tau}), \quad (15b)$$

$$w_{it}^{\text{MoY}} = \eta + \sum_{\tau \in \mathcal{C}_{i,tr}} \mathbf{1}(\text{MoY}_{it} = \text{MoY}_{i\tau}), \quad (15c)$$

where $\eta > 0$ is a constant to be chosen a-priori.

Then, the fitted model is used to predict counterfactuals associated with placebo events. This yields a set of $|\mathcal{C}_{i,pl}|$ paired samples from which we can obtain a proxy of the estimation bias that remains even after assigning sample weights according to the previous step. Finally, to obtain an empirically de-biased estimate of actual Phase 1 DR events, we simply subtract this proxy of the estimation bias from predicted Phase 1 DR event outcomes.

Due to space constraints, we omit the cross-validation metrics for different choices of η , as well as metrics showing that this two-step de-biasing operation yields smaller variances in the predictions compared to a one-step procedure that only utilizes steps 4-7 from Algorithm 1.

D. Estimation of Individual Treatment Effects

To obtain point estimates for user i ’s ITE β_i , we simply average all one-sample estimates (5) according to (6). To obtain an estimate of whether or not a given user i has actually reduced consumption, we utilize a nonparametric permutation test with the null hypothesis of a zero ITE:

$$H_0 : \beta_i = 0, \quad H_1 : \beta_i \neq 0. \quad (16)$$

Given user i ’s paired samples $\{z_{it} = \hat{y}_{it}^0 - y_{it}^1 \mid t \in \mathcal{T}_i\}$ during DR periods, the p -value associated with H_0 (16) is

$$p = \frac{\sum_{D \in \mathcal{P}_i} \mathbf{1}(\bar{D} \leq \hat{\beta}_i)}{2^{|\mathcal{T}_i|}}. \quad (17)$$

In (17), \bar{D} denotes the mean of D . \mathcal{P}_i denotes the set of all possible assignments of signs to the pairwise differences in the set $\{z_{it} = y_{it}^1 - \hat{y}_{it}^0 \mid t \in \mathcal{T}_i\}$. That is,

$$\mathcal{P}_i = \{s_1 z_{i1}, \dots, s_{|\mathcal{T}_i|} z_{i|\mathcal{T}_i|} \mid s_j \in \{\pm 1\}, 1 \leq j \leq |\mathcal{T}_i|\} \quad (18)$$

which is of size $2^{|\mathcal{T}_i|}$. Finally, the p -value from (16) is calculated as the fraction of all possible assignments whose means are less than or equal the estimated ITE $\hat{\beta}_i$. In practice, as the number of DR events per user in Phase 1 is about 25 (see Figure 2), the number of total possible assignments becomes computationally infeasible. Thus, we randomly generate a subset of 10^5 assignments from \mathcal{P}_i to compute the p -value in (17).

Moreover, we use the percentile bootstrap method [35] to compute a confidence interval of the estimated ITE for user i around the point estimate $\hat{\beta}_i$.

V. NONEXPERIMENTAL ESTIMATION RESULTS

A. Average Treatment Effects

Figure 6 shows ATE point estimates and their 99% bootstrapped confidence intervals conditional on differing reward levels for all estimators as well as the CAISO BL. Due to empirical de-biasing with Algorithm 1, the point estimates for estimators E1-E6 are close to each other. BL appears to be biased in favor of the DRP, as it systematically predicts smaller reductions than E1-E6.

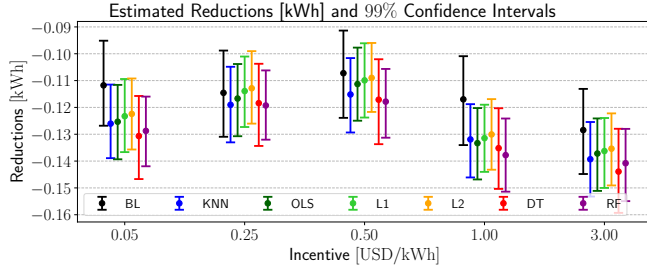


Fig. 6: CATEs by Incentive Level with Bootstrapped Confidence Intervals

Table II reports the estimated ATE (in kWh and percent reduction) and the estimated intercept and slope of the demand curve, aggregated across all incentive levels. Due to the idiosyncratic nature of the CATE for $r = 0.5 \frac{\text{USD}}{\text{kWh}}$, the slope and intercept have to be interpreted with caution. However, the results give rise to a notable correlation between larger incentives levels and larger reductions.

Estimator	ATE (kWh)	ATE (%)	Intercept	Slope $\cdot 3\text{USD}$
BL	-0.116	-12.9	-0.110	-0.018
KNN	-0.126	-13.9	-0.120	-0.020
OLS	-0.125	-13.8	-0.119	-0.019
L1	-0.123	-13.6	-0.116	-0.021
L2	-0.122	-13.5	-0.115	-0.021
DT	-0.129	-14.2	-0.122	-0.022
RF	-0.129	-14.2	-0.123	-0.019

TABLE II: ATE Estimates and Demand Curve by Estimator, all 4791 Users

To compare the prediction accuracy of the estimators, Table III reports the width of the confidence intervals for each method and incentive level. The inferiority of the CAISO baseline compared to the non-experimental estimators, among which RF achieves the tightest confidence intervals, becomes apparent. Therefore, in the remainder of this paper, we restrict all results achieved with non-experimental estimators to those obtained with RF.

	0.05 $\frac{\text{USD}}{\text{kWh}}$	0.25 $\frac{\text{USD}}{\text{kWh}}$	0.50 $\frac{\text{USD}}{\text{kWh}}$	1.00 $\frac{\text{USD}}{\text{kWh}}$	3.00 $\frac{\text{USD}}{\text{kWh}}$
BL	0.0317	0.0321	0.0325	0.0331	0.0317
KNN	0.0274	0.0282	0.0277	0.0273	0.0278
OLS	0.0277	0.0269	0.0272	0.0266	0.0270
L1	0.0272	0.0263	0.0277	0.0250	0.0261
L2	0.0265	0.0270	0.0257	0.0263	0.0269
DT	0.0310	0.0306	0.0317	0.0301	0.0313
RF	0.0260	0.0258	0.0256	0.0273	0.0260

TABLE III: Width of 95 % Confidence Intervals around ATE Point Estimate Conditional on Incentive Level, All Estimators

B. Individual Treatment Effects

Figure 7 plots ITEs for a randomly selected subset of 800 users who received at least 10 DR events in Phase 1, estimated with RF. Users are sorted by their point estimates (blue), whose 95% bootstrapped confidence intervals are drawn in black. Yellow lines represent users with at least one active smart home automation device. By marginalizing the point estimates over all users with at least 10 events, we obtain an ATE of -0.139 kWh (-14.2%), which is close to -0.129 kWh as reported in Table II. The difference ensues from only considering users with at least 10 DR events. The 99% ATE confidence interval is $[-0.154, -0.125] \text{ kWh}$.

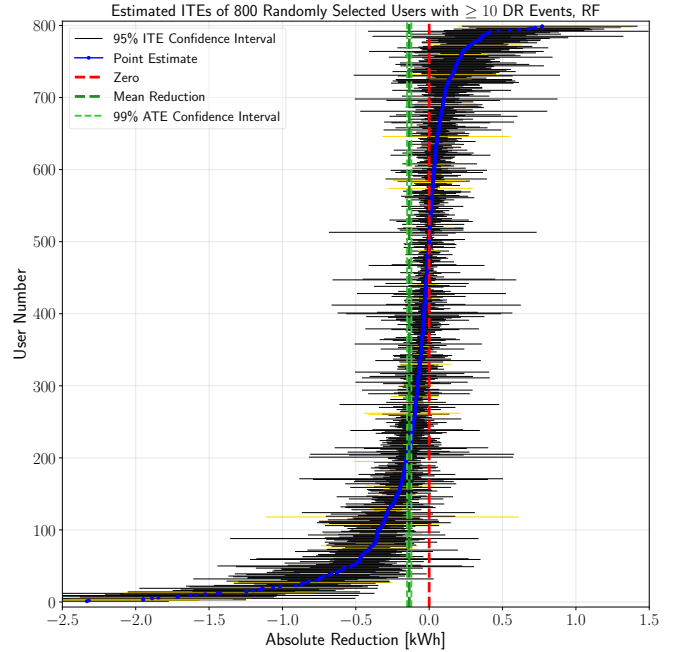


Fig. 7: Distribution of ITEs with Bootstrapped Confidence Intervals

Table IV reports estimated ATEs for users with or without active smart home automation devices, which are obtained by aggregating the relevant estimated ITEs from Figure 7. We notice larger responses as well as a larger percentage of estimated reducers among automated users.

	# Users	% Reducers	ATE (kWh)	ATE (%)
Automated	372	80.4	-0.332	-38.2
Non-Automated	3853	66.7	-0.121	-12.0
All	4225	67.9	-0.139	-14.2

TABLE IV: Estimated CATEs by Automation Status, RF Estimator (E6)

Table V reports the percentage of significant reducers for different confidence levels, obtained with the permutation

test under the null (16). From Tables IV and V, it becomes clear that automated users show larger reductions than non-automated ones, which agrees with expectations. Lastly,

Fraction of Significant Reducers (among sample of size 4225)			
	$1 - \alpha = 0.9$	$1 - \alpha = 0.95$	$1 - \alpha = 0.99$
# Automated	182	158	131
% of Total	48.9	42.5	35.2
# Non-Automated	1240	1041	695
% of Total	32.2	27.0	18.0
# All	1422	1199	826
% of Total	33.7	28.4	19.6

TABLE V: Estimated Percentage of Significant Reducers according to Permutation Test, RF Estimator (E6)

Figure 14 in the Appendix gives rise to a noticeable positive correlation between ambient air temperature and the ITE.

VI. ATE ESTIMATION WITH FIXED EFFECTS MODELS

To estimate the ATE of DR interventions on electricity consumption, we consider the following fixed-effects model with raw consumption (kWh) as the dependent variable:

$$\text{kWh}_{it} = X_{it} \cdot \beta + \alpha_{it} + u_{it}. \quad (19)$$

In (19), subscripts i and t refer to user i at time t , respectively. X_{it} is a row vector of observable covariates, α_{it} are unobserved fixed effects, and u_{it} is the noise term which is assumed to be uncorrelated with the regressors and Gaussian distributed with zero mean and finite variance. The fixed effects term α_{it} removes persistent differences across users in their hourly and monthly consumption interacted with a business day indicator variable:

$$\alpha_{it} \sim C(\text{HoD}_{it}) : C(\text{is_Bday}_{it}) + C(\text{MoY}_{it}). \quad (20)$$

Recall Assumption 1, which states that for an unbiased estimate of the ATE, we require a *randomized* assignment of reward levels to users. However, we observe a notable correlation between the reward level and the CAISO BL, suggesting that the DRP might systematically assign larger reward levels to users with a higher projected baseline. To test this conjecture with a nonparametric hypothesis test, we again make use of a permutation test with the following null and alternative hypothesis:

$$H_0 : \max_{i,j \in \mathcal{R}} \text{KS}_{ij} = 0, \quad H_1 : \max_{i,j \in \mathcal{R}} \text{KS}_{ij} > 0, \quad (21)$$

where KS_{ij} denotes the KS distance between the empirical CAISO BL distributions for all DR events observed for reward levels i and j . The idea of this formulation is to exploit the idea that a randomized assignment of reward levels to users is reflected in a small KS distance and vice versa. To perform a permutation test, we pool all CAISO BL observations during DR events and randomly assign them to differing reward levels, where each group has the same sample size as the original group. Repeating this process many times yields a permutation distribution of the metric $\max_{i,j \in \mathcal{R}} \text{KS}_{ij}$, from which the p -value associated with (21) follows. After 10,000 such iterations, we obtain a p -value of 0.0975, such that we can reject the null at the $1 - \alpha = 0.9$ confidence level. This is indicative of a non-random

assignment of reward levels to users. To account for this fact, we include the CAISO BL in the regression specifications described more closely in the following subsections.

Due to space constraints, the regression tables, which include point estimates and their 95% confidence intervals, t -values of the regression, and clustered standard errors, are relegated to the appendix.

A. Estimation by Incentive Level

To estimate the CATE by incentive level, the covariate matrix X_{it} in (19) is specified as follows:

$$X_{it} = [\text{is_treat}_{it} \quad \text{BL}_{it} \quad T_{it} \quad R_{it}], \quad (22a)$$

$$R_{it} = [\mathbb{1}(r_{it} = 0.05) \quad \cdots \quad \mathbb{1}(r_{it} = 3.00)]. \quad (22b)$$

In (22a), is_treat_{it} is an indicator set to one for all treatment users (and zero for all control users). BL_{it} is the CAISO baseline for user i at time t , which is necessary to control for the non-random assignment of reward levels to users, T_{it} is the ambient air temperature, and R_{it} is the reward level.

B. Estimation by Hour of the Day

To estimate the CATE by hour of the day, we pool all reward levels into the indicator variable is_DR_{it} , which is one if user i received treatment at time t and zero otherwise:

$$X_{it} = [\text{is_treat}_{it} \quad \text{BL}_{it} \quad T_{it} \quad C(\text{HoD}) : \text{is_DR}_{it}]. \quad (23)$$

C. Estimation by Month of the Year

The CATE by month of the year is found in a similar fashion to the CATE by hour of the day:

$$X_{it} = [\text{is_treat}_{it} \quad \text{BL}_{it} \quad T_{it} \quad C(\text{MoY}) : \text{is_DR}_{it}]. \quad (24)$$

D. Role of Smart Home Automation

The CATE by automation status is determined by introducing the indicator is_auto_{it} :

$$X_{it} = [\text{is_treat}_{it} \quad \text{BL}_{it} \quad T_{it} \quad C(\text{is_auto}_{it}) : \text{is_DR}_{it}]. \quad (25)$$

E. Effect of Automation Uptake Encouragement

Lastly, the effect of incentivizing users to purchase a smart home automation device on energy consumption during DR events is determined as follows:

$$X_{it} = [\text{is_enc}_{it} \quad \text{is_nonenc}_{it} \quad \text{BL}_{it} \quad T_{it} \quad \text{is_enc}_{it} \cdot \text{is_DR}_{it} \quad \text{is_nonenc}_{it} \cdot \text{is_DR}_{it}]. \quad (26)$$

In (26), the indicators is_enc and is_nonenc are 1 for all users in the “Treatment-Encouraged” and in “Treatment-Non-Encouraged”, respectively, and zero otherwise.

VII. COMPARISON OF ESTIMATION METHODS

In this section, we benchmark the results obtained from the best non-experimental estimator (RF) to those from the fixed effects model with specification (22a).

Figure 8 compares the point CATEs by reward levels and their 95% confidence intervals. It can be seen that the point estimates are close to each other (-0.123 kWh aggregated for fixed effects vs. -0.129 for non-experimental approach with RF, a less than 5% difference), a finding that suggests

that our non-experimental estimation technique produces reliable estimates comparable to the experimental gold standard. The fact that the confidence intervals are notably tighter for RF corroborates this notion. The remaining comparisons

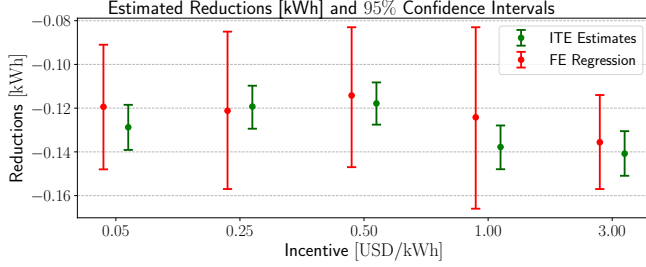


Fig. 8: CATEs by Incentive Level with Confidence Intervals, Comparison Fixed Effects Estimators and Non-Experimental Estimators

by month of the year, automation status, and automation takeover encouragement, accompanied by the Fixed Effects regression results, are relegated to the appendix.

VIII. EFFECT OF ADAPTIVE TARGETING

The goal of adaptive targeting is to maximize the reduction per dollar paid to the users, which is achieved by either minimizing the payout and/or maximizing users' reductions. Since we have no a-priori understanding about the distribution of *individual* users' elasticities in response to incentives, we focus on minimizing the payouts to users. Assuming that users are relatively price inelastic (indeed Figure 8 only shows a weak negative slope of the demand curve), we explore the potential of assigning large incentives to low reducers (and small incentives to high reducers) to minimize the total payout from DRP to users.

A. Targeting Assignment Algorithm

Algorithm 2 describes the targeting assignment algorithm on a given set of users, which we denote with \mathcal{S} . Users

Algorithm 2 Adaptive Targeting of Users

Input: Set of users \mathcal{S} with completed Phase 1

Output: Groups \mathcal{S}_n , \mathcal{S}_l , and \mathcal{S}_h of proportion 2 : 1 : 1

- 1: Randomly split $\mathcal{S} = \mathcal{S}_n \cup \mathcal{S}_t$, $-1 \leq |\mathcal{S}_n| - |\mathcal{S}_t| \leq 1$
- 2: Estimate ITE $\hat{\beta}_i$ with E6 for each user $i \in \mathcal{S}_t$.
- 3: Sort $\{\hat{\beta}_i\}_{i \in \mathcal{S}}$ in ascending order
- 4: Split $\mathcal{S}_t = \mathcal{S}_l \cup \mathcal{S}_h$, such that $-1 \leq |\mathcal{S}_l| - |\mathcal{S}_h| \leq 1$ and $\hat{\beta}_i < \hat{\beta}_j \forall i \in \mathcal{S}_l, j \in \mathcal{S}_h$
- 5: Assign users in \mathcal{S}_l and \mathcal{S}_h to low- and high-targeted, respectively

are transitioned into Phase 2 on a weekly basis. That is, for a particular week, all users who have reached 90 days of age in Phase 1 form the current weekly cohort, which is *randomly* split into a non-targeted group \mathcal{S}_n and targeted group \mathcal{S}_t of equal size (ties are broken randomly). For each user in \mathcal{S}_t , we calculate the ITE based on Phase 1 events. These ITEs are then sorted in ascending order. The 50% of the largest reducers (with the most negative ITEs) are defined to be the low-targeted group \mathcal{S}_l , whereas the other half is assigned to high-targeted group \mathcal{S}_h . This targeting scheme trades off between two sides of the same coin: On

the one hand, the DRP pays less money to large reducers and also achieves larger reductions for previously small reducers, increasing the desired ratio. On the other hand, previously large reducers now reduce less (in response to smaller rewards) and previously small reducers are paid more money for increased reductions, thereby counteracting the desired goal. However, the latter factors are dominated by the gains from the former ones, as we show in the following.

B. Results of Adaptive Targeting

We evaluate the reduction by reward ratios for the targeted (ρ_t) and non-targeted (ρ_n) groups by averaging the per-event reductions $\hat{\beta}_{it}(\mathbf{x}_{it})$ (5) normalized by the reward r_{it} :

$$\rho_t = \frac{\sum_{i \in \mathcal{S}_t} \sum_{t \in \mathcal{T}_i^{(2)}} \hat{\beta}_{it}(\mathbf{x}_{it}) / r_{it}}{\sum_{i \in \mathcal{S}_t} |\mathcal{T}_i^{(2)}|}, \quad (27a)$$

$$\rho_n = \frac{\sum_{i \in \mathcal{S}_n} \sum_{t \in \mathcal{T}_i^{(2)}} \hat{\beta}_{it}(\mathbf{x}_{it}) / r_{it}}{\sum_{i \in \mathcal{S}_n} |\mathcal{T}_i^{(2)}|}, \quad (27b)$$

where $\mathcal{T}_i^{(2)}$ denotes user i 's set of Phase 2 DR events. Table VI reports these metrics together with the CATE by treatment group as well as the number of observations for targeted n_t and non-targeted users n_n . We restrict our analysis to samples obtained after June 27, 2017, as we are able to observe larger effects of targeting in summer months.

Estimator	Targeting Metrics for Phase 2					
	ρ_t	ρ_n	CATE _t	CATE _n	n_t	n_n
BL	1.23	0.728	-0.129	-0.179	2099	2135
RF	1.26	0.876	-0.133	-0.175	2099	2135

TABLE VI: Targeting Results for 452 users between June 27, 2017 - September 28, 2017. $[\rho] = \frac{\text{kWh}}{\text{USD}}$, [CATE] = kWh.

RF predicts a difference of $\rho_t - \rho_n \approx 0.38 \frac{\text{kWh}}{\text{USD}}$, or an increase of about 43% compared to the non-targeted scheme. For BL, this increase is even larger (69%). However, due to the biasedness of the BL (see Figure 6), the RF estimate is more reliable. We can observe the tradeoff between smaller reductions (indeed RF predicts a CATE for targeted users that is 24% smaller compared to non-targeted users) and a reduced average payout, which decreases by 75% (not reported in Table VI). The latter effect dominates the decrease in net reductions, resulting in the 43% increase of the reduction per reward ratio (27a).

IX. CONCLUSION

We analyzed Residential Demand Response as a human-in-the-loop cyber-physical system that incentivizes users to curtail electricity consumption during designated hours. Utilizing data collected from a Randomized Controlled Trial funded by the CEC and conducted by a Demand Response provider in the San Francisco Bay Area, we estimated the causal effect of hour-ahead price interventions on electricity reduction. To the best of our knowledge, this is the first major study to investigate DR on such short time scales.

We developed a *non-experimental* estimation framework and benchmarked its estimates against those obtained from an *experimental* Fixed-Effects Linear Regression Model. Importantly, the former does not depend on the existence of an

experimental control group to construct counterfactuals that are necessary to estimate the treatment effect. Instead, we employ off-the-shelf regression models to learn a consumption model on non-DR periods, which can then be used to predict counterfactuals during DR hours of interest. We find that the estimated treatment effects from both approaches are close to each other. The estimated ATE is -0.13 kWh (14%) per Demand Response event and household. Further, we observe a weak positive correlation between the incentive level and the estimated reductions, suggesting that users are only weakly elastic in response to incentives.

The fact that the estimates obtained from both approaches are close to each other is encouraging, as our non-experimental framework permits to go a step further compared to the experimental method in that it allows for an estimation of *individual* treatment effects. From an economic perspective, being able to differentiate low from high responders allows for an adaptive targeting scheme, whose goal is to minimize the total payout to users while maximizing total reductions. We utilize this fact to achieve an increase of the reduction-per-reward ratio of 43%.

Moreover, we discovered notable heterogeneity of users in time and by automation status, since the largest reductions were observed in summer months as well as among users with at least one connected smart home automation device. Further, the ambient air temperature was found to positively correlate with the amount of reductions, suggesting that air conditioning units are a major contributor to reductions.

Lastly, we emphasize that our non-experimental estimation framework presented in this paper is generalizable to any human-in-the-loop cyber-physical system that requires the incentivization of users to achieve a desired objective. This is because our non-experimental framework admits results on an individual user level, which could be of particular interest in the incentivization of users in transportation systems.

Future work includes the analysis of adversarial user behavior (baseline gaming) and advanced effects including peer and network effects influencing Demand Response. We will report the full data set upon completion of the study.

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APPENDIX

A. Summary Statistics

Figures 9-12 illustrate the distribution of the number of DR events received among users with *completed* Phase 1, as well as the total number of DR events broken out by hour of the day, day of the week, and month of the year.

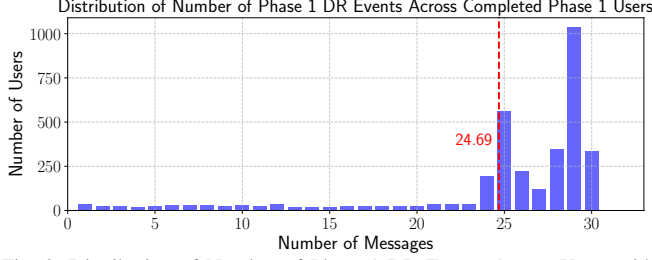


Fig. 9: Distribution of Number of Phase 1 DR Events Across Users with Completed Phase 1

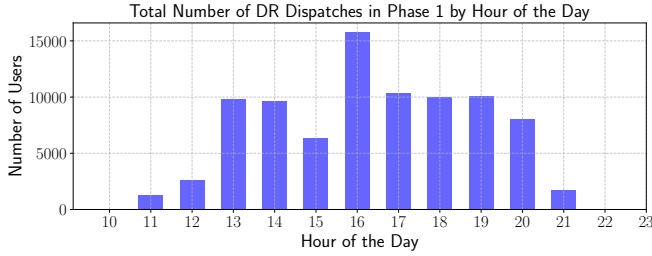


Fig. 10: Distribution of DR Events by Hour of the Day Across Users with Completed Phase 1

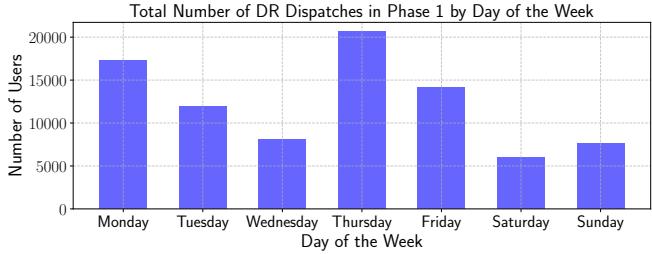


Fig. 11: Distribution of DR Events by Day of the Week Across Users with Completed Phase 1

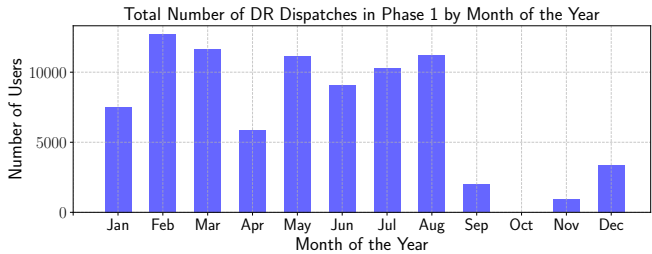


Fig. 12: Distribution of DR Events by Month of the Year Across Users with Completed Phase 1

B. Balance Checks

Table VII provides the balance metrics introduced in Section III-D.

Balance Metrics for Control and Treatment Group			
	Cohen's D	Hellinger Dist.	Diff. Mean
kWh, HoD = 13	0.023	0.002	0.022
kWh, HoD = 14	0.026	0.002	0.026
kWh, HoD = 15	0.017	0.002	0.017
kWh, HoD = 16	0.006	0.002	0.006
kWh, HoD = 17	0.002	0.002	0.002
kWh, HoD = 18	0.002	0.002	-0.007
kWh, HoD = 19	0.002	0.002	-0.010
kWh, HoD = 20	0.002	0.002	-0.008
air_temp, HoD = 13	0.013	0.005	0.032
air_temp, HoD = 14	0.013	0.005	0.032
air_temp, HoD = 15	0.015	0.005	0.038
air_temp, HoD = 16	0.015	0.005	0.039
air_temp, HoD = 17	0.016	0.004	0.041
air_temp, HoD = 18	0.016	0.003	0.041
air_temp, HoD = 19	0.018	0.003	0.043
air_temp, HoD = 20	0.018	0.002	0.042
# historical obs. (hours)	4.632	0.037	303.7

TABLE VII: Balance Checks for Users in Control and Treatment Group

C. Fixed Effects Regression Tables

Tables VIII-X provide the results of the Fixed Effects Regressions presented in Section VI. The point estimates of interest are printed in boldface and are accompanied by the standard errors as well as their 95% confidence intervals. The t -value of the regression gives rise to the p -value, where we use (*), (**), (***) to denote statistical significance at the 90%, 95%, 99% confidence level, respectively.

Effect of DR by Incentive Level on Electricity Consumption				
Parameter	Estimate (Std. Err.)	t -Value	95% Conf. Int.	p -value
is_treat_{it}	-0.006 (0.003)	-2.100	[-0.013, 0]	0.047**
BL_{it}	0.879 (0.010)	88.89	[0.859, 0.900]	<0.001***
T_{it}	0.0205 (0.002)	10.79	[0.017, 0.024]	<0.001***
$1(is_DR_{it}, r_{it} = 0.05)$	-0.120 (0.014)	-8.532	[-0.148, -0.091]	<0.001***
$1(is_DR_{it}, r_{it} = 0.25)$	-0.121 (0.018)	-6.910	[-0.157, -0.085]	<0.001***
$1(is_DR_{it}, r_{it} = 0.50)$	-0.115 (0.016)	-7.369	[-0.147, -0.083]	<0.001***
$1(is_DR_{it}, r_{it} = 1.00)$	-0.124 (0.020)	-6.219	[-0.166, -0.083]	<0.001***
$1(is_DR_{it}, r_{it} = 3.00)$	-0.136 (0.010)	-12.95	[-0.157, -0.114]	<0.001***

TABLE VIII: Fixed Effect Regression Results by Incentive Level

D. Comparison of Estimation Methods

Figure 8 visually compares the ATEs broken out by incentive level, and it can be seen that both methods produce similar estimates. Figure 13 does the same for month of the year. Agreeing with intuition, the reductions are notably larger in summer months compared to winter periods. Conditional on the automation status, Table X states that the reductions are -0.331 and -0.103 kWh for automated and non-automated users, respectively, compared to -0.332 and -0.121 kWh calculated by the non-experimental case. These values are close to each other. Lastly, no significant difference in the magnitude of reductions can be found between encouraged and non-encouraged users.

Effect of DR by Month of Year on Electricity Consumption				
Parameter	Estimate (Std. Err.)	t-Value	95% Conf. Int.	p-value
is_treat_{it}	-0.007 (0.003)	-1.962	[-0.014, 0.001]	0.078*
BL_{it}	0.879 (0.016)	55.52	[0.844, 0.915]	<0.001***
T_{it}	0.021 (0.006)	3.326	[0.007, 0.034]	0.008***
$1(is_DR_{it}, MoY_{it} = 1)$	-0.041 (0.010)	-4.298	[-0.063, -0.020]	0.002***
$1(is_DR_{it}, MoY_{it} = 2)$	-0.022 (0.009)	-2.571	[-0.041, -0.003]	0.028**
$1(is_DR_{it}, MoY_{it} = 3)$	-0.076 (0.004)	-18.62	[-0.085, -0.067]	0.002***
$1(is_DR_{it}, MoY_{it} = 4)$	-0.062 (0.004)	-15.14	[-0.071, -0.053]	<0.001***
$1(is_DR_{it}, MoY_{it} = 5)$	-0.094 (0.005)	-20.07	[-0.104, -0.083]	<0.001***
$1(is_DR_{it}, MoY_{it} = 6)$	-0.155 (0.008)	-20.25	[-0.172, -0.138]	<0.001***
$1(is_DR_{it}, MoY_{it} = 7)$	-0.227 (0.007)	-32.68	[-0.242, -0.211]	<0.001***
$1(is_DR_{it}, MoY_{it} = 8)$	-0.159 (0.008)	-19.39	[-0.177, -0.141]	<0.001***
$1(is_DR_{it}, MoY_{it} = 9)$	-0.179 (0.014)	-9.071	[-0.218, -0.142]	<0.001***
$1(is_DR_{it}, MoY_{it} = 11)$	-0.029 (0.009)	-3.055	[-0.050, -0.008]	0.012**
$1(is_DR_{it}, MoY_{it} = 12)$	-0.022 (0.010)	-2.172	[-0.045, 0.001]	0.055*

TABLE IX: Fixed Effect Regression Results by Month of Year

Effect of Home Automation on Electricity Consumption				
Parameter	Estimate (Std. Err.)	t-Value	95% Conf. Int.	p-value
is_treat_{it}	-0.006 (0.003)	-2.101	[-0.013, 0]	0.047**
BL_{it}	0.879 (0.010)	88.94	[0.859, 0.900]	<0.001***
T_{it}	0.021 (0.002)	10.79	[0.017, 0.024]	<0.001***
$1(is_DR_{it}, is_auto_{it})$	-0.331 (0.042)	-7.800	[-0.418, -0.243]	<0.001***
$1(is_DR_{it}, -is_auto_{it})$	-0.103 (0.014)	-7.310	[-0.132, -0.074]	<0.001***

TABLE X: Fixed Effect Regression Results by Automation Status

Effect of Automation Uptake Incentive on Electricity Consumption				
Parameter	Estimate (Std. Err.)	t-Value	95% Conf. Int.	p-value
is_enc_{it}	-0.005 (0.003)	-1.422	[-0.012, 0.002]	0.168
is_nonenc_{it}	-0.008 (0.003)	-2.485	[-0.015, -0.001]	0.021**
BL_{it}	0.9366 (0.024)	38.38	[0.886, 0.987]	<0.001***
T_{it}	0.0206 (0.002)	10.794	[0.017, 0.024]	<0.001***
$1(is_DR_{it}, is_enc_{it})$	-0.121 (0.016)	-7.703	[-0.153, -0.088]	<0.001***
$1(is_DR_{it}, is_nonenc_{it})$	-0.125 (0.015)	-8.304	[-0.156, -0.094]	<0.001***

TABLE XI: Fixed Effect Regression Results by Automation Uptake Encouragement

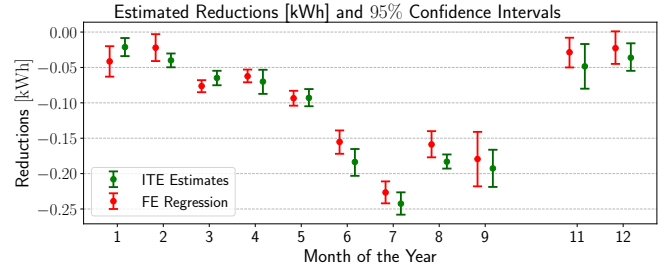


Fig. 13: CATEs by Month of Year with Confidence Intervals, Comparison Fixed Effects Estimators and Non-Experimental Estimators

E. Correlation of Temperature and ITE

As mentioned in the previous subsection, larger reductions are estimated in warm summer months. To test the hypothesis whether or not there exists such a correlation, Figure 14 scatter plots estimated ITEs as a function of the average ambient air temperature observed during the relevant DR events. We can notice a notable positive correlation of ambient air temperature and the magnitude of reductions. Indeed, a subsequent hypothesis test with the null being a zero slope is rejected with a p -value of less than $1e - 9$.

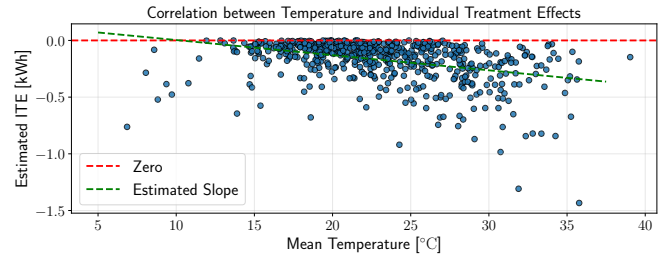


Fig. 14: Correlation between Average Ambient Air Temperature and ITEs.

To support this notion, we marginalize ITEs for each ZIP code to obtain the geographic distribution of CATEs by location, see Figure 15, and it is visually striking that users in coastal areas in California show smaller reductions than users in the Central Valley, where the climate is hotter.

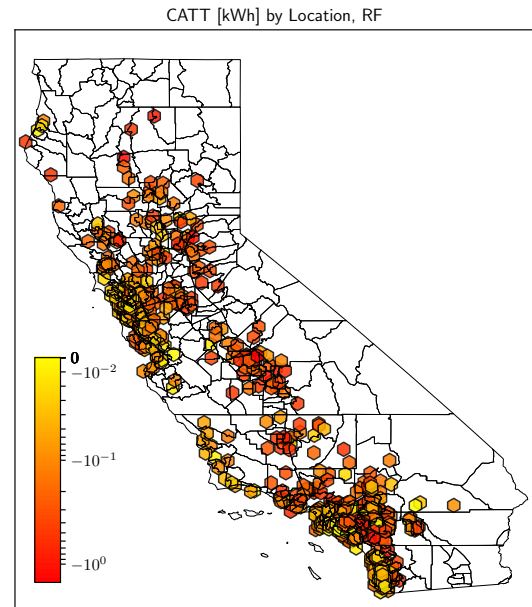


Fig. 15: Correlation between Average Ambient Air Temperature and CATEs.