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Default Effect versus Active Decision: Evidence from a Field Experiment in Los Alamos

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Abstract

We examine how consumers respond to distinct combinations of preset defaults and subsequent economic incentives. A randomised controlled trial is implemented to investigate the demand reduction performance of two electricity pricing programmes: opt-in and opt-out critical peak pricing. Both the intention-to-treat and the treatment-on-the-treated are more pronounced for customers assigned to the opt-in group. This result suggests that the opt-in type active enrolment itself had an impact on customers' subsequent behavior and made them more responsive to the treatment intervention. Moreover, only the opt-in treatment has spillover effects beyond the treatment time window. Our results, therefore, highlight the important difference between an active and a passive decision-making process.

JEL classification: C93, D12

Keywords: Field Experiment, Default Effect, Opt-in, Opt-out, Price Elasticity

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1 Introduction

Decisions by default have become an important issue in behavioural economics and public policy (Johnson and Goldstein, 2013). We take an example from employees' decisions on a 401(k) retirement savings plan (Madrian and Shea, 2001). When employees must opt into the plan, fewer than half enrol on their own. However, when they are automatically enrolled, few employees choose to opt out, resulting in close to 100% enrolment. A voluminous literature now documents the successful applications of default effects, including retirement saving (Madrian and Shea, 2001; Choi et al., 2002; Thaler and Benartzi, 2004; Chetty et al., 2014), organ donation (Spital, 1995; Johnson and Goldstein, 2003; Abadie and Gay, 2006), influenza vaccination (Chapman et al., 2010), contractual choice in health clubs (DellaVigna and Malmendier, 2006), and car insurance plan choices (Johnson et al., 1993). Most of these studies advocate for policies with opt-out defaults (i.e. automatic enrolment defaults).

However, we emphasise in this paper that, in many situations, the calculation of an optimal default may not be straightforward because the welfare impact on consumers could depend not only on their initial choices but also on their subsequent behaviors after the enrolment. Indeed, the high enrolment rate is by itself a powerful outcome in the saving literature (and the literature cited above) because the enrolment automatically changes consumers' choices in a direction that is considered desirable by the policy maker. In contrast, there are also many situations that enrolled consumers must demonstrate active subsequent behaviors for the programme to be effective. Here, we encounter a trade-off. On the one hand, the option to opt into an intervention may result in a limited number of participants, while the subsequent outcomes for these participants may be large because of the attention triggered by the active decision-making process. On the other hand, an opt-out default typically leads to extremely high participation in the first stage, while the subsequent outcomes might be relatively small across a large number of participants.

Therefore, the answer to the issue of optimal default options could be rather unclear, and the related empirical evidence remains sparse, particularly evidence obtained from framed field experiments. In an effort to bridge this gap, we implement a randomised experiment in Los Alamos County (LAC), New Mexico, United States. Our primary data are high-frequency data on household electricity consumption. The treatments are based on a popular dynamic electricity pricing programme, namely critical peak pricing (CPP), which pre-commits households to a high marginal price of electricity during peak demand hours. We randomly assign households to one of three groups: 1) an opt-in CPP group, 2) an opt-out CPP group, and 3) a control group. Note that the interventions in our experiment is relatively more complicated than those in the retirement saving literature. In fact, our design can be regarded as a 'two-stage' policy composed of a default-based enrolment process in the first stage of the experiment and price-based incentives in the second stage. Under such experimental design, the eventual impact of the policy will depend on how these factors interact with each other. For example, although inertia may result in high participation in the first stage, customers' attention and effort may play a central role in the outcome of the second stage.

We present several findings from the experiment. First, the customer enrolment rate is 97.2% for the opt-out CPP group and 63.8% for the opt-in CPP group. We note that the opt-in enrolment rate is relatively high compared with similar dynamic pricing programmes (Potter et al., 2014). The high opt-in rate is particularly important to an experiment with first-stage defaults and second-stage interventions because it helps identify the distinct effects of opt-in and opt-out defaults on the subsequent outcomes. To the best of our knowledge, our field experiment is among the first to identify such difference, which could be very hard to capture if the opt-in enrolment rate is too low.

Second, we estimate the intention-to-treat (ITT) and treatment-on-the-treated (TOT) for each treatment group, and the estimation results suggest that the opt-in default itself may have made customers more responsive, reducing more electricity consumption during the event period. In particular, the ITT captures the average causal effect of the treatment group as a whole, and thus informs us of the overall policy outcome. We find that although the opt-in enrolment rate is relatively low, the estimated ITT of opt-in CPP customers shows an average percentage reduction (9.8%) of on-peak usage higher than that of opt-out CPP customers (5.8%). In addition, the TOT captures the average causal effect of the customers who actually switched to the new dynamic pricing tariff (i.e. the compliers) in each treatment group. The estimated TOTs of opt-in customers show percentage reductions as high as 14.7%, much higher than those of opt-out customers (6.0%). The ITT and TOT results also allow us to deduce that the net effect of active enrolment itself corresponds to an average percentage reduction larger than 5.6% (i.e., 38% of the opt-in TOT) among customers who opt into the CPP programme.

Third, we find that among the two treatment groups, only the opt-in group has spillover effects in the sense that it even generated significant consumption reductions during the time window preceding and following peak hours (i.e. shoulder hours) on treatment days. This result also suggests that opt-in customers were more attentive than opt-out customers, and highlights the difference between active decision making (opt-in) and passive decision making (opt-out).

This paper contributes to the literature on default effects and optimal enrolment rules, which so far has focused on the initial impact of preset defaults. In contrast, how do these defaults affect subsequent behavior of programme participants has not been well studied. Here, we emphasise the importance of such investigation as distinct enrolment procedures may enhance or offset consumers' subsequent behaviors in distinct ways. We document an example in which the opt-in default and related active decision-making process had a more profound impact on households' subsequent behaviors than its opt-out counterpart, both within and beyond treatment event periods. Our result, therefore, suggests that the design of policies with default options should be approached with caution, and the potential interactions among various components of the policies may play a central role in determining the optimal procedure. These findings may have policy implications in many fields of public economics such as health insurance, cell phone service, and energy conservation, where consumers' initial attention and decisions on plan choice may significantly affect their subsequent behaviors on utilisation.

Additionally, our paper contributes to research in energy economics. Non-varying retail prices do not reflect the high marginal cost of electricity during peak demand periods and, thus, result in one of the largest inefficiencies in electricity markets. It has been widely recognised that dynamic pricing such as CPP provides a promising solution. Unlike most existing studies, our experiment is conducted in a rather mild climate (the average maximum temperature of LAC is 77.2°F in summer), with low saturation of the central air conditioning (CAC) systems (about 10%), and we find significant treatment effects even in such an environment.

The remainder of this paper is organised as follows. Section 2 describes our experimental design, data, and customer compliance. Section 3 presents the main results of our study, including the treatment effect estimation strategies and results, and we conclude in Section 4.

2 Research Design and Data

2.1 Experiment Overview

The randomised field experiment was conducted for households in LAC in 2013. The experiment was implemented in collaboration with the Los Alamos Department of Public Utilities, the Los Alamos National Laboratory, New Energy and Industrial Technology Development Organization, Toshiba and Itochu. Smart meters, which record households' electricity consumption at 15-minute intervals, were installed in all the 1,648 households residing in the areas of North and Barranca Mesas in LAC; these households form the target of our recruitment activities.

The installation of the meter system was completed in September 2012, and participant recruitment began in February 2013 (Figure 1 shows the timeline of the experiment). To recruit households, the Los Alamos Department of Public Utilities held a neighbourhood meeting on the introduction of the randomised experiment and sent details of the experiment by mail to households. We offered households US$50 as a participation incentive for the summer season and US$50 for the winter season. Additionally, US$80 was offered upon the completion of customer survey questions. The recruitment process ended in April 2013, and we recruited 914 households to participate in our experiment, which was more than half the total number of target households. Note that these participants were self-selected samples as were the samples in previous studies for electricity pricing experiments (Wolak, 2010, 2011; Faruqui and Sergici, 2011; Jessoe and Rapson, 2014; Ito, Ida, and Tanaka, 2017). A total of 798 (87.3%) of these participant households also responded to the customer survey questions.

We randomly assigned the participants into treatment and control groups, which we clarify in Section 2.2. In May 2013, participants were notified of their group assignment by mail and e-mail, and were given the opportunity to choose between the dynamic pricing rate and standard LAC flat rate on an opt-in or opt-out basis. The development of the smart grid system (that is, the community energy management system) was completed at the end of June, and it was in charge of the collection of participants' consumption data, transmission of pricing signals, and calculation of participants' economic incentives.

The experiment ran during the summer from July to September and during the winter from December to February. Those who decided to use the dynamic pricing rates were subject to a maximum of 15 event days (i.e. treatment days) during summer and a maximum of 15 event days during winter. In addition, dynamic pricing event hours were designed to be from 4 pm to 7 pm on event days.[1] Event days were defined as the weekdays when on-peak aggregate electricity consumption strains the capacity of the grid. Specifically, for the summer experimental period, treatment days were announced if the day-ahead forecast of the peak load in the system exceeded 13,400 kW and the day-ahead forecast of the maximum temperature exceeded 78.8°F (26°C). For the winter season, treatment days were announced if the day-ahead forecast of the peak load exceeded 13,000 kW and the day-ahead forecast of the minimum temperature was lower than 42.8°F (6°C). As a result, the treatment groups experienced 14 event days in summer and 15 event days in winter. The process of the determination of event days is demonstrated in Figure 2.

The primary data of our study consist of the 15-minute electricity consumption records, including both the data on the customers who participated in the experiment and the data on those who decided not to participate. We also collect household data from surveys and temperature data from the National Climatic Data Center (NOAA 2013-2014).

2.2 Treatments and Randomised Group Assignment

The treatments of this study are based on a popular dynamic pricing tariff, in which the price during the peak period on a small number of demand-response event days is set much higher than the standard rate.[2]

[1] We chose 4 pm to 7 pm as the event hours because the experiment was implemented in a residential area where electricity usage peaks in the evening.

[2] Our experimental design also includes a peak time rebate (PTR) tariff, in which a customer is given a rebate if the on-peak usage is lower than certain PTR baseline on event days. The PTR tariff is of interest, particularly to regulators and the electric power industry, because it does not charge high prices during the event period and, thus, is more desirable than the CPP tariff in terms of customer protection. However, it is not useful to our current goal of comparing policies with the same economic incentive and different preset default options. Therefore, we focus on the study of the two CPP-based treatments in this paper. Furthermore, although CPP is totally exogenous to customers, PTR is endogenous because the PTR baseline for each customer is determined as a function of the customer's own electricity consumption during the previous week. It is thus difficult to compare directly the average treatment effects of the CPP groups with those of the PTR group.

Table 1: Pricing Schemes

Tariffs	Event Day On-Peak	Event Day Off-Peak	Non-Event Day
Flat	9.52¢/kWh	9.52¢/kWh	9.52¢/kWh
CPP	75¢/kWh	7.77¢/kWh	7.77¢/kWh

Notes: This table reports the details of the two pricing schemes studied in the paper: the standard flat rate ('Flat' in the table) and the CPP rate ('CPP' in the table). The term 'On-Peak' refers to the time period from 4 pm to 7 pm and 'Off-Peak' refers to the remaining time period of the day.

Critical Peak Pricing: CPP is a dynamic pricing form that combines a fixed price structure (either the usual flat rate or a discounted rate) with occasional departures from the fixed tariff when power demand is high. In our experiment, the CPP tariff pre-commits households to a high marginal price of electricity between the hours of 4 pm and 7 pm on event days. At the same time, households pay a discounted tariff for consumption during other hours. Specifically, the standard retail tariff in LAC is 9.52 cents/kWh. During the dynamic pricing events, the electricity price for CPP customers was raised by a factor of approximately eight compared with the standard rate, namely 75 cents/kWh. However, these customers needed only pay a discounted price of 7.77 cents/kWh for consumption during all the other time periods of the experiment.[3] Table 1 demonstrates the structure of the LAC standard flat rate and CPP rate.

We randomly assigned 733 participant households to one of three groups: control, opt-in CPP, and opt-out CPP (see Figure 3 for the experimental design and group assignment); these households form the sample for our 'opt-in versus opt-out' study.[4] Some attrition occurred

[3]This discounted price was designed under the revenue neutrality condition, which guarantees that bills under the standard flat rate and CPP rate would be the same, on average, if there were no price elasticity; that is, if the customer's consumption behaviour remains the same under the two alternative rates. County-level aggregate consumption data in the summer and winter seasons of 2012 were used for the calculation of revenue neutrality.

[4]The remaining 181 participants were randomly assigned to the PTR treatment group (3 of them moved or requested to be removed from the study before the beginning of the summer experiment).

before the beginning of the summer experiment; 11 households (1.5%) either moved or requested to be removed from the study. Additionally, some attrition occurred after the completion of the summer experiment; six households (0.8%) did not participate in the winter experiment. Because the attrition occurred at approximately the same rate in each group and is small compared with the total number of participants, it is unlikely to significantly bias our estimates. We describe the control and treatment groups in detail:[5]

1. **Control Group:** A total of 174 households were assigned to the control group. These households were informed of their group assignment, and they were subject to the standard LAC flat rate during the experimental period. The control group did not receive any dynamic pricing signals.

2. **Opt-in CPP Group:** A total of 365 households were assigned to this treatment group. These households were informed of their group assignment and were notified that their default rate was the standard flat rate and that they needed to "opt in" actively to receive the dynamic price signals and use the CPP rate during the event periods. To do so, they had to respond to an e-mail or an SMS message from the utility department. We assigned relatively more customers to this group because, based on the results in other experimental studies of dynamic pricing, we expected that the actual customer enrolment rate would be much lower than the enrolment rate for the other treatment group.

3. **Opt-out CPP Group:** A total of 183 households were assigned to the opt-out CPP group. These households were informed of their group assignment and notified that their default rate was the CPP rate. In addition, households were informed that to switch to the standard flat rate, they needed to 'opt out' from the CPP rate by responding to an e-mail or an SMS message from the utility department.

Table 2 presents the descriptive statistics of the on-peak and off-peak usage preceding the first CPP event (9 days of 15-minute consumption data[6]) and appliance ownership for each group.

[5] The number of households in each group is as of the beginning of the summer experiment, excluding the 11 dropouts.

[6] As illustrated in Figure 1, the development of the smart grid system was completed at the end of June 2013, and it began the collection of household-level 15-minute consumption data from July 2013. As a result, we have 9 days of 15-minute consumption data preceding the first CPP event; these data were used as baseline usage data in the difference-in-difference regression that will be described below.

Each column shows the mean and standard deviation of these observable characteristics of households by group. The columns 'P-value' report the p-values of t-statistics for the difference in means between each treatment group and control group. Because of the random assignment of the groups, none of the difference in means is statistically significant. This supports the integrity of the randomization.

CPP customers in both the opt-in and the opt-out treatment groups were informed of the event days by day-ahead and same-day notices via e-mail or SMS messages. By contrast, customers who chose to use the standard flat rate did not receive any notice during the experiment. The detail of the notice is as follows:

'Price event mm/dd, Peak 4p-7p. CPP rate \$0.75/kWh peak, \$0.0777/kWh non-peak.'

In addition, an incentive system similar to those in Jessoe and Rapson (2014, p.1421) and Wolak (2010, 2011) was applied in our experiment. Following these experiments, we transmitted the experimental price incentives via an off-bill account, and this account was credited with 50 points (i.e. the participation incentive) at the beginning of each season. During the experimental period, the amount of incentives lost or earned[7] by the household was subtracted from or added to the account balance. At the end of the experiment, any balance remaining in the account was the customers to keep (i.e. one point = US\$1). Throughout the experiment, CPP customers in both treatment groups were apprised of their points accrual in the same manner through a series of messages delivered by e-mail or SMS:

'Points on DR day (mm/dd) = X_1. Cumulative = X_2 including non-DR days = X_3.'

Additionally, at the conclusion of each season, the system informed CPP customers of the total points earned for that season:

"Total points you've earned for this season are X_4."

[7] It equals the difference between the LAC standard flat tariff and CPP tariff multiplied by the quantity of the household's actual usage.

Table 2: Summary Statistics

Variables	Control Mean (S.D.)	Opt-in CPP Mean (S.D.)	P-value	Opt-out CPP Mean (S.D.)	P-value	Obs.
Pre-event on-peak usage (kWh/h)	1.09	1.06	0.74	1.03	0.44	722
	(0.77)	(0.73)		(0.66)		
Pre-event off-peak usage (kWh/h)	0.82	0.79	0.41	0.81	0.78	722
	(0.51)	(0.50)		(0.48)		
Number of central ACs	0.12	0.10	0.55	0.08	0.31	596
	(0.41)	(0.32)		(0.30)		
Number of window-unit ACs	0.37	0.30	0.31	0.40	0.73	596
	(0.72)	(0.67)		(0.77)		
Number of space heaters	0.66	0.60	0.48	0.68	0.90	596
	(0.89)	(0.84)		(0.91)		
Number of electric water heaters	0.33	0.30	0.48	0.28	0.38	596
	(0.54)	(0.52)		(0.48)		
Number of refrigerators	1.33	1.32	0.88	1.37	0.46	596
	(0.50)	(0.53)		(0.57)		
Number of dryers	0.81	0.78	0.38	0.80	0.80	596
	(0.40)	(0.44)		(0.41)		
Number of televisions	1.99	1.93	0.50	2.03	0.69	596
	(0.87)	(0.85)		(0.82)		
Number of desktop computers	1.04	1.07	0.72	1.07	0.80	596
	(0.78)	(0.75)		(0.72)		
Number of sprinkler systems	0.37	0.39	0.66	0.40	0.65	596
	(0.49)	(0.55)		(0.62)		

Notes: This table reports summary statistics for households in the opt-in/opt-out CPP and control groups. Means are reported by group, with standard deviations in parentheses below. The columns 'P-value' report the p-values of t-statistics for the difference in means between each treatment group and control group. The availability of appliance data is subject to survey compliance.

2.3 Analysis of Customer Compliance

Understanding how customer compliance differs among various treatments is critical for policy-makers when designing an effective programme. Table 3 reports the results of group assignment and customer enrolment rates for each treatment group. Consistent with existing studies, the opt-out CPP enrolment rate is extremely high (97.2%). However, it turns out that 63.8% of those assigned to the opt-in CPP group actively chose to switch from the standard rate to the CPP rate. This enrolment rate is relatively high compared with those reported in other dynamic pricing experiments. For example, the opt-in CPP enrolment rates of the experiment in the Sacramento Municipal Utility District (SMUD) are approximately 20% (Potter et al., 2014; Fowlie et al., 2017). However, we note that the random assignment implemented in the SMUD experiment is very different from that in our experiment. Specifically, their experiment was undertaken using the randomised encouragement design (RED), where customers were not inquired before the random assignment whether they would like to participate in the experiment. On the other hand, similar to that in Jessoe and Rapson (2014), our random assignment follows the RCT procedure and was implemented on the customers who already agreed to participate in the experiment.[8]

The high opt-in enrolment rate is especially valuable to an experiment with first-stage default options and second-stage policy interventions because as we see in Section 3, it largely contributes to the overall impact of the opt-in CPP programme. This then makes it possible to identify the distinct effects of the opt-in and opt-out defaults (i.e., active decision-making versus passive decision-making) on the second-stage outcomes, and makes it possible to answer the central question of this study: does the active enrolment itself make customers more attentive and responsive to subsequent economic incentives? Indeed, such a difference could be very hard to capture if the opt-in enrolment rate is too low[9].

To understand further the consumption characteristics (usage and load profile) of customers who actively chose to opt in, we estimate a probit model by assuming that individual decisions

[8]If we also take the 734 non-participants into account, our opt-in enrolment rate corresponds to the rate around 35% in an RED-type experiment as non-participants are unlikely to actively opt in.

[9]When the opt-in enrolment rate is too low, the opt-out-type programme typically has much larger overall impact (in terms of the ITT) because of its extremely high enrolment rate.

on whether to opt in depend on a linear function of certain characteristic variables X_i:

$$Y_i = 1\left(X_i'\delta + v_i \geq 0\right) \tag{1}$$

where Y_i equals one if household i decides to opt into the CPP rate and zero otherwise, and v_i is assumed to be normally distributed. Here, we construct household-level average usage and the average on-peak/off-peak ratio of usage as the customer characteristic variables, using pre-event consumption data.

In particular, we want to know whether customers with relatively low on-peak/off-peak ratios, so-called 'structural winners',[10] were more likely to opt in. Note that the new tariff offers a discounted rate for time periods outside CPP events; these customers may therefore have large gains from switching even without significantly changing their consumption behaviors on treatment days. If a large number of enrolled households turn out to be structural winners, the overall impact of the opt-in treatment could be compromised. The estimation result is reported in Table 4, and the coefficient on the average usage is statistically insignificant. In addition, the coefficient on the on-peak/off-peak ratio is positive and statistically significant at the 10% level. This finding suggests that, in our experiment, customers' probability of switching to CPP slightly increases with their on-peak/off-peak ratio; i.e., 'structural winners' are not more likely to opt in.

3 Main Results

3.1 Estimation Strategy for the Average Treatment Effects

Our primary research interest is studying how customers change their peak hour electricity consumption under distinct default options. In this section, we present the econometric framework used to estimate the ITT and TOT of each treatment group. The ITT corresponds to the average causal effect of assignment to treatment, irrespective of customers' actual compliance status. Thus, it measures the overall impact of the opt-in or opt-out CPP treatment.

Following the methodology of Wolak (2006, p.15) and Jessoe and Rapson (2014, pp.1428-

[10] e.g., see Borenstein (2013) for the definition and related discussion on this issue.

Figure 1: Experiment Timeline

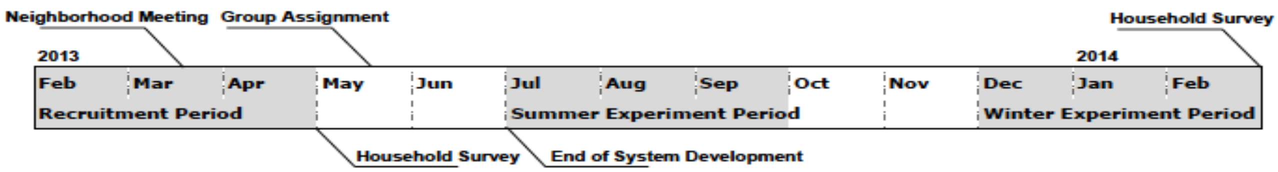


Table 3: Group Assignment and Customer Enrolment Rates

Groups	Total	Flat	CPP	Enrolment Rate
Opt-in CPP	365	132	233	63.8%
Opt-out CPP	183	5	178	97.2%
Control	174	174	N/A	N/A

Notes: This table reports the number of households assigned to each group and number of households who accepted the offer of treatment. 'Total' denotes the total number of households assigned to a certain group; 'Flat' denotes the number of households who decided to use the LAC flat rate; 'CPP' denotes the number of households who decided to use the dynamic pricing tariffs (i.e. who accepted the offer of the CPP programme); 'Enrolment Rate' equals the number of 'CPP' divided by the number of 'Total' in each group.

Figure 2: Algorithm for Demand-Response Event Days

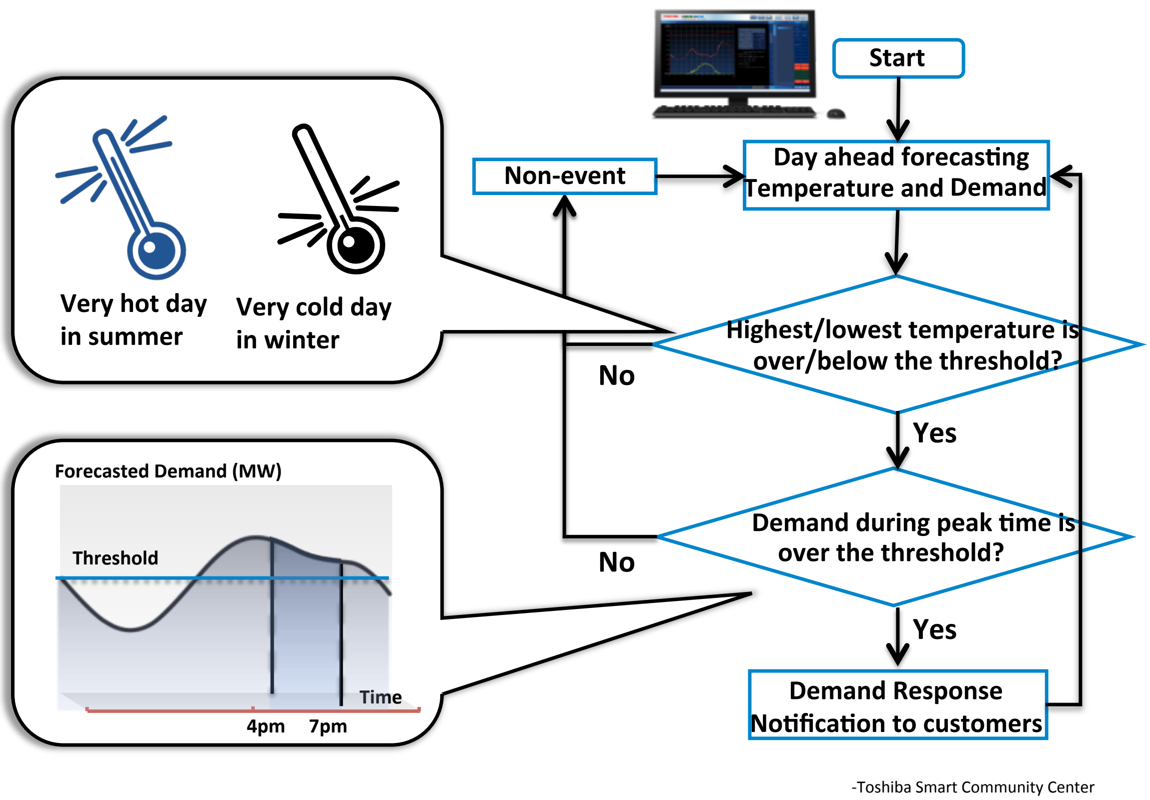
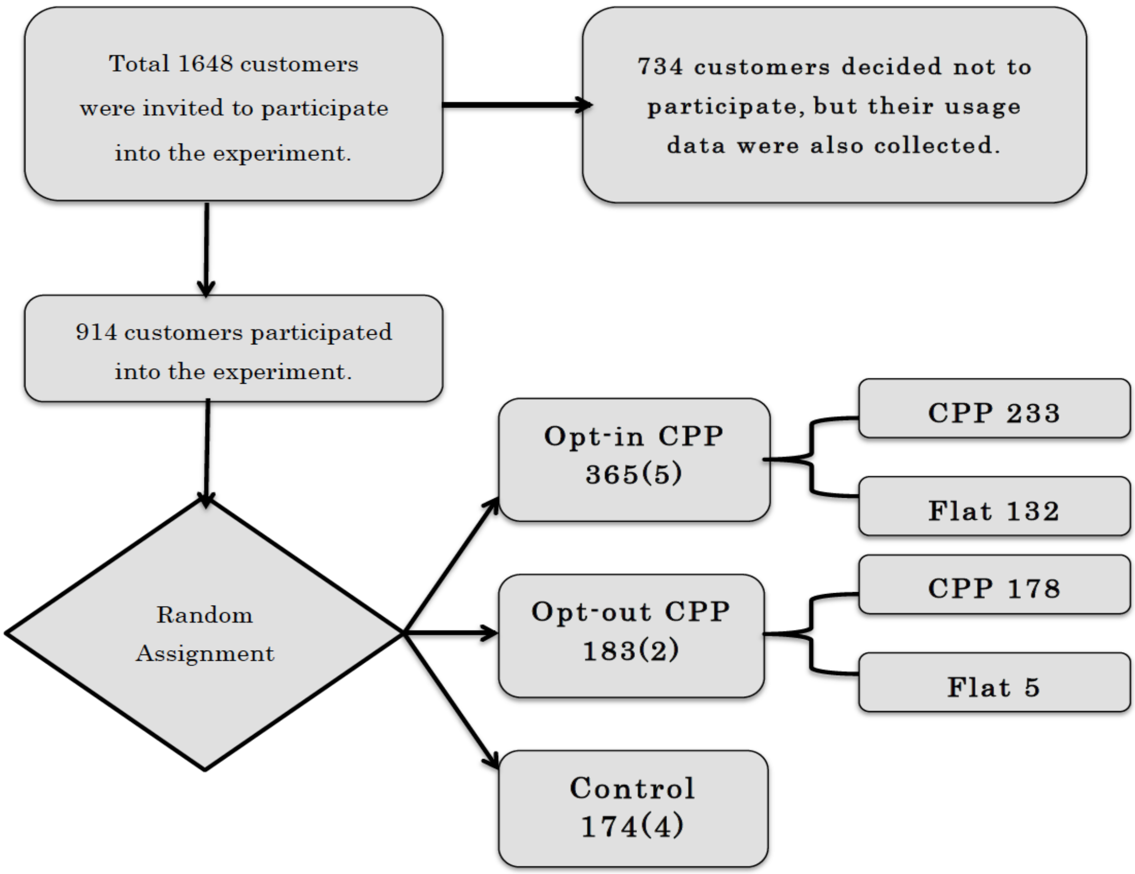


Figure 3: Experimental Design and Group Assignment



Notes: The 914 participants were randomly assigned to four groups: control, opt-in CPP, opt-out CPP and opt-out PTR groups. We note that 181 participants were randomly assigned to the opt-out PTR group (3 of them moved or requested to be removed from the study before the beginning of the summer experiment). The control, opt-in CPP and opt-out CPP groups form the sample for the "opt-in versus opt-out" study of this paper. Numbers of attrition are reported in parentheses.

Table 4: CPP Selection Probit Model of the Opt-in CPP Group

Explanatory Variable	(1)	(2)
Average Consumption	0.149	
	(0.182)	
On-peak/Off-peak Ratio		0.132*
		(0.076)
Observations	365	365

Notes: This table reports the result of the marginal effects for the probit model, in which the dependent variable equals one if the household assigned to the opt-in CPP treatment group decided to opt into the CPP tariff and zero otherwise. *, **, and *** show 10%, 5%, and 1% statistical significance, respectively.

1429), we use the consumption data during peak-time period (4 pm to 7 pm) to estimate the ITTs of the two treatment groups during CPP event hours. Let y_{it} denote household i's electricity consumption during a 15-minute interval period t, then our panel data model controlling for household fixed effects and time fixed effects can be written as:

$$lny_{it} = \sum_{g \in \{CPP_{in}, CPP_{out}\}} \beta^{g}_{ITT} \cdot I^{g}_{it} + \theta_i + \lambda_t + \epsilon_{it} \tag{2}$$

where the indicator variable I^{g}_{it} equals one if household i is in treatment group g with $g \in \{CPP_{in}, CPP_{out}\}$ and if a dynamic pricing event occurs for i in interval t.[11] 'CPP_{in}' and 'CPP_{out}' denote the opt-in CPP group and the opt-out CPP group, respectively. θ_i denotes a household fixed effect that controls for persistent differences in consumption across households and λ_t denotes a time fixed effect for each 15-minute interval t that accounts for weather and other shocks specific to t. ϵ_{it} is an unobserved mean zero error term. Here, the explanatory

[11] We use the natural log of usage for the dependent variable to enable us to interpret the treatment effects approximately in percentage terms. The treatment effects in the exact percentage terms can be obtained by $exp(\beta^{g}_{ITT}) - 1$.

variables of interest are the indicators I^g_{it}, and the coefficients β^g_{ITT} correspond to the average percentage change in electricity usage from assignment to each treatment during pricing events. Note that high-frequency data on customer-level electricity consumption are likely to be serially correlated; we, therefore, cluster standard errors at the customer level. Bertrand et al. (2004) contains a detailed discussion on the consistency of such standard errors in the presence of any time-dependent correlation pattern in ϵ_{it} within i.

Moreover, as our experiment involves distinct preset default options, which result in very different customer enrolment rates, we also estimate the TOTs for each treatment group. The TOT captures the average causal effect of each treatment on the subpopulation of compliers, that is, households who actually enrolled in the CPP tariff. Although the initial treatment assignments were implemented randomly in our experiment, some households assigned to the treatment groups did not enrol in CPP. Thus, the actual receipt of treatment depends on households' self-selection and can be regarded as endogenous; in such cases, an ordinary least squares regression cannot consistently estimate the TOTs. The standard econometric solution to this problem is to use the instrumental variable (IV) regression. Our TOT specification uses the initial treatment assignment as an IV for the actual receipt of treatment and is estimated by using the two-stage least squares regression.[12] The randomisation of initial treatment assignment and high rates of customer compliance (63.8% for opt-in CPP and 97.2% for opt-out CPP) ensure both the validity and the strength of the IV in our regressions. The following specification is used to estimate the TOTs of each treatment group:

$$lny_{it} = \sum_{g \in \{CPP_{in}, CPP_{out}\}} \beta^g_{TOT} \cdot T^g_{it} + \theta_i + \lambda_t + \epsilon_{it} \tag{3}$$

where the indicator variable T^g_{it} equals one if I^g_{it} equals one and if household i is actually enrolled. As with the ITT regressions, we use the on-peak consumption data in the estimation, and cluster standard errors at the customer level to account for serial correlations in ϵ_{it}.

[12] Our experiment is an RCT with one-sided non-compliance: customers assigned to the treatment groups can decline the treatment but customers assigned to the control group are not allowed to take the treatment. Therefore, the TOT in our experiment is equal to the local average treatment effect.

3.2 Estimation Results for the Average Treatment Effects

The columns in Table 5 labelled 'ITT' report the results from the ITT estimators of each treatment group. Investigating these results, we find that households in both treatment groups consumed significantly less electricity during event periods (4 pm to 7 pm on treatment days) than households in the control group. In particular, both ITTs are statistically different from zero at the 1% significance level. Despite the fact that many dynamic pricing experiments have been implemented in hot climates, very few studies have been carried out in moderate climates.[13] It is thus remarkable that significant peak time reduction is achieved in a region with a rather mild climate (the average maximum temperature of LAC is 77.2°F during the summer months) with a low saturation of central air conditioning systems (about 10% in LAC).

More importantly, it turns out that the opt-in CPP group has relatively large estimates of ITT (9.8% in absolute value). It is remarkable that even with a relatively low enrolment rate, the opt-in group succeeded in generating a larger aggregate impact than its opt-out counterpart (5.8%). In addition, the corresponding P-value for the test of difference between the treatment effects is 0.029. We note the standard economic theory would predict that the opt-out CPP group generates higher ITTs because it faces a higher (overall) marginal price of electricity than the opt-in CPP group during on-peak periods and a lower (overall) marginal price during off-peak periods.[14] Moreover, the RCT design ensures that the only systematic difference between the two treatment groups is the default option, and customers in the two treatment groups have similar overall potential for on-peak reduction. The ITT result, therefore, suggests that the opt-in type active enrolment itself had an impact on customers' subsequent behavior and made them more responsive during the CPP event periods.

The columns in Table 5 labelled 'TOT' report the results for the TOT estimators, that is, the estimators of the average causal effect on the compliers in each treatment group. Not surprisingly, the estimated TOT of the opt-in group (14.7%) is much larger than those of the opt-out group (6.0%). The TOT estimates of the opt-out group are very similar to its ITT

[13] To the best of our knowledge, Faruqui et al. (2014) is the only existing study in a moderate climate.

[14] During on-peak periods of event days, 97.2% of opt-out CPP customers were on 75 cents/kWh and 2.8% were on 9.52 cents/kWh, while 63.8% of opt-in CPP customers were on 75 cents/kWh and 36.2% were on 9.52 cents/kWh. On the contrary, during off-peak periods, 97.2% of opt-out CPP customers were on 7.77 cents/kWh and 2.8% were on 9.52 cents/kWh, while 63.8% of opt-in CPP customers were on 7.77 cents/kWh and 36.2% were on 9.52 cents/kWh.

Table 5: Average Treatment Effects

Treatment Groups	ITT	TOT
	(1)	(2)
CPPin	-0.098***	-0.147***
	(0.016)	(0.025)
CPPout	-0.058***	-0.060***
	(0.020)	(0.021)
P-value[CPPin = CPPout]	0.029**	0.000***
Household Fixed Effect	Yes	Yes
Time Fixed Effect	Yes	Yes
Observations	584,616	584,616

Notes: This table reports the estimation results of the average treatment effects of each treatment group during the dynamic pricing events (4 pm to 7 pm on treatment days). The columns 'ITT' and 'TOT' show the estimation results for the intention-to-treat and the treatment-on-the-treated of each treatment group, respectively. Standard errors in parentheses are clustered at the household level to adjust for serial correlation. *, **, and *** show 10%, 5%, and 1% statistical significance, respectively.

estimates because of the extremely high customer enrolment rates. A potential concern is that the very high TOTs of the opt-in group are due to customers' selection into the new tariff: those who are most price responsive tend to opt in. However, this scenario alone cannot explain the obtained results because the overall impact (i.e. in terms of the ITT) of the opt-in treatment is also larger than that of the opt-out treatment. Thus, we expect that the opt-in and opt-out defaults do have distinct effects on customers' elasticity.

Table 6 reports estimated average treatment effects for summer and winter separately. The results for summer are presented in Panel A and those for winter are presented in Panel B, and they have a similar pattern as those in Table 5. The opt-in CPP group has relatively large estimates of ITT in absolute value (8.7% for summer and 10.4% for winter), and the corresponding P-value of the testing of the equality of ITTs is 0.086 for summer and 0.089 for winter. Not surprisingly, the opt-in TOTs are much larger than the opt-out TOTs in both summer and winter.

We note that opt-out defaults have been applied successfully in the retirement saving literature because, in these applications, individuals are not required to take any action after the initial enrolment. Indeed, opt-out defaults exploit the significant inertia among customers to obtain extremely high participation in saving plans, and the participants typically retain the plan contribution rates chosen by companies. However, how do initial defaults affect consumers' subsequent behaviors has not been well studied in the literature. The situation considered in this paper is more complicated than the retirement saving, and could be considered to be two-stage policies as they involve a customer enrolment process in the first stage and (possibly repeated) treatment interventions in the second stage. Here, the eventual success of the policies depends not only on initial enrolment rates but also on the attention that could be triggered by the first-stage procedure, which, in turn, may substantially affect the impact of the second-stage interventions. In the current context, to face CPP events and achieve significant usage reductions, households must possess a good understanding of the pricing scheme and incentive system, identify which home appliances consume a relatively high amount of electricity, and decide which appliances or services the family is willing to live without during event periods; all these activities may require considerable attention and cognitive effort.

Table 6: Average Treatment Effects

Treatment Groups	ITT	TOT
	(1)	(2)
Panel A: Summer Estimates		
CPPin	-0.087***	-0.131***
	(0.020)	(0.030)
CPPout	-0.051**	-0.052**
	(0.024)	(0.024)
P-value[CPPin = CPPout]	0.086*	0.002***
Household Fixed Effect	Yes	Yes
Time Fixed Effect	Yes	Yes
Observations	319,716	319,716
Panel B: Winter Estimates		
CPPin	-0.104***	-0.156***
	(0.019)	(0.029)
CPPout	-0.066***	-0.068***
	(0.023)	(0.024)
P-value[CPPin = CPPout]	0.089*	0.002***
Household Fixed Effect	Yes	Yes
Time Fixed Effect	Yes	Yes
Observations	264,900	264,900

Notes: This table reports the results of the average treatment effects of each treatment group during the dynamic pricing events (4 pm to 7 pm on treatment days) for summer and winter separately. The columns 'ITT' and 'TOT' show the estimation results for the intention-to-treat and the treatment-on-the-treated of each treatment group, respectively. Standard errors in parentheses are clustered at the household level to adjust for serial correlation. *, **, and *** show 10%, 5%, and 1% statistical significance, respectively.

3.3 Spillover Effects of the Opt-in CPP Group

In general, when the marginal cost of electricity supply is high during on-peak periods, its cost is also likely to be high during the hours preceding and following these periods. Therefore, if households simply choose to curtail on-peak consumption and shift their usage into these off-peak hours (i.e. shoulder hours), the economic benefits of dynamic pricing programmes could be compromised. For instance, there may be pre-cooling behaviors among CPP households before summer events, or a 'backfire' effect might be observed after summer events when customers conduct activities that they avoided during on-peak hours. Similarly, during the winter experiment, households might have pre-heating behaviors or they might adjust heaters to a higher temperature as soon as CPP events end.

Interestingly, we find that in our experiment, the opt-in treatment does not result in such peak–off-peak load shifting and even has spillover effects in the sense that the opt-in CPP reduction of on-peak electricity usage spills over into the hours preceding and following the event period. This result is highlighted in Table 7, where we present the estimated average treatment effects of both treatment groups during the shoulder hours (i.e., the three hours before the event period and the three hours after the event period). In particular, we use exactly the same econometric methodology as that used in the previous section for the estimation of on-peak ITTs in eq.(2) and on-peak TOTs in eq.(3), but with the consumption data preceding (1 pm to 4 pm) or after (7 pm to 10 pm) the on-peak time window.[15]

We find that the opt-in CPP group generated a 5.4% usage reduction in terms of the ITT during the time window before the CPP events and a 4.8% reduction during the time window after the events, with both coefficients being statistically different from zero at the 1% significance level. By contrast, we do not find such significant spillover effects for the opt-out CPP group. Although the coefficients of the opt-out group are also estimated as negative, they are quite small compared with the estimates of the opt-in group and statistically indistinguishable from zero. In addition, the tests of the equality of ITTs report P-values of 0.069 and 0.001 for the shoulder hours before and after the CPP events, respectively. Not surprisingly, the corresponding TOTs of the opt-in group (8.1% before the events and 7.2% after the events) are

[15] We also estimated using the data from 11 am to 4 pm and from 7 pm to 12 pm, and the results have very similar patterns.

Table 7: Spillover Effects

Treatment Groups	ITT	TOT	ITT	TOT
	(1)	(2)	(3)	(4)
	3 hrs before events		3 hrs after events	
CPPin	-0.054***	-0.081***	-0.048***	-0.072***
	(0.019)	(0.028)	(0.013)	(0.020)
CPPout	-0.019	-0.020	-0.003	-0.003
	(0.021)	(0.022)	(0.015)	(0.016)
P-value[CPPin = CPPout]	0.069*	0.011**	0.001***	0.000***
Household Fixed Effect	Yes	Yes	Yes	Yes
Time Fixed Effect	Yes	Yes	Yes	Yes
Observations	633,539	633,539	584,784	584,784

Notes: This table reports the estimation results of the average treatment effects of each treatment group during the time window preceding (1 pm to 4 pm) or following (7 pm to 10 pm) the dynamic pricing events. The columns 'ITT' and 'TOT' show the estimation results for the intention-to-treat and the treatment-on-the-treated for each treatment group 3 hours before the events and 3 hours after the events. Standard errors in parentheses are clustered at the household level to adjust for serial correlation. *, **, and *** show 10%, 5%, and 1% statistical significance, respectively.

much larger than those of the opt-out group.

In summary, similar to those in the previous section, the results obtained here indicate that opt-in customers may have been more attentive and responsive than opt-out customers, and their energy conservation efforts significantly extend beyond peak reduction during CPP event periods. Such spillover effects may also lead to extra social and environmental benefits.

4 Further Discussion and Concluding Remarks

This paper reports on the result of a randomised field experiment on dynamic pricing programmes. We find that customers in both opt-in and opt-out programmes significantly reduce their peak electricity consumption. Second, the opt-in group succeeded in generating a larger aggregate impact (i.e. the ITT) than the opt-out group. Third, we find that only the opt-in treatment succeeded in triggering significant spillover effects among customers: the opt-in CPP group generated a usage reduction even during shoulder hours before and after the events.

How large is the net effect of active enrolment on customers' subsequent behaviors? We may deduce the lower and upper bounds of its value by using the estimated ITTs and TOTs. To proceed, let us divide the opt-out CPP participants (97.2% of the opt-out group) into two types: 1) active customers (around 63.8% of the opt-out group), who would enroll not only under the opt-out default but also under the opt-in default, and 2) passive customers (around 33.4% (i.e., 97.2% − 63.8%) of the opt-out group), who would only enroll under the opt-out default.[16] Then, the net effect of opt-in enrolment (on active customers) can be written as the difference between the TOT of active customers in the opt-in group and the TOT of active customers in the opt-out group (say, $TOT_{CPPin,Active} - TOT_{CPPout,Active}$), and we can obtain the bounds on this value by considering several interesting cases. First, its lower bound can be obtained by considering the case that the passive customers are unresponsive (i.e., have zero treatment effect) so that the opt-out CPP treatment effect is totally generated by the subgroup of active customers:

$$\begin{aligned} & TOT_{CPPin,Active} - TOT_{CPPout,Active} \\ = \;& TOT_{CPPin} - (ITT_{CPPout}/63.8\%) \\ = \;& -14.7\% - (-5.8\%/63.8\%) = -5.6\%, \end{aligned}$$

i.e., 5.6% reduction in electricity usage during CPP events. Second, its upper bound can be obtained by considering the case that the passive customers are as responsive as the active customers (i.e., the two types of customers have the same treatment effect); in this case,

[16] Note that the random assignment allows the opt-in and opt-out groups to have similar fraction of active and passive customers.

$TOT_{CPPout,Active} = TOT_{CPPout,Passive} = TOT_{CPPout}$ and we obtain:

$$\begin{aligned} & TOT_{CPPin,Active} - TOT_{CPPout,Active} \\ = \ & TOT_{CPPin} - TOT_{CPPout} \\ = \ & -14.7\% + 6.0\% = -8.7\%. \end{aligned}$$

Thus, these calculations suggest that the net effect of opt-in enrolment corresponds to an average percentage reduction of on-peak usage ranging from 5.6% to 8.7% (i.e., 38% to 59% of the opt-in TOT) within the subgroup of active customers.

Libertarian paternalists often advocate that policymakers should select the default option that the majority of people would choose (Thaler and Sunstein, 2003), which typically corresponds to opt-out procedures. Our result suggests that the default option chosen by the majority may not always maximise social efficiency. However, it should not be interpreted as the evidence that the opt-in default is superior to its opt-out counterpart. Indeed, our focus is on the effect of default options on consumers' subsequent behaviors, and we emphasise that the calculation of an optimal default is not straightforward as it may depend on specific characteristics of the policy as well as the heterogeneity among customers (e.g., fraction of active and passive customers); all these factors may vary considerably among different policies. Therefore, the design of policies with preset defaults should be approached with caution, particularly in the case of 'two-stage' policy interventions. The practical examples of such policies could be extensive considering that possible second-stage treatments include not only economic incentives but also non-pecuniary behavioral instruments. For instance, Ferraro et al. (2011) and Ferraro and Price (2013) study three types of non-pecuniary treatments for water conservation: information dissemination on behavioral and technological modifications, appeal for prosocial preferences, and provision of social comparisons. Individuals' attention may also be crucial to the eventual impact of these treatments.

Finally, an important part of the future research agenda could be the long-run persistency of the treatment effects generated under different default options. Allcott and Rogers (2014) show that as the intervention (social comparison by home energy report) is repeated, people gradually develop new 'capital stock' that generates persistent changes in electricity usage. This capital stock might be physical capital such as energy-efficient light bulbs or appliances or 'consumption capital' such as a stock of energy use habits in the sense of Becker and Murphy (1988). In particular, the stock of past conservation behaviors (i.e. rehearsal of conservation behaviors) is likely to lower the future marginal cost of conservation and, thus, facilitate long-term habit formation. Here, the active decision-making process triggered by opt-in-type defaults might positively affect the formation of both physical and consumption capital. For instance, relatively attentive customers might be more likely to replace their home appliances with energy-efficient models. Long-term habit formation could also be more likely to occur among these customers.

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A Online Appendix A (Not for Publication)

A.1 High-frequency Treatment Effects

The estimation results in Sections 3.2 and 3.3 in the paper demonstrate that, on average, the opt-in treatment has a relatively large impact of consumption reduction during both the CPP event period and the time window preceding and following CPP events. However, a potential concern is that although the overall impact of the opt-in CPP group is relatively large, the opt-in treatment effect might have considerable variation and could be larger than the opt-out treatment effect during some parts of the event period but smaller during other parts.

In this section, we study this issue by making use of our high-frequency data. Specifically, we estimate the ITTs and TOTs for each 15-minute time interval during both event and shoulder hours (i.e. 1 pm to 10 pm on treatment days), using a one-hour rolling window to smooth over idiosyncratic variation. For each time index d, we use observations that are 15 or 30 minutes before d, observations on time d, and observations 15 or 30 minutes after d; the panel fixed effects regression for the corresponding ITTs can thus be written as

$$lny_{it} = \sum_{g \in \{CPP_{in}, CPP_{out}\}} \beta^g_{ITT} \cdot I^g_{it} + \theta_i + \lambda_t + \epsilon_{it},\ \forall t \in \{d-30, d-15, d, d+15, d+30\} \quad (1)$$

The corresponding TOTs are estimated using the indicator variable I^g_{it} as an IV.

Figure A1 plots the estimation results of summer high-frequency ITT/TOTs and the corresponding (pointwise) 95% confidence intervals; several important features emerge from the figure. First, it is reassuring to find that the estimated values of the opt-in treatment effects are relatively large throughout the whole estimation period. Second, we observe from the figure that the consumption reduction of both groups gradually increases as CPP events begin, reaches its peak around 5 pm to 6 pm, and gradually backslides afterwards. Third, the treatment effects of the opt-out CPP group seem to be particularly weak during the beginning part of the event period (4 pm to 5 pm). By contrast, opt-in ITTs remain higher than 7% during this time window; such stability in treatment effects may be important from utility companies' or policymakers' perspectives because they can be confident that during any period of the event, a certain level of on-peak usage reduction is expected. Figure A2 plots the results for the winter season, showing that the pattern of these treatment effects is similar to that found in the summer sample, with the opt-in treatment effects being relatively large throughout the whole time window. We also observe that the opt-out ITT/TOT estimates (and corresponding confidence intervals) become slightly positive during some periods of time after the CPP events, indicating that some opt-out customers may have offset the conservation during event hours by increasing usage in adjacent non-event hours. As we discussed at the beginning of Section 3.3, such peak–off-peak load shifting might compromise the overall impact of the intervention.

A.2 Heterogeneous Treatment Effects

In this section, we explore the possible variation of treatment effects across our participants and attempt to gain further understanding on the mechanisms through which the two treatments affect customer behaviours. Our investigation proceeds by analysing the behaviour of households that have electric appliances such as ACs (including centralised and window ACs) and electric heaters. These appliances account for a large proportion of household electricity usage and are more likely to be related to the level of households' willingness or motivation to reduce on-peak consumption compared with other home appliances. Suppose that on a certain summer treatment day, the weather is very hot. If some households' conservation motivation is relatively low, the high temperature may have a negative impact on the willingness of these customers to reduce electricity consumption by adjusting their ACs. Then, we should be able to observe a decrease in the treatment effect among AC holders, particularly among AC holders in the less motivated treatment group. Similar arguments could be made for electric heater owners during the winter experiment because cold weather may have a negative impact on the willingness of customers, particularly less motivated customers, to reduce consumption by adjusting their heaters.

By using the summer analysis for example, we estimate the following panel fixed effects

Figure A1: Summer High-Frequency Treatment Effects

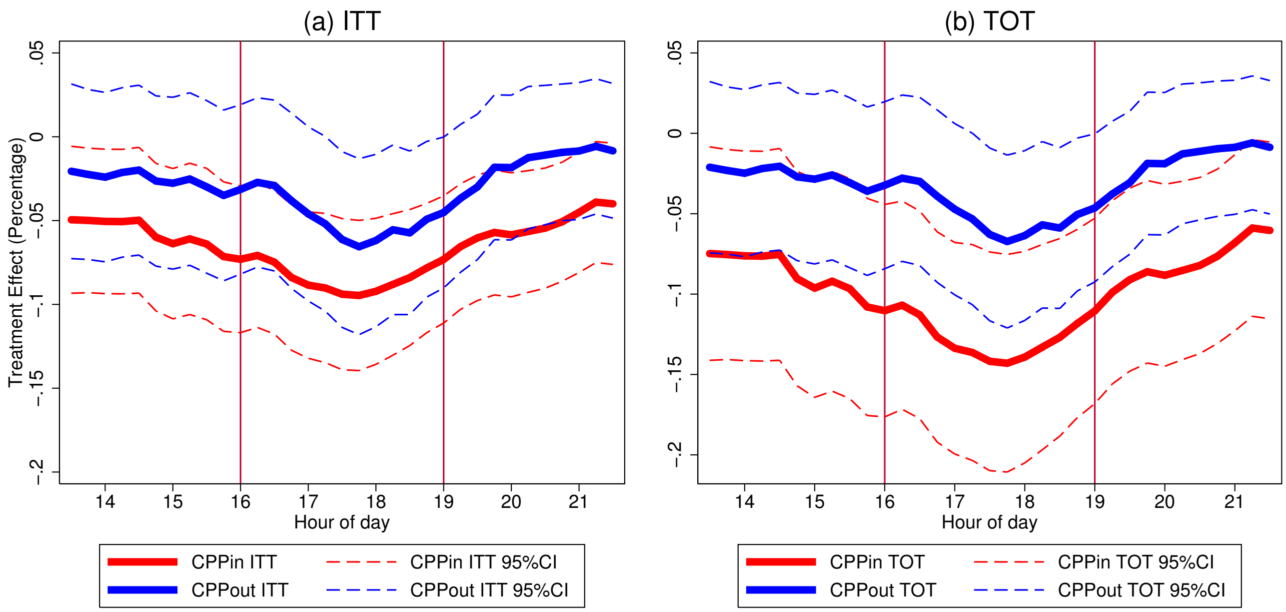
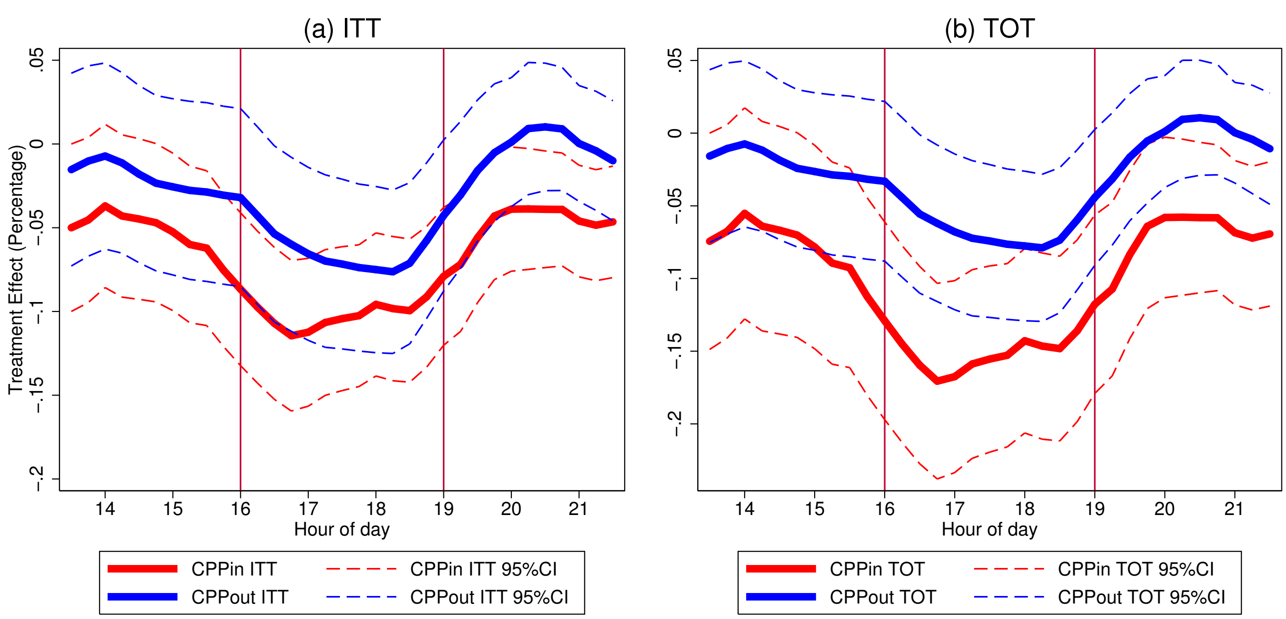


Figure A2: Winter High-Frequency Treatment Effects



model augmented with interaction terms:

$$
\begin{aligned}
lny_{it} = & \sum_{g\in\{CPP_{in},CPP_{out}\}} \beta_1^g \cdot I_{it}^g + \sum_{g\in\{CPP_{in},CPP_{out}\}} \beta_2^g \cdot I_{it}^g \cdot I_{[i,Cooler]} \\
& + \sum_{g\in\{CPP_{in},CPP_{out}\}} \beta_3^g \cdot I_{it}^g \cdot I_{[t,Hot\,Event\,Day]} + \sum_{g\in\{CPP_{in},CPP_{out}\}} \beta_4^g \cdot I_{it}^g \cdot I_{[i,Cooler]} \cdot I_{[t,Hot\,Event\,Day]} \\
& + \theta_i + \lambda_t + \epsilon_{it}
\end{aligned} \tag{2}
$$

where I_{it}^g has the same definition as in Section 3 of the paper. Additionally, we introduce two indicator variables $I_{[i,Cooler]}$ and $I_{[t,Hot\,Event\,Day]}$ for the current analysis. Specifically, $I_{[i,Cooler]}$ equals one if household i owns ACs and zero otherwise, while $I_{[t,Hot\,Event\,Day]}$ equals one if the time interval t is during a treatment day whose temperature is higher than the average temperature of the 14 summer treatment days. Therefore, in this interaction model, β_1^g captures the (conditional) average treatment effect for group g households without ACs on relatively cool treatment days whose temperatures are below the 14-treatment-day average (i.e. the case that $I_{[i,Cooler]} = 0$ and $I_{[t,Hot\,Event\,Day]} = 0$); similarly, $\beta_1^g + \beta_2^g$ corresponds to the case that $I_{[i,Cooler]} = 1$ and $I_{[t,Hot\,Event\,Day]} = 0$, and $\beta_1^g + \beta_3^g$ corresponds to the case that $I_{[i,Cooler]} = 0$ and $I_{[t,Hot\,Event\,Day]} = 1$. Finally, $\beta_1^g + \beta_2^g + \beta_3^g + \beta_4^g$ captures the treatment effect for AC holders on relatively hot treatment days (i.e. $I_{[i,Cooler]} = 1$ and $I_{[t,Hot\,Event\,Day]} = 1$). For the purpose of the current analysis, we are particularly interested in the estimates of β_4^g because these capture the interaction effect between ACs and temperatures on treatment days. Similarly, we can specify the econometric model for the winter analysis as follows:

$$
\begin{aligned}
lny_{it} = & \sum_{g\in\{CPP_{in},CPP_{out}\}} \beta_1^g \cdot I_{it}^g + \sum_{g\in\{CPP_{in},CPP_{out}\}} \beta_2^g \cdot I_{it}^g \cdot I_{[i,Heater]} \\
& + \sum_{g\in\{CPP_{in},CPP_{out}\}} \beta_3^g \cdot I_{it}^g \cdot I_{[t,Cold\,Event\,Day]} + \sum_{g\in\{CPP_{in},CPP_{out}\}} \beta_4^g \cdot I_{it}^g \cdot I_{[i,Heater]} \cdot I_{[t,Cold\,Event\,Day]} \\
& + \theta_i + \lambda_t + \epsilon_{it}
\end{aligned} \tag{3}
$$

where we introduce indicator variables $I_{[i,Heater]}$ and $I_{[t,Cold\,Event\,Day]}$; $I_{[i,Heater]}$ equals one if household i owns electric heaters, while $I_{[t,Cold\,Event\,Day]}$ equals one if time interval t is during a treatment day with a temperature lower than the average temperature of the 15 winter treatment days.

The summer estimation results are presented in Table A1. The estimates of β_1^{CPPin} and β_1^{CPPout} are -7.8% and -5.0%, respectively. In addition, all the signs of β_2^{CPPin}, β_2^{CPPout}, β_3^{CPPin}, and β_3^{CPPout} are negative, indicating that the treatment effects might have a tendency to become slightly larger when households have ACs or when the event day temperature is relatively high; all these coefficients are too small to be statistically significant. Finally, and most interestingly, we find that both β_4^{CPPin} (3.9%) and β_4^{CPPout} (5.8%) are positive, with β_4^{CPPout} being relatively large and statistically significant. Therefore, the estimates of β_4^{CPPin} and β_4^{CPPout} suggest that AC holders, particularly those in the opt-out CPP group, may have generated relatively small on-peak reduction during hot treatment days. This result is consistent with our hypothesis that opt-out households could be less motivated than opt-in households. The winter results are presented in Table A2; the patterns of the estimates of β_4^{CPPin} and β_4^{CPPout} are similar to those in the summer results, although β_4^{CPPout} is not statistically significant.

A.3 Analysis of Participant Characteristics

The usage data of both participants and non-participants allow us to investigate the type of customer likely to participate in our dynamic pricing programme. Here, we assume that individual decisions on whether to participate in the experiment depend on a linear function of customer-specific characteristic variables X_i:

$$Y_i = 1\left(X_i'\delta + v_i \geq 0\right) \tag{4}$$

where Y_i equals one if household i decides to participate in the experiment and zero otherwise. Assuming that v_i is normally distributed, this can be estimated using a probit regression. We construct household-level average usage and the average on-peak/off-peak ratio of usage as the customer characteristic variables using pre-event consumption data. Table A3 reports the estimation result of the probit regression. The estimated coefficient on average usage is positive and statistically significant at the 1% level, while the on-peak/off-peak ratio estimate is insignificant.

Table A1: Heterogeneous Treatment Effects (Cooler)

	(1)
CPPin	-0.078***
	(0.023)
CPPout	-0.050*
	(0.027)
CPPin × 1[Cooler]	-0.029
	(0.029)
CPPout × 1[Cooler]	-0.016
	(0.046)
CPPin × 1[Hot Event Day]	-0.010
	(0.023)
CPPout × 1[Hot Event Day]	-0.006
	(0.025)
CPPin × 1[Cooler] × 1[Hot Event Day]	+0.039
	(0.029)
CPPout × 1[Cooler] × 1[Hot Event Day]	+0.058*
	(0.030)
Household Fixed Effect	Yes
Time Fixed Effect	Yes
Observations	278,391

Notes: This table reports the results of summer heterogeneous treatment effects. Standard errors in parentheses are clustered at the household level to adjust for serial correlation. *, **, and *** show 10%, 5%, and 1% statistical significance, respectively.

Table A2: Heterogeneous Treatment Effects (Heater)

	(1)
CPPin	-0.114***
	(0.033)
CPPout	-0.075**
	(0.032)
CPPin × 1[Heater]	+0.011
	(0.033)
CPPout × 1[Heater]	+0.007
	(0.041)
CPPin × 1[Cold Event Day]	-0.020
	(0.026)
CPPout × 1[Cold Event Day]	-0.014
	(0.031)
CPPin × 1[Heater] × 1[Cold Event Day]	+0.010
	(0.024)
CPPout × 1[Heater] × 1[Cold Event Day]	+0.036
	(0.031)
Household Fixed Effect	Yes
Time Fixed Effect	Yes
Observations	220,272

Notes: This table reports the results of winter heterogeneous treatment effects. Standard errors in parentheses are clustered at the household level to adjust for serial correlation. *, **, and *** show 10%, 5%, and 1% statistical significance, respectively.

Table A3: Selection Probit Model of Participants

Explanatory Variable	(1)	(2)
Average Consumption	0.474***	
	(0.091)	
On-peak/Off-peak Ratio		-0.036
		(0.026)
Observations	1634	1634

Notes: This table reports the result of the estimated marginal effects for the probit model, in which the dependent variable equals one if the household decided to participate in the experiment and zero otherwise. *, **, and *** show 10%, 5%, and 1% statistical significance, respectively.

Table A4: Comparison of Demographic Characteristics

	Participants	LAC	New Mexico	United States
Median household income	US$116,875	US$105,989	US$44,968	US$53,482
Bachelor's degree or higher (age 25+)	72.3%	64.0%	26.1%	29.3%
Persons under 18 years old	N/A	23.3%	24.1%	23.1%
Persons over 65 years old	N/A	16.6%	15.3%	14.5%
Number of persons per household	N/A	2.38	2.66	2.63

Notes: This table reports households' characteristics for the experiment participants and the population in LAC, New Mexico and the United States. The data for the experiment participants are taken from our household survey. The data for the population in LAC, New Mexico and the United States are taken from the 'State and County Quick Facts' of the US Census Bureau.

A.4 Demographic Characteristics of LAC Households

LAC households have relatively high education and income levels compared with other regions in the United States; as shown in Table A4, the percentage of people (aged 25 years and older) with a bachelor's degree or higher in LAC is 64%, whereas the percentage of people with a bachelor's degree or higher in New Mexico and the United States is 26.1% and 29.3%, respectively. Moreover, the median household income of LAC is US$105,989 while the median household income of New Mexico and the United States is US$44,968 and US$53,482, respectively. However, LAC households are similar to households in New Mexico and the United States in terms of other demographic characteristics such as age and household size. We also note that the total number of households residing in LAC is 7,495. These data are taken from the 'State and County Quick Facts' of the US Census Bureau. In addition, we note that compared with the whole population in LAC, our experiment participants have slightly higher income and education levels: their median household income is US$116,875 and the percentage of people (aged 25 years and older) with a bachelor's degree or higher is 72.3%.