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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, and the opt-in treatment effects are relatively more persistent over repeated interventions. 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 interventions. Moreover, only the opt-in treatment has significant effects beyond the treatment time window. Our results, therefore, highlight the important difference between an active and a passive decision-making process. We also estimate a marginal treatment effect model to inform the external validity of our experiment.

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), car insurance plan choices (Johnson et al., 1993), and green product purchases (Pichert and Katsikopoulos, 2008; Ebeling and Lotz, 2015). Most of these studies advocate for policies with opt-out defaults (i.e. automatic enrolment defaults); e.g., see Jachimowicz et al. (2019).[1]

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

[1] Beyond the discussion on the default option, the existence of active decisions, where consumers or employees are required to make explicit choices, is known to affect 401(k) enrolment (Carroll et al., 2009) or purchase of health insurance (Handel, 2013).

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%).

Third, we find that the opt-in treatment effects are relatively more persistent over repeated interventions, while the opt-out treatment effects seem to have patterns of habituation and dishabituation similar to those found in Ito et al. (2018). Moreover, we find that among the two treatment groups, only the opt-in group generated significant consumption reductions during the time window preceding and following peak hours on treatment days. These results also suggest 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).

Finally, we estimate a marginal treatment effect (MTE) model following Brinch et al. (2017), and the estimation results suggest that there exists substantial treatment effect heterogeneity among the CPP participants. Furthermore, the MTE model allows us to perform extrapolation and compare our results with those obtained in Fowlie et al. (2017), who conducted a similar CPP experiment in the Sacramento Municipal Utility District (SMUD). We find that although the two experiments are quite different in terms of experiment sites and recruitment procedures, the degrees of treatment effect heterogeneity with respect to households' resistance to CPP enrolment may be rather similar. From the standpoint of external validity, such similarity also suggests that our results are not obtained from a very extreme environment.

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.[2] 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. In Section 4 we discuss the external validity of our experiment, and we conclude in Section 5.

2 Research Design and Data

2.1 Experiment Overview

The 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

[2]Another solution is to assist or nudge households to reduce on-peak electricity usage (Jessoe and Rapson, 2014; Ito, Ida, and Tanaka, 2018; Brandon et al., 2019).

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 et al., 2018). 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.[3] 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.

[3]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.

We also collect household data from surveys and temperature data from the National Climatic Data Center (NOAA 2013-2014). 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.

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.[4]

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.[5] Table 1 demonstrates the structure of the LAC standard flat rate and CPP rate.

[4]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.

[5]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

Figure 1: Experiment Timeline

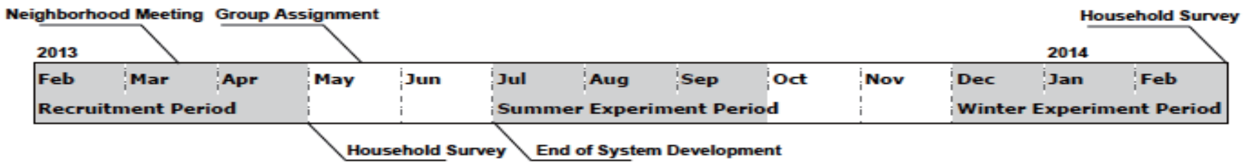
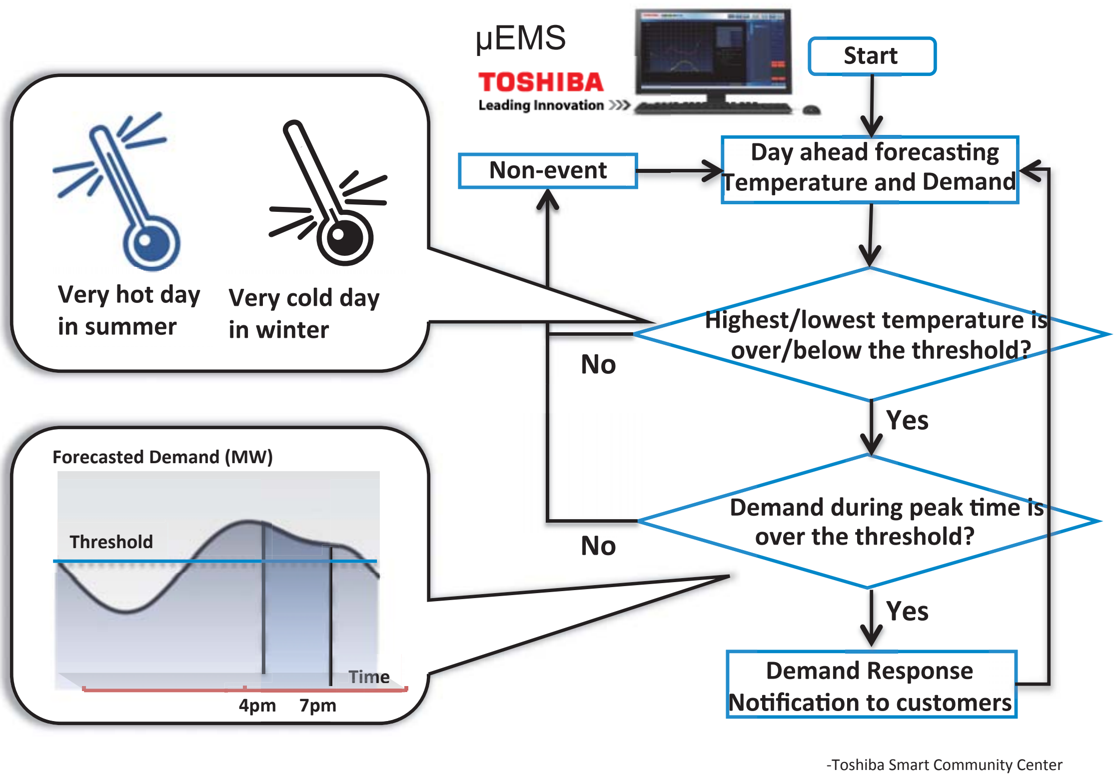


Table 1: Pricing Schemes

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.

Figure 2: Algorithm for Demand-Response Event Days



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.[6] Some attrition occurred 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:[7]

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.

the summer and winter seasons of 2012 were used for the calculation of revenue neutrality.

[6]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).

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

Table 2 presents the descriptive statistics of the on-peak and off-peak usage preceding the first CPP event and appliance ownership for each group. Each column shows the mean and standard deviation of the 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[8] 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:

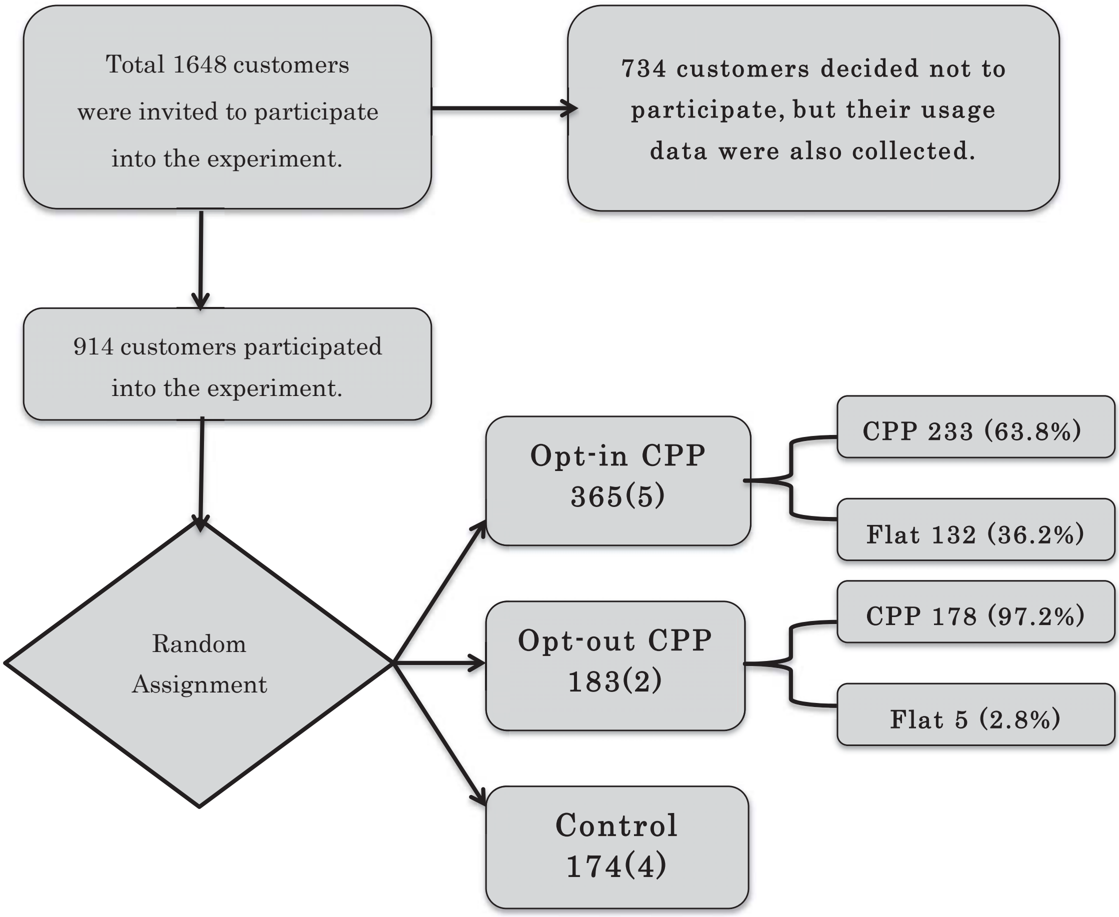
'Points on DR day (mm/dd) = X_1. Cumulative = X_2 including non-DR days = X_3.'

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."

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

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 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 policymakers 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.[9]

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[10].

[9] If we also take the non-participant households 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.

[10] 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.

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.

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.1428-1429), 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:

$$lny_{it} = \sum_{g \in \{CPP_{in}, CPP_{out}\}} \beta^g_{ITT} \cdot I^g_{it} + \theta_i + \lambda_t + \epsilon_{it} \tag{1}$$

where the indicator variable I_{it}^{g} 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 variables of interest are the indicators I_{it}^{g}, and the coefficients β_{ITT}^{g} 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:

[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 $exp(\beta_{ITT}^{g}) - 1$.

[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.

$$lny_{it} = \sum_{g \in \{CPP_{in}, CPP_{out}\}} \beta^g_{TOT} \cdot T^g_{it} + \theta_i + \lambda_t + \epsilon_{it} \quad (2)$$

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}.

3.2 Average Treatment Effects during the Event Periods

The column (1) in Table 4 labelled 'ITT' report the results from the overall ITT estimators of each treatment group, combining the sample and winter samples. 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 conventional economic theory in which agents rationally maximize their payoff would predict that the opt-out CPP group generates higher ITTs because the opt-out CPP group 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: 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

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

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. 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 may had an impact on customers' subsequent behavior and made them relatively more responsive during the CPP event periods.

The column (2) in Table 4 labelled 'TOT' report the overall 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 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, these results suggest that the opt-in and opt-out defaults may have distinct subsequent effects on customers' elasticity during treatment periods. Furthermore, we compute the effects sizes of the two treatments by using the formula of Cohen's d. The obtained effect size of the opt-in CPP group is 0.249, while that of the opt-out CPP group is 0.105.

Columns (3)-(6) in Table 4 report estimated average treatment effects for summer and winter separately. The results for summer are presented in (3)-(4) and those for winter are presented in (5)-(6), and they have a similar pattern as those in (1)-(2). 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.

Furthermore, to investigate how households' response to the CPP events evolves over repeated interventions, we divide the treatmen days into 6 cycles (3 summer cycles with 5, 5, and 4 treatment days, and 3 winter cycles with 5 treatment days for each cycle). The estimated average treatment effects of the opt-in and opt-out groups for the 6 cycles are reported in Table 5, where Panel A present the ITT results and Panel B presents the TOT results.

First, we note that the estimated coefficients of the opt-in group are larger in absolute value than the corresponding coefficients of the opt-out group for all the cycles, with the opt-in ITTs ranged from -0.071 to -0.134 while the opt-out ITTs from -0.032 to -0.081. Second, the opt-out ITT is relatively high (-0.081) at the beginning of the summer experiment, but decreases in the following 2 cycles (-0.032 and -0.041). Then, the treatment effect is restored back towards the original level (-0.066) with the beginning of the winter experiment. Such pattern is similar to the finding of habituation and dis-habituation in Ito et al. (2018, p.255). By contrast, the treatment effects of the opt-in group seem to be more persistent over repeated interventions.

Interestingly, we also note that the opt-in group generated very high average treatment effect near the end of the experiment (with an ITT equal to -0.134), while such pattern is not observed for the opt-out group (with an ITT equal to -0.057). One explanation is that with the increase of temperature (the 6th cycle is in February while the 4th and 5th cycles are in

Table 4: Average Treatment Effects during the Event Periods

	Overall		**Summer**		**Winter**	
Treatment Groups	ITT	TOT	ITT	TOT	ITT	TOT
	(1)	(2)	(3)	(4)	(5)	(6)
CPPin	-0.098***	-0.147***	-0.087***	-0.131***	-0.104***	-0.156***
	(0.016)	(0.025)	(0.020)	(0.030)	(0.019)	(0.029)
CPPout	-0.058***	-0.060***	-0.051**	-0.052**	-0.066***	-0.068***
	(0.020)	(0.021)	(0.024)	(0.024)	(0.023)	(0.024)
P-value[CPPin = CPPout]	0.029**	0.000***	0.086*	0.002***	0.089*	0.002***
Household Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	584,616	584,616	319,716	319,716	264,900	264,900

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). 'ITT' and 'TOT' show the estimation results for the intention-to-treat and the treatment-on-the-treated of each treatment group, respectively. The columns (1)-(2) report the overall estimation results, the columns (3)-(4) report the summer estimation results, and the columns (5)-(6) report the winter estimation results. Standard errors in parentheses are clustered at the household level to adjust for serial correlation. *, **, and *** show 10%, 5%, and 1% statistical significance, respectively.

December and January, respectively), households in the opt-in group may have become more willing to reduce electricity consumption by turning down/off air conditioners or heaters than the opt-out group. This also suggests that the opt-in group might have been relatively more attentive and responsive. Another possibility is that the opt-in group might have been more clearly aware of the schedule of the experiment and the number of treatment days left than the opt-out group, and used the last cycle of CPP events to gain more incentive points. However, this alternative explanation also points to the scenario in which the opt-in group might have been more attentive.

Table 5: Persistency of Average Treatment Effects during the Event Periods

	Summer			**Winter**		
Treatment Groups	1st Cycle	2nd Cycle	3rd Cycle	4th Cycle	5th Cycle	6th Cycle
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: ITT Estimates						
CPPin	-0.108***	-0.071***	-0.082***	-0.090***	-0.090***	-0.134***
	(0.027)	(0.025)	(0.032)	(0.021)	(0.028)	(0.029)
CPPout	-0.081**	-0.032	-0.041	-0.066***	-0.073**	-0.057*
	(0.032)	(0.030)	(0.036)	(0.025)	(0.033)	(0.033)
Household Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	319,716	319,716	319,716	264,900	264,900	264,900
Panel B: TOT Estimates						
CPPin	-0.158***	-0.109***	-0.127***	-0.134***	-0.134***	-0.200***
	(0.039)	(0.038)	(0.049)	(0.032)	(0.041)	(0.043)
CPPout	-0.083**	-0.032	-0.042	-0.069***	-0.076**	-0.059*
	(0.033)	(0.031)	(0.037)	(0.026)	(0.034)	(0.034)
Household Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	319,716	319,716	319,716	264,900	264,900	264,900

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) for the 6 treatment cycles, with 3 cycles for the summer treatment days and 3 cycles for the winter treatment days. Panel A reports the ITT results and Panel B reports the TOT results. 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 Average Treatment Effects before and after the Event Periods

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 we even observe the opt-in CPP reduction of on-peak electricity usage during the hours preceding and following the event period. This result is highlighted in Table 6. In columns (1)-(2) and (5)-(6), we present the estimated average treatment effects of both treatment groups during the shoulder hours (i.e., 1 pm - 4 pm, the three hours before the event period, and 7 pm - 10 pm, 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 or after the on-peak time window.

We find that the opt-in CPP group generated a 5.4% usage reduction in terms of the ITT (column (1)) during the time window before the CPP events and a 4.8% reduction during the time window after the events (column (5)), with both coefficients being statistically different from zero at the 1% significance level. By contrast, we do not find such significant reduction 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 (columns (1) and (5)) for the shoulder hours before and after the CPP events, respectively. Not surprisingly, the corresponding TOTs of the opt-in group reported in columns (2) and (6) (8.1% before the events and 7.2% after the events) are much larger than those of the opt-out group. A potential explanation of such reduction during off-peak periods is risk aversion: the CPP customers might choose to reduce their energy consumption in the hours before and after the events because of the fear that inattentively using energy during the on-peak periods would result in a massive bill. However, as pointed out by a referee, risk aversion cannot explain the difference in consumption reduction between the two CPP groups during these periods.

For robustness check, we also estimate the ITTs and TOTs using the data from 6 am to 4 pm and 7 pm to 6 am, and the results are reported in columns (3)-(4) and (7)-(8) in Table 6. The results have very similar patterns and only the ITT estimates of the opt-in CPP group are statistically significant. The estimated coefficients are relatively small in absolute value compared with those using the data from 1 pm to 4 pm and 7 pm to 10 pm, suggesting that the difference in electricity usage between the treatment and control groups is relatively small during late night and early morning.

In summary, similar to those in the previous section, the results in Table 6 indicate that opt-in customers may have been more attentive and responsive than opt-out customers, and their energy conservation efforts extend beyond peak reduction during CPP event periods.

4 Discussion on External Validity

External validity of the estimation results obtained from an experiment is always an important consideration. In this section, we examine further households' consumption characteristics and treatment effect heterogeneity to inform external validity. In particular, by using a marginal treatment effect (MTE) model we perform extrapolation to obtain the local average treatment effects (LATEs) of always takers and compliers of our experiment but with the same CPP enrolment rates as those in Fowlie et al. (2017).

4.1 Analysis with Consumption Characteristics

First, to understand further the consumption characteristics (usage and load profile) of customers who actively chose to opt in (i.e., always takers), we estimate a probit model where individual decisions on whether to opt in depend on a linear function of certain characteristic

Table 6: Average Treatment Effects before and after the Event Periods

	Before Events				After Events			
	1 pm - 4 pm		**6 am - 4 pm**		**7 pm - 10 pm**		**7 pm - 6 am**	
Treatment Groups	ITT	TOT	ITT	TOT	ITT	TOT	ITT	TOT
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CPPin	-0.054***	-0.081***	-0.036***	-0.054***	-0.048***	-0.072***	-0.026***	-0.039***
	(0.019)	(0.028)	(0.014)	(0.021)	(0.013)	(0.020)	(0.009)	(0.014)
CPPout	-0.019	-0.020	-0.013	-0.013	-0.003	-0.003	-0.003	-0.003
	(0.021)	(0.022)	(0.016)	(0.017)	(0.015)	(0.016)	(0.010)	(0.011)
P-value[CPPin = CPPout]	0.069*	0.011**	0.110	0.025**	0.001***	0.000***	0.014**	0.002***
Household Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	633,539	633,539	1,949,360	1,949,360	584,784	584,784	2,144,152	2,144,152

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, 6 am to 4 pm) or following (7 pm to 10 pm, 7 pm to 6 am) 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 before the events and 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.

variables X_i:

$$Y_i = 1\left(X_i'\delta + v_i \geq 0\right) \tag{3}$$

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 of the opt-in CPP group.

In particular, we want to know whether customers with relatively low on-peak/off-peak ratios, so-called 'structural winners',[14] 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 enroled households turn out to be structural winners, the overall impact of the opt-in treatment could be compromised. The estimation result is reported in column (1) of Table 7, 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. The finding suggests that in the opt-in CPP group, 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. As pointed out by a referee, this is also consistent with the scenario that the opt-in enrolment procedure may have made the opt-in CPP households more attentive.

In addition, the availability of the usage data of both participants and non-participants during the experiment allows us to investigate the type of customer who are more likely to participate in our randomized experiment. For this analysis, we use only the data from the households in the control group and the non-participant households because of the concern that the CPP customers' behaviour may have been affected by the dynamic pricing events called during the experimental period.[15] Column (2) of Table 7 reports the estimation result

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

[15] Because treatment group observations are not used in the current probit regression, the control group observations are weighted by using sampling weights equal to the inverse of the percentage of control group households among participant households to preserve balance.

Table 7: Analysis of Consumption Characteristics

Explanatory Variable	(1)	(2)
Average Consumption	0.149	0.633***
	(0.182)	(0.136)
On-peak/Off-peak Ratio	0.132*	-0.018
	(0.076)	(0.014)
Observations	365	908

Notes: Column (1) 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. Column (2) reports the result of the marginal effects for the probit model, in which the dependent variable equals one if household decided to participate in the experiment and equals zero otherwise. *, **, and *** show 10%, 5%, and 1% statistical significance, respectively.

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. This result suggests that households with relatively high usage were more likely to participate into the experiment. One explanation is that such households may be more concerned about the possibility of having large waste of electricity in daily use, thus more interested in the dynamic pricing program.

To further inform the external validity of the experiment, we report the average characteristics for the always takers, compliers, and never takers in Table 8. We define the always takers as those who will choose the CPP even under the opt-in enrolment, the compliers as those who will choose the CPP only under the opt-out enrolment (but not under the opt-in enrolment), and the never takers as those who will not choose the CPP even under the opt-out treatment. Note that the average characteristics for the always takers and never takers can be obtained directly from data. For the compliers, we follow Kowaloski (2016, p.11, Section 3.1) and compute their average characteristics by taking advantage of the random assignment across opt-in and opt-out groups. In particular, based on the CPP enrolment rates reported in Table 3, the shares of the always takers, compliers, and never takers are given by 63.8%, 33.4% (i.e., 97.2% - 63.8%), and 2.8% (i.e., 100% - 97.2%), respectively. We emphasise that the following important assumption is needed for this result: the opt-in and opt-out groups have the same shares of always takers, compliers, and never takers. This assumption is plausible in the current context because of the random assignment, which implies that on average the shares of each type will be the same across the two groups. In Table 8, the columns under 'Mean' report the average characteristics and those under 'P-value' report the p-values for testing the equality of average characteristics between the always takers and never takers, always takers and compliers, compliers and never takers, respectively. We observe that the average pre-event on-peak usage of the always takers is relatively high compared with the compliers and never takers, while the average pre-event off-peak usage of the never takers is relatively low compared with the other two groups. However, these differences are not statistically significant. The differences in terms of household appliances are also statistically insignificant across the always

Table 8: Average Characteristics of Always Takers, Compliers, and Never Takers

	Mean			P-value		
Variables	AT	C	NT	AT-NT	AT-C	C-NT
Pre-event on-peak usage (kWh/h)	1.09	0.91	0.90	0.48	0.40	0.98
Pre-event off-peak usage (kWh/h)	0.80	0.85	0.68	0.50	0.70	0.42
Number of refrigerators	1.30	1.50	1.66	0.19	0.23	0.60
Number of desktop computers	1.12	0.96	1.33	0.45	0.48	0.27
Number of sprinkler systems	0.39	0.43	0.33	0.83	0.85	0.77

Notes: This table reports average characteristics by customer type. AT, C, and NT denote always takers, compliers, and never takers, respectively. The columns under 'Mean' report the average characteristics of each type. The columns under 'P-value' report the *p*-values of testing the null hypothesis that there is no difference in average characteristics between AT and NT, AT and C, C and NT, respectively.

takers, compliers, and never takers.[16]

4.2 Analysis with Marginal Treatment Effects

In this section, we examine treatment effect heterogeneity by estimating a MTE model and then perform extrapolation to compare the results obtained in our experiment with those in Fowlie et al. (2017), who conducted a similar CPP experiment with a much larger sample. In particular, while the opt-out CPP enrolment rate in their experiment is similar to ours, their opt-in CPP enrolment rate is around 20%, and the overall impact (i.e. in terms of ITT) of the opt-out assignment is larger than that of the opt-in assignment. Then from the standpoint of external validity, it is natural to ask if our opt-in CPP enrolment rate were also around 20%, would our estimation results be in contradiction to those obtained in Fowlie et al. (2017). We shed light on this question by obtaining the LATEs of our experiment participants under the same enrolment rates as those in Fowlie et al. (2017).

[16] Average characteristics of some home appliances listed in Table 2 are not reported here because there is no observation or variation for the never takers.

4.2.1 MTE Model

To estimate the MTE, we formulate a household's choice using the generalized Roy model. Let Y_1 be the potential outcome of a household (in term of natural log of the on-peak usage on event days) when enroled into the CPP tariff ($D = 1$) and Y_0 be its potential outcome when not enroled ($D = 0$). The observed outcome (Y) can therefore be represented by a combination of the potential outcomes and enrolment status:

$$Y = (1 - D)Y_0 + DY_1.$$

Following the MTE literature (e.g., Heckman and Vytlacil (2005)), we specify the potential outcome as $Y_j = \mu_j + U_j$, where $j = 0$ or 1, and U_j are random variables for which we normalize $E[U_j] = 0$. Furthermore, a household enrols into the CPP tariff if the net benefit of the enrolment (I_D) is positive:

$$I_D = \mu_D(Z) - U_D > 0, \tag{4}$$

where $\mu_D(\cdot)$ is an unspecified function, Z is an instrument that we define below, and U_D is a continuous random variable with a strictly increasing distribution function. In addition, the marginal distribution of U_D can be normalized to a uniform distribution on the unit interval, and then $\mu_D(Z)$ represents a propensity score: $P(Z) \equiv P(D = 1|Z) = \mu_D(Z)$.

Here U_D is interpreted as the unobservable resistance to enroling in the CPP tariff. Under a standard economic model that assumes consumers make fully-informed decisions, such resistance corresponds to unobservable switching costs (see, e.g., the first term of equation (3) in Section 7.1 of Fowlie et al. (2017)). Alternatively, it could be associated with households' degree of inattention to the CPP tariff, which Fowlie et al. (2017) investigate as a potential explanation of the default effects. For example, some households may have relatively high values of U_D because they are less attentive to the new tariff, thus being less likely to enrol in the new tariff. Such households may also be relatively less responsive during the CPP event periods and thus generate low treatment effects.

Following Brinch et el. (2017), we express the conditional expectations of U_1 and U_0 as:

$$k_j(p) = E[U_j|U_D = p], j = 0 \, \text{or} \, 1,$$

and the MTE can then be defined as:

$$MTE(p) = E[Y_1 - Y_0 | U_D = p] = \mu_1 - \mu_0 + k_1(p) - k_0(p). \tag{5}$$

In our context, the MTE captures the average treatment effect in the second stage of the experiment, conditional on the value of unobservable resistance to CPP enrolment ($U_D = p$) in the first stage of the experiment.[17] In particular, we are interested in whether the value of the MTE increases or decreases with the value of p (i.e., whether the second-stage treatment effect increases or decreases with the level of resistance).

We use a separate estimation approach proposed by Brinch et al. (2017, p.993) to identify the MTE in the case of a binary instrument Z. With this approach, each component of the MTE model is separately estimated by the following linear specification:

$$k_1(p) = \alpha_1 p - \frac{1}{2}\alpha_1, \text{ and } k_0(p) = \alpha_0 p - \frac{1}{2}\alpha_0.$$

Then the MTE function is given by $MTE(p) = \mu_1 - \mu_0 + \frac{1}{2}(\alpha_1 - \alpha_0) - p(\alpha_1 - \alpha_0)$. In addition, using the expressions of $k_1(p)$ and $k_0(p)$, the mean outcomes given the value of propensity score and treatment status can be written as

$$E\left[Y | P(Z) = p, D = 1\right] = E\left[Y_1 | U_D < p\right] = \mu_1 + K_1(p), \tag{6}$$

where the first equality follows from the selection equation (4) while $K_1(p) = E\left[U_1 | U_D < p\right] = \frac{1}{p}\int_0^p k_1(u)du = \alpha_1(p-1)/2$, and

$$E\left[Y | P(Z) = p, D = 0\right] = E\left[Y_0 | U_D \geq p\right] = \mu_0 + K_0(p), \tag{7}$$

where $K_0(p) = E\left[U_0 | U_D \geq p\right] = \frac{1}{1-p}\int_p^1 k_0(u)du = \alpha_0 p/2$.

The identification of the separate estimation approach is based on equations (6) and (7), and both equations are linear in p. To proceed, we focus on the samples of the opt-in and opt-out treatment groups, and define the binary instrument Z, where $Z = 1$ if the household was assigned to the opt-out group and $Z = 0$ if assigned to the opt-in group. With the instrument Z, the empirical analogues of $E[Y|P(Z) = p, D = 1]$ and $E[Y|P(Z) = p, D = 0]$ are observed

[17] Since conditioning on $U_D = p$ is equivalent to conditioning on the intersection of $P(Z) = p$ and $I_D = 0$, the MTE can also be interpreted as the average effect of treatment for those who are on a margin of indifference between participation in treatment and nonparticipation.

for two different values of propensity score p. More specifically, in the context of our experiment, the opt-in group samples provide the empirical analogues of $E[Y|P(Z) = 0.64, D = 1]$, which corresponds to the average treated outcome of always takers, and $E[Y|P(Z) = 0.64, D = 0]$, which corresponds to the average untreated outcome of compliers and never takers. The opt-out group samples provide those of $E[Y|P(Z) = 0.97, D = 1]$, the average treated outcome of always takers and compliers, and $E[Y|P(Z) = 0.97, D = 0]$, the average untreated outcome of never takers. As a result, we can identify μ_1, μ_0, α_1, and α_0 in equations (6)-(7) and obtain the MTE estimates.

4.2.2 MTE Estimation Results

Table 9 summarizes the MTE estimates obtained by the separate estimation approach. The table shows estimates of the intercept and the slope of the linear MTE model as well as its underlying components (namely, $\mu_1+K_1(p)$, $\mu_0+K_0(p)$, $\mu_1+k_1(p)$ and $\mu_0+k_0(p)$) at both values of propensity score. Estimates of the MTE function are presented in the last two columns of the last row. The linear MTE function is estimated as $MTE(p) = -0.360 + 0.599p$, with a negative intercept and a positive slope.

The estimation result suggests that there exists remarkable treatment effect heterogeneity. Furthermore, according to the definition of MTE in (5), the positive slope coefficient suggests that the unobservable resistance to CPP enrolment in the first stage of the experiment is associated with the second-stage outcome of usage reduction, and households with relatively low levels of resistance (i.e., low values of p) have generated relatively significant treatment effect. When the level of resistance to CPP enrolment increases (i.e., when the value of p increases), the magnitude of the treatment effect decreases.

Now to study the external validity of our experiment, we perform extrapolation for $LATE_{AT}$, the local average treatment effect of always takers, and $LATE_C$, the local average treatment effect of compliers, using the estimated MTE model in Table 9 but with the values of the propensity score p (the CPP enrolment rates) different from our experiment. We consider the case where the values of p are the same as those in Fowlie et al. (2017): 0.20 for the opt-in group and 0.96 for the opt-out group, respectively. The first row of Table 10 shows the values of $LATE_{AT}$ and $LATE_C$ in their paper, and the second row shows the result of extrapolation using our estimated MTE function: $(LATE_{AT}, LATE_C) = (-0.299, -0.012)$. Interestingly, comparing the two rows in Table 10, we observe that by moving our estimated linear MTE function downward by 0.12, we can obtain LATEs very similar to those in Fowlie et al. (2017). That is, the MTE slope coefficients of the two experiments turn out to be quite similar, suggesting that the marginal change in treatment effect with respect to households' resistence to selection into CPP could be similar between the two experiments (i.e., the degrees of treatment effect heterogeneity with respect to the resistence could be similar).

In sum, the MTE analysis in this section provides two major insights. First, there is remarkable treatment effect heterogeneity among our CPP participants, and the impact of the CPP tariff on electricity consumption reduction decreases when the level of unobservable resistance to enrol increases. This is in line with the ITT and TOT results obtained in Section 3. Namely, the opt-out procedure is able to enrol the households who would not enrol if assigned to the opt-in group but these housedholds (i.e., the compliers) typically generate relatively low treatment effects. Second, the extrapolation analysis indicates that our results are not in contradiction to those in Fowlie et al. (2017) if our opt-in enrolment rate were as low as theirs. This could be surprising considering that the two papers are quite different in terms of both experiment sites (thus potential for consumption reduction) and recruitment procedures (RCT procedure vs RED procedure). Nonetheless, the values of marginal change in treatment effect with respect to household's resistance to CPP enrolment seem to be rather similar for the two experiments. From the point of view of external validity, these results also suggest that our estimates are not obtained from a very extreme environment.

5 Concluding Remarks

This paper reports on the result of a 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, and the opt-in treatment effects are more persistent over repeated interventions. Third, we find that only the opt-in treatment succeeded in triggering significant treatment effects among customers during hours before and after the events. In

Table 9: Estimates of Linear MTE Model

Linear MTE Model:	$p = 0.64$	$p = 0.97$	Intercept	Slope
$\mu_1 + K_1(p) = E[Y_1 \mid U_D < p]$	-0.081	-0.044	-0.164	$0.124p$
	(0.003)	(0.004)	(0.013)	(0.016)
$\mu_0 + K_0(p) = E[Y_0 \mid U_D \geq p]$	-0.096	-0.150	0.020	$-0.175p$
	(0.005)	(0.026)	(0.058)	(0.086)
$\mu_1 + k_1(p) = E[Y_1 \mid U_D = p]$	0.001	0.077	-0.164	$0.249p$
	(0.009)	(0.018)	(0.013)	(0.032)
$\mu_0 + k_0(p) = E[Y_0 \mid U_D = p]$	-0.038	-0.144	0.196	$-0.351p$
	(0.030)	(0.023)	(0.144)	(0.172)
$MTE(p) = E[Y_1 - Y_0 \mid U_D = p]$	0.039	0.222	-0.360	$0.599p$
	(0.032)	(0.029)	(0.145)	(0.174)

Notes: This table reports the estimation results of the linear MTE model and its components. Standard errors in parentheses are computed by nonparametric bootstrap with 1000 bootstrap replications.

Table 10: Comparison of LATEs

	Always Taker	Complier
Fowlie et al.	-0.424	-0.124
Our Extrapolation	-0.299	-0.012

Notes: This table compares the estimates of LATEs of the always takers and compliers in Fowlie et al. (2017) and those of our extrapolation from the linear MTE model with the value of propensity scores equal to 0.20 and 0.96.

addition, concerning the extenal validity of our experiment, we study customer characteristics and estimate a MTE model, which allows us to perfom extrapolation and compare treatment effect heterogeneity with similar experiment.

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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Appendices

A Analysis of the Non-treatment Days

In this section, we report the results of some further analysis on household usage of the opt-in and opt-out groups during the non-treatment days (i.e., the days on which dynamic pricing events were not called). Table A.1 presents the results of differences in mean comparing the treatment groups and the control group with four specifications: column (1) for all hours on non-treatment days, column (2) for 6 am - 4 pm on non-treatment days ('Before Peak'), column (3) for 4 pm - 7 pm on non-treatment days ('On Peak'), and column (4) for 7 pm - 6 am on non-treatment days ('After Peak'). As we can see from the table, the opt-in CPP group has consumption reduction during all the periods on non-treatment days, although the coefficients are not statistically significant. The opt-out CPP group also has some slight reduction during the 'On Peak' and 'After Peak' periods, but the values of the coefficients are much smaller than those of the opt-in group.

Table A.1: Comparison of Household Usage on Non-treatment Days

Treatment Groups	All Hours	Before Peak (6 am - 4 pm)	On Peak (4 pm - 7 pm)	After Peak (7 pm - 6 am)
	(1)	(2)	(3)	(4)
CPPin	-0.015	-0.012	-0.015	-0.018
	(0.012)	(0.012)	(0.015)	(0.012)
CPPout	-0.0002	0.003	-0.003	-0.002
	(0.014)	(0.014)	(0.017)	(0.014)
Observations	2,728,608	1,136,920	341,076	1,250,612

Notes: This table reports the results of differences in mean between the treatment groups and the control group on non-treatment days. Column (1) reports the result for all hours, column (2) reports the result for the period from 4 pm to 7 pm ('On Peak'), column (3) reports the result for the period from 6 am to 4 pm ('Before Peak'), and column (4) reports the result for the period from 7 pm to 6 am ('After Peak'). Standard errors in parentheses are clustered at the household level.

B Demographic Characteristics of LAC Households

We note that households in the Los Alamos County (LAC) have relatively high education and income levels compared with other regions in the United States; as shown in Table A.2, 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. Households with relatively high education levels might be more interested or open to new technologies such as dynamic pricing programs. 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. 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 (column (1) vs column (2)) 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%.

Table A.2: Comparison of Demographic Characteristics

	Participants (1)	LAC (2)	New Mexico (3)	United States (4)
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 (column (1)) are taken from our household survey. The data for the population in LAC, New Mexico and the United States (columns (2) - (4)) are taken from the 'State and County Quick Facts' of the US Census Bureau.

Customer Education Materials

Draft for Review

Project Overview

The Los Alamos County Department of Public Utilities is proud to announce the installation of 1,700 smart meters at residences in North Mesa and Barranca Mesa as a part of the U.S./Japan Smart Grid demonstration project. These meters will allow our customers to see their energy usage in near-real time increments, giving them better control of household electric bills. In addition, the new meters will mark the beginning of an infrastructure build-out that will eventually allow for remote updates on outages, decreasing the time to restore power. [FOR WEB COPY ONLY: Learn more about the new smart meters **here**.]

LAC SMART METER PROJECT RESEARCH STUDY

Customers in these areas receiving a new meter will also be invited to participate in a voluntary research study. The study is separate from their normal electric bill and looks at demand response pricing programs. In return, participants can earn cash incentives for completing the survey.

STUDY GOALS

Demand response pricing programs incentivize customers to reduce their energy consumption when the utility sees or expects higher levels of energy use from its customers. Known as “peak” times, the study will set out to determine how customers respond to these peaks and the ensuing “demand response events.” These pricing programs are being studied to determine whether they encourage customers to reduce their household energy use during demand response events to help manage the increased strain on the electric grid and prevent load-related outages.

There are two types of “dynamic” pricing models we are studying:

Critical Peak Pricing (CPP): This type of pricing is based on the idea that using electricity during a peak time (for example, between 4 and 7 p.m., when everyone is coming home from work and school) will be higher than an off-peak time, when there are less people using electricity. In this model, customers can save energy by avoiding or reducing use during a peak-time price increase, thereby lowering their bill.

Peak Time Rebate (PTR): This pricing is calculated based upon a household’s previous consumption during a peak period. If the household uses less energy during the next peak period than they did in the previous one, they would receive a price incentive, thereby lowering their bill.

The research study will use "virtual" pricing groups in conjunction with customer notifications to determine responses to demand response events. Our partners will review each meter's data for usage changes, based on the customer's virtual pricing group. In turn, the customer will be able to earn points during demand response events for reducing use. The points will equal cash incentives.

To reiterate, this virtual pricing system is part of the research program only, and will not be tied to the customer's bill. Each customer will still receive a normal bill from LAC. However, reducing use for the "virtual" points could also help reduce the actual electric bill.

STUDY GROUPINGS

Based on survey responses, households will be placed into one of five groupings:

1. Peak Time Rebate: Flat Rate
2. Peak Time Rebate: PTR Rate
3. Critical Peak Pricing: Flat Rate
4. Critical Peak Pricing: CPP Rate
5. Control Group

Incentive ceilings, which are determined by points at the end of the season (1 point = $1), are as follows:

Summer
Initial Survey: 50 points
Dynamic Pricing Group: 0-150 points
Control Group: 50 points

Winter
Dynamic Pricing Group: 0-150 points
Control Group: 50 points
Final Survey: 30 points

If the customer does not earn points, or goes below zero points, they will not be required to pay anything as part of the study. They will still be responsible for their regular electric bill from LAC.

Please note that our partners in this study, who are objective third-party researchers, will randomly determine the groupings. Once a customer has been assigned, there is no changing your grouping.

SUMMARY

The Smart Grid demonstration study is made up of two components – the **survey** and the **pilot program**:

Customer Survey

- Each household North Mesa and Barranca Mesa will be asked to complete a simple online survey about the way they use electricity. It will take less than 15 minutes to fill out the survey – and those who do so will receive the $50 incentive.
- Answers to the surveys will be used only to understand the sample group's electricity usage and to create research groupings for the pilot program.
- All proprietary data will be kept strictly confidential, tied only to the household's unique meter number.
- If customers opt-in, the aggregate information (non-specific to individual customers) will be shared with LAC's valued partners in the Smart Grid demonstration, including the New Energy and Industrial Technology Organization (NEDO) Toshiba and Kyoto University.

Pilot Program

- Households within North Mesa and Barranca Mesa will also be asked to participate in a voluntarily pilot program.
- The program is completely separate from the household's electric bills.
- Participation is simple and uses interaction via email and SMS to alert customers to days (demand response events) when dynamic pricing programs are "virtually" in effect.
- Customers outside of the control group can earn points on these days by reducing energy usage during designated timeframes. The total amount of points at the end of the season will dictate the dollar incentive. (1 point = $1)
- The incentive ceiling will be determined by the household's assigned group, which is done randomly and cannot be changed.
- The Los Alamos County Department of Public Utilities is excited about this opportunity, which will supply valuable information to help manage the increased energy needs of homes and businesses in the coming decades, and create a secure energy future for us all.

Questions

[WEB COPY ONLY: For a list of FAQ's, please click here.]

To learn more, call XXX-XXX-XXXX or email dpu@lacnm.us