DEFAULT EFFECTS AND FOLLOW-ON BEHAVIOR: EVIDENCE FROM AN ELECTRICITY PRICING PROGRAM

Meredith Fowlie
Catherine Wolfram
C. Anna Spurlock
Annika Todd
Patrick Baylis
Peter Cappers*

April 25, 2018

We study default effects in the context of a residential electricity-pricing program. We analyze an implementation of a large-scale randomized controlled trial, in which one treatment group was given the option to opt-in to time-based pricing while another was defaulted into the program but allowed to opt-out. We provide dramatic evidence of a default effect. We also observe customers' electricity consumption in light of the pricing plan they face, which we describe as "follow-on" behavior. This provides insight on the mechanisms behind the default effect, particularly for "complacent" households (i.e., those who only enroll in time-varying pricing if assigned to the opt-out treatment).

*Fowlie: UC Berkeley and NBER, fowlie@berkeley.edu. Wolfram: UC Berkeley and NBER, cwolfram@berkeley.edu. Spurlock: Lawrence Berkeley National Laboratory, caspurlock@lbl.gov. Todd: Lawrence Berkeley National Laboratory, ATodd@lbl.gov. Baylis: Stanford University, baylis@stanford.edu. Cappers: Lawrence Berkeley National Laboratory, pacappers@lbl.gov. We received many helpful comments from seminar participants at Arizona State University, Cornell, Toulouse School of Economics, UC Berkeley and University of Oxford. The authors gratefully acknowledge contributions from and discussions with Hunt Allcott, Stefano Dellavigna, Steven George, Nick Kuminoff, Brigitte Madrian, Jennifer Potter, Lupe Strickland, Michael Sullivan and Nate Toyama. We also thank Severin Borenstein, Lucas Davis, and Michael Greenstone for helping to make this project possible through their initial involvement with the Smart Grid Investment Grant program. This material is based upon work supported by the Office of Electricity Delivery and Energy Reliability, of the U.S. Department of Energy under Contract No. DE-AC02-05CH11231. The views and opinions of authors expressed herein do not necessarily state or reflect those of the United States Government or any agency thereof. Corresponding author: Catherine Wolfram, Haas School of Business, UC Berkeley, Berkeley, CA 94720-1900, (510)-642-2588.

1. Introduction

When confronted by a choice with a default option, decision-makers are often predisposed to accept the default. Prior work in psychology and economics has documented this "default effect" for a range of decisions that would seem to merit deliberate choices, including retirement plans (Madrian and Shea, 2001), health insurance (Handel, 2013), and organ donations (Johnson and Goldstein, 2003). This phenomenon is of general interest because it provides businesses and public policymakers with a relatively easy and non-intrusive way to influence choices.

Although the effect of default options on decision-making has been clearly demonstrated in the literature, the broader economic implications of these default effects have been much harder to discern. One reason is that the economic impacts of a default effect can work through several channels. To comprehensively assess these impacts, one must consider not only the initial choice subject to the default manipulation, but also any "follow-on" behaviors that can depend on the initial choice. For example, consumers who are defaulted onto a health insurance plan with high co-pays may invest less in preventative health compared to those who actively chose such a plan. Employees defaulted into a high 401(k) savings rate may alter their retirement savings in other vehicles less than employees who make an active choice. Given that many default manipulations aim to induce changes in follow-on behavior, it is important to account for both direct and indirect impacts of default manipulations on economic outcomes.

This study analyzes the use of default provisions in a new choice setting: time-varying electricity pricing. This choice context is important because policy makers are looking to significantly increase electricity demand response to meet challenges associated with aging power sector infrastructure, increasing grid integration of renewables, and system reliability concerns. An important first step towards increased demand response is increased consumer acceptance of time-varying pricing programs. We leverage experimental variation in the pricing program default, together with detailed data on electricity consumption, to analyze how defaulting customers into time-varying electricity pricing affects both the initial program participation choice and follow-on behavior for different types of customers. In particular, we are able to isolate the follow-on behavior of those who actively opted in (referred to here as "always takers"), from those who only ended up on the new pricing structure because of the default (referred to here as "complacents").

A significant increase in customer participation in electricity demand response programs could generate substantive efficiency gains. Benefits include lower electricity system operating costs, lower renewable integration costs, and a more resilient electricity grid. Importantly, the scale of these benefits increase with the number of customers confronted by and responding to time-varying prices. However, customer participation in time-varying pricing programs has historically been very low. The vast majority (over 95 percent in 2012) of U.S. residential customers currently face time-invariant prices for electricity (FERC, 2014). Recent investments in smart grid infrastructure, including smart meters, make it technologically feasible to enroll many customers in time-varying pricing programs. As of 2017, more than 72 million smart meters had been deployed to over half of US households (Institute for Electric Innovation, 2017).[1] These investments notwithstanding, proactive approaches to increasing active participation in these programs will be required to fully leverage demand response potential.

This paper explores an innovative approach to increasing participation - and demand response - in a residential time-varying electricity pricing program. The analysis is based on a field experiment run by the Sacramento Municipal Utility District (SMUD) in 2011-2013. In one set of treatment groups, customers were invited to opt-in to a new time-varying pricing structure. In another set of randomly selected groups, customers were informed that they would be defaulted onto the new pricing programs unless they opted out. We show that making time-varying pricing the default choice can significantly increase participation – over 90 percent of the customers stayed with time-varying pricing when defaulted onto it. In contrast, approximately 20 percent actively opted in.

The economic importance of this default effect will depend critically on whether the households susceptible to the default effect actively reduce their peak consumption in response to the time-varying electricity prices. If complacent customers do not adjust consumption in response to time-varying pricing, then there is little point in defaulting them into this pricing regime. We obtain very detailed measurements of electricity consumption in the periods prior to and following the experimental intervention. We show that complacent customers, who comprise more than 75 percent of the sample, do reduce consumption when prices increase during peak times. Although the average demand response among complacent customers is approximately half as large as the average response among customers who actively opted in, higher participation rates in the opt-out group mean that the average effect of the opt-out offer on peak demand is significantly larger than the average effect of the opt-in offer.

These findings notwithstanding, policy makers may be reluctant to authorize the use of default provisions until they understand the consumer welfare implications. For example, if the default effect is driven by high switching costs, customers could be considerably worse off under a new pricing

[1] The deployment of smart grid technology was dramatically accelerated under the American Recovery and Reinvestment Act of 2009.

plan. Alternatively, suppose that switching inattentive or uninformed customers into an unfamiliar pricing regime encourages customers to learn about a new experience and "construct" their preferences (e.g., Hoeffler and Ariely, 1999; Barkan and Busemeyer, 2003; Simon et al., 2008). If preferences over alternative electricity pricing regimes are constructed or poorly understood, customers may in fact be better off in the pricing regime they would not proactively choose.

Given that there are several candidate models to explain the default effect, we opt not to perform a full welfare analysis under all possible rationalizations. Instead, we assess the extent to which alternative explanations for the default effect are consistent (or not) with observed patterns of behavior. While not dispositive, the evidence appears to reject a neoclassical switching cost model and instead points to customers with limited attention (either through rational inattention or lack of awareness) and non-standard choice heuristics.

The paper proceeds as follows. Section 2 situates our paper relative to the existing work on the default effect. Section 3 describes the experiment. Section 4 describes the data and our empirical approach. Section 5 presents our main results on the default effect and follow-on behavior. Section 6 describes the net benefits of the time-varying pricing programs from the utility's perspective. In Section 7, we present several pieces of evidence on the underlying factors behind the default effect to shed some light on its likely impacts on customers. Section 8 concludes.

2. Default Effects, Choice Modification, and Follow-on Behavior

A rich literature documents and explores various aspects of default effects in a range of settings, including 401(k) participation (Madrian and Shea, 2001; Choi et al., 2002, 2004), organ donation (Johnson and Goldstein, 2003; Abadie and Gay, 2006), car insurance (Johnson et al., 1993), car purchase options (Park et al., 2000), and email marketing (Johnson et al., 2002). This literature offers a range of possible explanations for default effects. In instances where the choice is relatively simple and not particularly important, such as agreeing to receive marketing emails, default effects may stem from rational inattention (Bellman et al., 2001; Sims, 2005). When confronting a decision that is more complicated or stressful, such as choices about health care or personal finance, choosing not to choose (and thus accepting the default) can allow the decision-maker to avoid incurring the costs of gathering information or evaluating difficult tradeoffs (Kressel and Chapman, 2007; Pichert and Katsikopoulos, 2007). If the consumer has limited personal experience with the choice context, the default option can be appealing, particularly if it is perceived to be the prescribed or recommended option (Beshears et al., 2009).

We aim to extend the literature on default effects in several ways. First, we highlight the importance of follow-on behavior. In many of the contexts where default provisions are used to influence choice outcomes, follow-on behavior plays a critical role in determining economic impacts. We make a distinction between two types of follow-on behavior. First, individuals may choose to subsequently modify the option they chose by default. For example, a consumer who accepts a particular 401(k) plan as a default option might subsequently adjust the parameters of this choice by changing the savings rate, changing the asset allocation, or dropping off the plan altogether. Second, there may be important choices or actions that are contingent on - but distinct from - the initial choice. Building on the retirement savings plan example, participating in a 401(k) plan could impact savings via other vehicles.[2]

To date, the literature on default effects has emphasized the initial choice and placed less emphasis on the implications for subsequent decisions. In particular, we are not aware of studies that consider the contingent behaviors that can be indirectly influenced by default effects. Analyses of 401(k) investment decisions have considered the first type of follow-on behavior – modifications to the original choice. For example, Carroll et al. (2009) analyze savings outcomes over time as a function of different default options at the initial plan participation decision. Other work includes information about follow-on choices, but does not model the impact of the default setting on those choices. For example, Ketcham et al. (2016) include information about Medicaid recipients' prescription drug spending in their welfare calculation, but do not model how plan choice impacts drug expenditures. Our study provides an unusual opportunity to analyze not only the direct effect of a default manipulation on an initial choice, but also the ways in which the default effect operates through the initial choice to affect subsequent consumer decisions.

Our empirical results also shed light on the underlying mechanisms that can give rise to default effects in this setting, and the associated welfare implications. Ultimately, the consumer-level impacts will depend on whether the default choice is well-suited to those who are susceptible to default effects. Recent papers have investigated the welfare effects of nudges in a variety of settings, including retirement savings plan default provisions (Carroll et al., 2009; Bernheim et al., 2015), health insurance plan choices (Handel, 2013; Handel and Kolstad, 2015; Ketcham et al., 2016), and home energy conservation reports (Allcott and Kessler, 2015). These papers augment the more standard utility maximization framework to accommodate features of consumer behavior (such as inattention) that could rational-

[2]Similarly, in a health insurance context, the relevant follow-on behavior could include subsequent choices about whether or not to go to the doctor, lifestyle choices that can affect health outcomes, or choice of medical procedures. In a social media context, default privacy settings could shape subsequent choices about posting personal photos or information.

ize a default effect (or, in the case of Bernheim et al., 2015, they mediate between several different explanations for the default effect).

We consider several alternative explanations for the default effect and assess which seems most consistent with our data. The most straightforward explanations are predicated on the assumption that consumers' preferences are pre-determined and their choices are utility-maximizing and well informed. Under these standard assumptions, a default effect can manifest if agents incur a cost to switch from the default choice, or if consumers are inattentive to unfamiliar choices. Alternative models, such as those introduced by Bernheim et al. (2015), assume that the default provision affects not only the level of effort required to select a given choice, but also the frame through which the choice is viewed and the process by which the agent constructs her preferences. If the utility-maximizing choice is frame dependent, welfare analysis becomes more complicated.

We evaluate alternative explanations of the default effect using not only observed participation decisions, but also rich data on subsequent electricity consumption patterns as well as survey responses describing consumer experiences. We find that observed consumer behavior is consistent with explanations under which consumers are not paying attention to the initial choice, but come to understand it and like it. One implication is that standard welfare analysis predicated on the assumption of known preferences and informed choices can generate misleading estimates of welfare impacts.

3. Empirical Setting and Experimental Design

Economists have noted for some time that efficient pricing of electricity should reflect changing electricity market conditions (e.g., Boiteux, 1964a,b). Electricity demand, marginal system operating costs, and firms' abilities to exercise market power vary significantly and systematically over hours of the day and seasons of the year. Figure 1 demonstrates the extent of this variation for a week during our study. The red line depicts hourly electricity demand, which cycles predictably over the course of a day, varying by a factor of 1.5 to almost 3 from the middle of the night to the peak hours in the late afternoon. The blue line depicts hourly wholesale prices, which fall below \$60/MWh in most hours, but spike to over \$1,000/MWh at critical peak times.

[FIGURE 1 HERE]

Although wholesale electricity prices can vary significantly across hours, at least partially reflecting variations in marginal costs, retail prices do not generally reflect these dynamic market conditions. The vast majority (over 95 percent in 2012) of U.S. residential customers pay time-invariant prices for electricity (FERC, 2014). If customers are not exposed to prices that reflect variable marginal operating costs, economic theory suggests that consumers will under-consume in periods of low marginal costs and over-consume in periods of high marginal costs. This further implies over-investment in capacity to meet excessive peak demand. For example, Borenstein and Holland (2005) simulate that by shifting a fraction of customers to time-based rates, utilities could construct 44 percent fewer peaking plants.

In principle, these inefficiencies can be mitigated - or eliminated - with the introduction of time-varying retail electricity pricing. Residential customers have an important role to play in electricity demand response, particularly in areas of the country where peak residential demand (driven by air conditioning in many parts of the U.S.) coincides with the system peak. When residential customers have been exposed to time-based prices, prior analyses suggest they are willing and able to adjust consumption in response (see, for example, EPRI, 2012).[3]

To reap benefits from time-varying pricing, though, utilities need to enroll more customers in time-varying pricing programs. In what follows, we describe a large-scale field experiment designed to evaluate a novel approach to increasing participation among residential electricity customers.[4]

3.1. The Experiment

The experiment was implemented as part of the Smart Grid Investment Grant (SGIG) program, which received $3.4 billion in funds from the American Recovery and Reinvestment Act of 2009. The goal of this program was to invest in the expansion of the smart grid in the U.S., and thereby create jobs and accelerate the modernization of the nation's electric system (Department of Energy, 2012). One of the objectives articulated in the Funding Opportunity Announcement (DE-FOA-0000058) under the heading of Consumer Behavior Studies (CBS) was to document the impacts and benefits of time-based rate programs and associated enabling control and *information* technologies. To be eligible for funding, the use of randomized controlled experimental designs for evaluating these impacts and benefits was

[3] In a 2012 meta-analysis, authors identified what they deemed to be the best seven U.S. residential pricing studies up to that time (EPRI, 2012). These studies document peak demand response to time-varying pricing in the range of 13-33%, depending on the existence of automated control technology (e.g., programmable communicating thermostat). These estimates imply an elasticity of substitution in the range of 0.07 - 0.24 and an own-price elasticity in the range of -0.07 - -0.3. Note that the experimental nature of our study allows us to assess many dimensions of customers' responses to time-varying pricing, including spillovers within and across days. Some previous evaluations of time-varying pricing have relied on within-customers comparisons, which assume there are no spillovers of this sort.

[4] A much smaller-scale experiment was conducted in Los Alamos. Results of this study are summarized in a recent working paper (Wang and Ida (2017)). Residential customers were recruited to participate in a demand response experiment. Of these, 365 were given the option to opt-in to a time-varying rate and 183 customers were defaulted onto the new rate. Whereas opt-in rates typically fall within the range of 2-10%, 64% of customers opted into the time varying rate. Presumably, this is because the study sample is comprised of only those customers who actively select into a field experiment. Interpretation of the estimated demand response is further complicated by the fact that program participants were insured against losses (i.e., they could only gain from participating in the experiment).

required.

The Sacramento Municipal Utility District (SMUD), a municipal utility that serves approximately 530,000 residential households in and around Sacramento, California, implemented one of the 11 consumer behavior studies that were funded under the SGIG program.[5] They were awarded a $127 million grant overall, which comprised part of a $308 million smart grid project. SMUD viewed the opportunity to study the impact of time-varying rates within their own service territory as a major benefit to participating in the program (Jimenez et al., 2013). SMUD had some demand response programs in place prior to the SGIG program (e.g., an air conditioner direct control program and some rates that varied by time-of-use), but these programs had not been broadly emphasized or marketed for a long time. Historic adoption of their "legacy" Time-of-Use (TOU) rates had been extremely low. From SMUD's perspective, the SGIG program was an opportunity to maximize the benefits of their smart-grid technology investments, and to test time-varying rates that were designed to meet their evolving load management needs (Jimenez et al. 2013).

The study sample was drawn from SMUD's population of residential customers. To define the experimental population, several selection criteria were applied. Households were excluded: if their smart meter had not provided a year's worth of data by June 2012; if they were participating in SMUD's Air Conditioning Load Management program, Summer Solutions study, PV solar programs, budget billing programs, or medical assistance programs; or if they had master-metered accounts. After these exclusions, approximately 174,000 households remained eligible for the experimental population.[6]

Households in the experimental population were randomly assigned to one of ten groups, five of which are the focus of this paper.[7] Households in four of these five groups were encouraged to participate in a new pricing program; the fifth group received no encouragement and serves as the control group. There were two pricing treatments: a Time-of-Use (TOU) and a Critical Peak Pricing (CPP) program. There were also two forms of encouragement: opt-in, where households were encouraged to enroll in the rate program; and opt-out, where households were notified that they were enrolled by default, but had the opportunity to leave the program if they wished. All encouraged households (opt-

[5] The other ten studies are described in Cappers and Sheer (2016). Most evaluated other aspects of time-varying pricing, such as the impact of providing customers with "shadow" bills, which documented how much they would have paid under standard pricing. Only one of the other studies compared opt-in and opt-out recruitment approaches (Lakeland Electric) but the data the utility provided did not contain enough detail to perform a comparable analysis.

[6] SMUD reports no statistically significant differences between the households in the study sample and the larger residential customer base. We did not have access to these sample comparisons, and we do not know which variables were analyzed. Most residential customers had smart meters in time for the experiment, though many were excluded because many meters had not reported a full year of data by June 2012.

[7] The other five groups were: defaulted to another time-varying rate that did not have a corresponding opt-in group treatment (i.e., Critical Peak Pricing (CPP) plus TOU rate); encouraged to opt in to CPP or TOU without the enabling technology described below; or were part of a recruit and deny randomized controlled trial for TOU rates.

in and opt-out) were also offered enabling technology – an in-home display that provided real-time information on consumption and the current price.

Figure 2 summarizes the standard, TOU, and CPP rate structures that are evaluated in this study. All SMUD customers faced an increasing block pricing structure. This means that the price paid for the first block or "tier" of electricity consumed during a billing period was lower than the price paid for the higher tier. During the time period of our study, customers on the standard rate plan (i.e., customers in the control group) paid a $10 monthly fixed charge plus $0.0938 per kWh for the first 700 kWh of consumption and $0.1765 per kWh for consumption above 700 kWh. Under the TOU program, customers faced the same monthly fixed charge of $10. These customers paid a higher rate, $0.2700 per kWh, for electricity consumed during the "peak period" from 4PM to 7PM on non-holiday weekdays. They paid a lower rate (relative to the standard rate structure), in all other "off-peak" hours, $0.0846 per kWh for the first 700 kWh and $0.1660 for consumption above 700 kWh. (On-peak consumption did not count towards the 700 kWh total.) Customers on the CPP plan paid a significantly higher rate, $0.7500 per kWh, for consumption between 4PM and 7PM on twelve "event days" over the course of the summer. Customers were alerted about event days at least one day in advance. Consumption outside of the CPP event window was charged at a rate of $0.0851 per kWh up to 700 kWh and $0.1665 per kWh beyond.

[FIGURE 2 HERE]

Both the CPP and TOU rates were only in effect between June 1 and September 30 for the two summers in the study (2012 and 2013). Low-income customers enrolled in the Energy Assistance Program Rate (EAPR) were eligible to participate in the study. No matter the pricing plan, EAPR customers received about a 30 percent discount on their rates. Both the TOU and CPP rates were designed to be approximately revenue neutral to the utility if customers selected their rate plan randomly and did not adjust their consumption (see Jimenez et al. 2013).

To summarize, the five randomized groups we study include: the CPP opt-in group, which was encouraged to enroll in the CPP program; the CPP opt-out group, which was notified of enrollment and encouraged to stay in the CPP program; the TOU opt-in group, which was encouraged to enroll in TOU program; the TOU opt-out group, which was notified of enrollment and encouraged to stay in TOU program; and the control group, which was not encouraged to participate in a rate program and remained on SMUD's standard rates.

3.2. Encouragement Messages

Customers assigned to the CPP or TOU treatment arms were encouraged to enroll in time-varying pricing. Materials and messaging were virtually identical across the opt-in and opt-out groups. The encouragement effort for opt-in households consisted of two separate mailed packets. The first was sent in either October 2011, to about 20 percent of the encouraged households, or November 2011, to the remaining 80 percent. The second was sent in January 2012. Each packet included a a letter, a brochure, and a postage-paid business reply card that the household could mail back to SMUD indicating their choice to either join the program or not. The recruitment materials listed generic benefits of participating in rate programs, including saving money, taking control, and helping the environment. In March of 2012, door hangers were placed on the doorknobs of encouraged households. Finally, an extensive phone bank campaign was carried out throughout April and May of 2012, with calls going out almost daily.

Recruitment activities and program enrollment are summarized in Figure 3. About half of the customers enrolled following the packet and door hanger recruitment phase, while the second half were successfully enrolled over the timeframe of the phone campaign (though about 22 percent of these still indicated their desire to enroll by way of the business reply cards).

[FIGURE 3 HERE]

The opt-out groups were mailed one packet containing a letter, brochure, and business reply card. These materials were designed to look as similar as possible to the materials received by members of the opt-in groups. Packet mailings were followed within two weeks by a reminder post card. About 10 percent of the packets were sent on March 12, 2012 and the remaining 90 percent were sent on April 5, 2012.

The TOU opt-in group received slightly different encouragement messages from the other groups because they were part of a recruit-and-delay randomized controlled trial (which we are not incorporating into this study). In the first packet mailed in late 2011, the households were given the same information as other groups regarding the starting date of the pricing experiment. However, in the packet mailed in January 2012, there was text that informed them that if they decided to opt-in to the rate program, they would be randomly assigned to a start date of either 2012 or 2014. The other three groups were told that their participation date would start in 2012 if they decided to opt-in or not opt-out throughout all communications they received. This means that the set of always takers in the CPP opt-in group could be somewhat different from the always-takers in the TOU group, as the TOU always takers had to be willing to accept some probability that their enrollment would be delayed. Thus, while the CPP opt-in group can be directly compared to the CPP opt-out group, comparisons between the TOU opt-out and opt-in groups are drawn with the caveat that these two groups were encouraged and recruited somewhat differently.

4. Data and Methodology

4.1. Data Description

The data we use in our analysis are comprised of household-specific data, electricity consumption data, and weather data. The household-specific data includes experimental cell assignment, dates of enrollment, disenrollment, and account closure information for households who moved. In addition, we observe whether households were on SMUD's EAPR program for low-income customers, as well as whether or not they had set up a "My Account" online to interface with their SMUD account, and the number of times they had signed in to their "My Account" page. Finally, for some households, we have responses to two large-scale surveys administered to customers on the new rate programs as well as a sample of control households, including a demographic survey and a customer satisfaction survey.

We also have data on households' energy consumption, as well as their associated expenditures. Specifically, we have data on hourly energy consumption for each household starting on June 1, 2011 and continuing through October 31, 2013, the end of the pilot period. Electricity consumption is measured in kilowatt hours (kWh). We collect energy consumption data for all households in the experimental sample, including the control group, for the duration of the study period. Households that moved are one exception. These households were not tracked to their new location, so data for these households ends when they moved from their initial location.

In addition to the hourly energy consumption data, billing data were also obtained for all households in the experiment. These data include the total energy (kWh) charged in each bill, as well as the total dollar amount of the bill. Hourly energy consumption and billing data are quite complete. Less than one percent of these data are missing. The frequency of missing data does not differ systematically across treatment groups, nor across households who did or did not opt in or opt out of treatment.

The final type of data we use are hourly weather data, including dry and wet bulb temperature as well as humidity. There is only one weather station in close proximity to all participants in the SMUD service area, so the weather data does not vary across households, only over time.

4.2. Validation of Randomization

Table 1 provides summary statistics by experimental group. The top three rows summarize information on daily consumption, the ratio of peak to off-peak energy consumption and billing from the pre-treatment summer (June to September 2011). SMUD households consume slightly less electricity than the average U.S. household – approximately 27 kWh per day during the four summer months compared to almost 31 kWh per day across the U.S. in 2011. The ratio of peak to off-peak usage provides one indication of a customer's exposure to the higher peak prices under CPP or TOU, and bill amounts reflect the average monthly bill in the pre-treatment summer. Bills in our sample are very close to the national average, reflecting that SMUD customers pay higher prices than the average U.S. residential customer. For all three variables, we also report t-statistics on the test that the mean for each treatment group equals the mean for the control group. The t-statistic exceeds one for only one of these comparisons, suggesting that the randomization yielded groups with very similar means across these three variables.[8]

The next two variables measure the share of households that would pay less on either the CPP or TOU pricing policy, assuming no change in their consumption. (Following industry convention, we refer to households who would pay less as "structural winners.") Approximately half of all customers are estimated to be structural winners, based on consumption data collected before the intervention. The bottom four rows summarize household-level covariates that we observe for every household in the experiment. "My Account" is a dummy variable indicating whether or not the household had signed up to use SMUD's online portal prior to our experiment. For those customers who have enrolled in the online portal, "My Account logins" summarizes the number of log-ins. "Paperless" is a dummy variable indicating whether or not the household had signed up to receive electronic bills. Finally, "Low income" is a dummy variable indicating enrollment in the low-income rate. Of the 24 t-statistics reported across these six variables, only one exceeds two, again confirming the integrity of the randomization process.

[TABLE 1 HERE]

[8] Given that we will be analyzing consumption across hours of the day, we are particularly concerned about balance in consumption profiles. In addition to the ratio of peak to off-peak usage, the appendix provides a breakdown of consumption across all 24 hours of the day (Figures A1, A2). Again, all four treatment groups look very similar to the control group.

4.3. Methodology

4.3.1. Estimating ITT for experimental treatment groups

We estimate a difference-in-differences (DID) specification using data from the pre-treatment and treatment periods to identify the average intent to treat (ITT) effect. Equation 1 serves as our baseline estimating equation, where y_{it} measures hourly electricity consumption for household i in hour t. These specifications are estimated separately for the opt-in and opt-out groups. Z_{it} is an indicator variable equal to one starting on June 1, 2012 if household i was encouraged to be in the treatment group, and zero otherwise. γ_i is a household fixed effect that captures systematic differences in consumption across households, and τ_t is an hour-of-sample fixed effect.

$$y_{it} = \alpha + \beta_{ITT} Z_{it} + \gamma_i + \tau_t + \varepsilon_{it} \tag{1}$$

We estimate four sets of regression equations. Each set uses data from the control group and one of the four treatment groups. The coefficient of interest is β_{ITT}, which captures the average difference in hourly electricity consumption across treated and control groups, controlling for any pre-treatment differences by group.[9] Within each set, we estimate the model separately using data from event day peak hours (4pm to 7pm on the twelve CPP days in each summer) and non-event day peak hours (4pm to 7pm on non-event, non-holiday weekdays during the summer).[10]

4.3.2. Estimating LATE for experimental treatment groups

We estimate a DID instrumental variables (IV) specification using data from the pre-treatment and treatment periods to identify a Local Average Treatment Effect (LATE). Specifically, we estimate equation 2, where y_{it}, γ_i, and τ_t are defined as in equation 1. $Treat_{it}$ is an indicator variable equal to one starting on June 1st, 2012 if household i was actually enrolled in treatment, zero otherwise (estimated separately for the opt-in and opt-out groups). We instrument for $Treat_{it}$ using the randomized encouragement to the corresponding treatment Z_{it}.

$$y_{it} = \alpha + \beta_{LATE} Treat_{it} + \gamma_i + \tau_t + \varepsilon_{it} \tag{2}$$

[9] We present specifications with the dependent variable measured in levels because the cost savings from time-varying pricing are a function of kWh reduced, not the percent reduction. Our results are not sensitive to alternative functional forms, and the appendix presents specifications in logs (Tables A1, A2, and A3).

[10] Note that customers under the TOU pricing plan face the same prices on event and non-event days. We estimate separate impacts for comparison to CPP.

The β_{LATE} coefficient captures the LATE. In this specification, the LATE measures the average reduction in household electricity consumption among customers enrolled in the time-varying pricing program. To interpret β_{LATE} as a causal effect, we must invoke an exclusion restriction, which requires that the encouragement (i.e., the offer to opt in or default assignment into treatment with the ability to opt out) affects electricity consumption only indirectly via an effect on participation. We also invoke a monotonicity assumption which requires that our encouragement weakly increases (versus reduces) the participation probability for all households. Appendix 3 discusses these identifying assumptions and summarizes some ancillary analysis of the exclusion restriction.[11]

4.3.3. Estimating LATE for Complacents

Conceptually, our sample of residential customers can be divided into three groups (see Figure 4). Never-takers are households who opt-out of an opt-out program and do not enroll in an opt-in program. Complacents are households who do not actively enroll in an opt-in program, but who also do not actively drop out of an opt-out program. Always-takers are households who actively enroll in an opt-in program and remain in an opt-out program. Note that a comparison of average electricity consumption across the opt-in and opt-out groups (the top two rows in Figure 4) estimates the average effect of being assigned to the opt-in versus opt-out groups. Scaling this difference by our estimate of the population share of complacents yields an unbiased estimate of the average effect of time-varying rates on electricity consumption among complacents.[12]

[FIGURE 4 HERE]

We estimate the DID IV specification using data from the opt-in and opt-out groups, as shown in equation 2, where all variables are defined as above, except now $Treat_{it}$ is instrumented for with an indicator variable equal to one for observations starting on June 1, 2012 if a household was encouraged into the opt-out treatment group only.

This IV specification provides an intuitive way to isolate the average causal effect of these pricing programs on electricity consumption among complacents. To interpret our estimates in this way, we again invoke the exclusion restriction which requires that the encouragement (the offer to opt-in or the default assignment with the ability to opt-out) does not directly affect electricity consumption among always takers, never takers, or complacents. As Figure 4 makes clear, we are also assuming that

[11] Ancillary analysis which assesses the plausibility of this exclusion restriction assumption is included in the appendix.

[12] Our approach to isolating the response of the complacents is very similar to Kowalski (2016), although our setting is considerably more straightforward since we randomized the selection of both the opt-in and the opt-out treatments.

always takers who actively enroll in the pricing programs under the opt-in treatment do not behave differently than always takers who are defaulted onto the programs through the opt-out treatment. The Appendix contains a detailed discussion of this assumption. We note that to the extent actively enrolling leads customers to consume less (i.e., respond more to the time-varying prices), our estimates of the complacents' reductions will be understated.

5. Main Results

5.1. Default Effects on Program Adoption

Table 2 summarizes customer acceptance of time-varying pricing in the opt-in and opt-out groups, respectively. The columns titled "Initial" summarize customer participation at the beginning of June 2012 (the month the new rates went into effect). The columns titled "Endline" summarize participation at the end of the second summer (September 2013). In both sets of results, the first column reflects the share of customers on the time-varying rate while the second column reports the number of customers on the rate.

[TABLE 2 HERE]

The initial participation results provide striking evidence of the default effect. For both the CPP and TOU rates, approximately 20 percent of those assigned to the opt-in encouragement elected to opt-in. Fewer than 5 percent opted out when defaulted onto the new rate structure, leaving over 95 percent of the customers on the new rates in the default treatment.[13]

To interpret the "Endline" columns, it is important to understand how we are describing the eligible population. If customers moved, they were no longer eligible for the time-based rates, even if they moved within SMUD's service territory. Also, new occupants were not included in the pilot program. The numbers in Table 2 report rates and enrollees after dropping movers. For instance, the number of customers on CPP from the opt-in group fell from 1568 to 1169 because 399 households (approximately 25 percent) moved between June 2012 and September 2013. SMUD reports move rates of approximately 20 percent per year across their entire residential population, so a move rate of 25 percent over a 16-month period that includes the summer, when moves are most likely, is reasonable. Across the four

[13] It is worth noting that SMUD was more successful than expected at recruiting customers onto time-varying rates. The company's expectations, and the basis for our ex ante statistical power calculations, were that between ten and fifteen percent of customers would opt-in. On the other hand, given that SMUD customers are generally satisfied with the utility and trust its recommendations, they may have been more likely to accept the default. SMUD anticipated that approximately 50 percent of the customers would remain on the rate with opt-out.

columns, the move rates are very similar, ranging from 23 percent in the CPP opt-out group to 26 percent in the TOU opt-in.[14]

5.2. Choice Modification

We observe modifications to consumers' participation choices after the program started, although program rules constrained the set of possible changes. Customers in the opt-in group were not allowed to enroll after June 1, 2012; customers in the opt-out group who had already opted-out were not allowed to change their minds and enroll. However, customers in both groups who had initially chosen to participate in the time-varying rate program could revert to the standard rate at any time.

The final column of Table 2 reports the difference between initial and endline participation rates, divided by the initial participation rate. Participation in both of the opt-in groups fell by fewer than 1.5 percentage points, reflecting fewer than 10 percent of the original participants. Participation in both of the opt-out groups fell by more percentage points (6.6 in the case of CPP opt out, 96.0 − 89.4, and 5.3 in the case of TOU opt out), but again reflected fewer than 10 percent of the original participants.

With such a small share of households dropping out of these programs, tests comparing attrition rates across the opt-in and opt-out groups are low powered. The appendix reports results from a hazard analysis of drop outs. Several interesting patterns emerge. First, although the rates of attrition over the entire study were similar, the opt-in participants (both TOU and CPP) dropped out sooner than opt-out. For households in the opt-out groups, the reminder sent to participants before the second summer had a statistically significant effect on drop-outs.

In sum, sections 5.1 and 5.2 provide strong evidence of a default effect and relatively little evidence of subsequent re-optimization.

5.3. Follow-on Behavior

5.3.1. ITT Effect Estimates

Table 3 summarizes the estimation results for the DID specification of equation 1 that uses data from the pre-treatment and treatment periods to identify an ITT effect. The first two columns use data from peak hours on "critical event" days. In the post-treatment period, these correspond to days when a CPP event was called. In the pre-treatment period, these correspond to the hottest non-holiday weekdays during the summer of 2011.[15] The right two columns use data from all other summer weekdays. In all

[14] Moving rates are not statistically significantly different from one another (z-statistic on the largest difference equals 1.3).

[15] We have also estimated specifications based on random samples of 12 days within the hottest 24 days. Our results are not sensitive to this choice.

cases the analysis is limited to the peak periods of the relevant days (4PM to 7PM).

[TABLE 3 HERE]

If we interpret the coefficients in Table 3 as estimates of the causal impact of encouragement to join the time-varying rates, we conclude that providing households the opportunity to opt-in to the CPP treatment leads to an average reduction in electricity consumption of 0.130 kWh during peak hours of event days (averaged across all household that received the opt-in offer). The estimate for the opt-out group is considerably larger at 0.299 kWh across all households defaulted onto the CPP rate.

The coefficients in the last two columns show that CPP customers *reduced* their consumption during peak hours on non-event days (by 0.028 kWh per household in the opt-in group and 0.095 kWh per household in the opt-out group). Recall that CPP customers faced rates that are slightly lower than the standard rates on these non-event days. These kWh reductions are considerably smaller compared to event days for the CPP households, but still statistically significant. Why might consumers respond to a decrease in electricity price with a decrease in consumption? This is consistent with habit formation, learned preferences, (e.g., if households learn that they can comfortably open windows instead of turning on the air conditioning), or a fixed adjustment cost (e.g., if customers set programmable thermostats to run air conditioning less between 4 and 7 PM on all days, even when they only face higher prices on a subset of those days).

In the case of the TOU group, who faced higher prices during peak hours for all weekdays (not just event days,) the results show that households reduced their daily peak consumption by 0.090 kWh on average in the opt-in treatment, and 0.129 kWh on average in the opt-out treatment on days that were called as event days for CPP customers (i.e., relatively hotter days). On all other peak days average reductions are estimated to be 0.055 kWh per household in the opt-in treatment, and 0.100 kWh per hour in the opt-out treatment. Given that non-event-day consumption is lower, the results are approximately the same in percentage terms (3.6-5.1% for the opt-in group and 5.9 - 7.2% for the opt-out group – see Appendix 4).

The exclusion restriction implies that always takers in the opt-out group are responding to the time-varying rates in the same way as their counterparts in the opt-in group. Under this assumption, differences in these estimated ITT effects across the opt-in and opt-out groups are driven by a demand response among complacents. We have also estimated the opt-in and opt-out equations jointly so that we could test equality of the coefficients. We can reject equality with at least 95% certainty in all cases except for event day TOU, where p = 0.055.

Finally, we regenerate the results reported in Table 3 using only the post-intervention data. In other words, we do not use the pre-period data, and we simply compare treated households' consumption to the control households' during event and non-event peak hours. This exercise yield qualitatively similar results, which are summarized in Appendix Table A4. The average reductions for the opt-out group are nearly 3 times larger than the average reductions for the opt-in group for CPP and 2 times larger for TOU. The coefficient estimates do differ slightly from those reported in Table 3 since there were some pre-period differences by group, even if those differences are not statistically significant.

5.3.2. LATE Estimates

Table 4 reports on the instrumental variables specifications (equation 2). Similar to Table 3, the columns on the left of the table report estimates using data from CPP event hours and the columns on the right report results estimated using data from non-event-day peak hours. The top of the table corresponds to CPP customers while the bottom corresponds to customers participating in TOU programs.

LATE estimates in the first two columns suggest that the always-takers in the opt-in CPP group reduced consumption during event-day peaks by almost twice as much as the larger group of always takers plus complacents participating in the CPP program in the opt-out group (0.664 compared to 0.323 kWh per household). The magnitude of the reduction for the opt-in group (664 watts per hour) is large and suggests consumers did more than simply turn off a few light bulbs. Given that electricity rates increased by almost 100 percent during critical peak events, this reduction off a mean of almost 2,500 watts is consistent with a price elasticity of approximately -0.25. This is on the high side of other short-run demand elasticities estimated for electricity consumption, though typically those estimates are based on demand reductions over longer time periods (EPRI, 2012). In the fourth and fifth columns, we see again that households in both the opt-in and opt-out CPP treatments significantly reduced their consumption on non-event peak days. Complacents' average reductions on non-event days comprise a larger share of the average critical peak reductions than is true for always takers. This is consistent with the latter group fine-tuning their demand to changing conditions, whereas complacents may rely to a larger extent on modifications that do not require sustained attention (such as reprogramming a thermostat to reduce cooling load during peak hours on all days).

In the case of the TOU treatments, the LATE estimates indicate that always-takers reduced consumption during daily peaks that were called as event days for the CPP treatment by about three times as much as the combination of always-takers plus complacents in the TOU opt-out group (0.473 relative to 0.136 kWh per household), and almost three times as much (0.288 relative to 0.105 kWh per household) during non-event regular peak days.[16]

[TABLE 4 HERE]

The results in the third and sixth column isolate the effect of time-varying rates on electricity consumption among the complacent households. Comparing the results in the first column (always-takers), to the results in the third column (complacents), suggests that the average response among always takers to the CPP rate was about 2.5 times larger than the response among complacents during event hours. Complacents were somewhat more similar to always takers during non-event peak hours, reducing by only half as much.[17] Differences between always takers and complacents are more pronounced with the TOU rates. Given that there are so many more complacents exposed to the rates under an opt-out experimental design, the aggregate savings from an opt-out design is significantly higher than from an opt-in design (as is made evident in Table 3).

Tables 3 and 4 have averaged treatment effects across all peak hours. Figure 5 illustrates these effects graphically, disaggregating by hour. The figure depicts hour-by-hour LATE estimates for event days across the four treatment groups relative to the control group. We also test for changes in consumption during non-peak hours. One might expect that some consumers would increase consumption in the hours leading up to the peak period (cooling the house when prices are relatively low, for example). However, we find that consumers are reducing consumption in the hours before the peak period, statistically significantly so for the always takers in both the CPP and TOU groups.

[FIGURE 5 HERE]

6. Cost-Effectiveness and System-Wide Impacts

This section summarizes the impact of the pricing plans on customer bills and utility revenues. To inform this analysis, we estimate an alternative form of equation 2 using customer-by-month observations and total bill amount as the dependent variable. Table 5 summarizes the estimation results. The coefficient estimate in the first column of the top panel suggests that bills for customers who opted in to the CPP rate plan fell by approximately 5% on average, with a mean reduction of $6.52 on an average summer bill of nearly $115. Bills for the typical participant in the opt-out group fell by less

[16] In joint specifications, we can reject that the coefficient estimates are equal across the opt-in and opt-out groups in all cases except for the CPP treatment on non-event days (p=0.249).

[17] Note that the coefficient estimates for the opt-out group in Table 4 are equal to the weighted sum of the coefficients for the always takers (e.g., -0.664 for CPP event hours) and the complacents (-0.233), with weights set equal to the share of always takers relative to total opt-out enrollees and one minus this number from Table 2.

– around \$4.50 for the group overall and slightly less for the complacents. This is consistent with the results presented in Table 4, which shows how complacent households reduced consumption by less during critical peak periods.[18]

[TABLE 5 HERE]

Table 6 analyzes the pricing programs from the perspective of the utility, comparing the costs of enrolling participants and implementing the program against the benefits (i.e., costs avoided when peak consumption is reduced). The analysis in Table 6 assumes each pricing program was scaled to SMUD's entire residential customer base and run for 10 years. Some of these program benefits and costs are summarized in Potter et al. (2014), a consulting report prepared to help SMUD decide whether to expand the pilot. We also obtained details not included in the report from personal communications with SMUD and their consultants. Appendix section 5 summarizes underlying assumptions, and explains why some of the assumptions pertaining to program benefits are likely conservative.

The two columns on the left summarize the two main benefits of the program. Reduced demand during CPP and TOU peak hours avoids two types of expenses – the costs incurred to supply sufficient electricity to meet peak demand during these hours, and the expected cost of new investments in peaking plants needed to meet demand in peak hours. To estimate the avoided capacity costs, the expectation is taken over the probability that demand in CPP or TOU hours would drive capacity expansion decisions. Notably, the avoided energy costs are considerably smaller than the avoided capacity costs, particularly for the CPP programs. This reflects the fact that electricity demand in a small number of peak hours drives costly generating capacity expansions. Given that electricity is not storable, current electricity systems include peaking plants that only operate several hours a year. Reducing demand in peak periods avoids the need to construct and maintain these plants going forward.[19]

We break the program costs into three components: (1) one-time fixed costs, which include items such as IT costs to adjust the billing system and initial program design costs, (2) one-time per-household costs which primarily include the customer acquisition costs, including the in-home devices offered to customers as part of the recruitment, and (3) recurring annual fixed and variable costs, which include

[18] Bill reductions should not be interpreted as a measure of consumer welfare impacts; customers may have made adjustments that were costly from a monetary or welfare perspective. We return to this point below.

[19] As we explain in the appendix, the calculations reflected in Table 6 may understate the capacity benefits, for example because they do not measure reductions in transmission and distribution level investments. Because the numbers in Table 6 reflect private benefits to the utility, they do not incorporate the value of avoided pollution. Given that the avoided energy savings are low relative to the avoided capacity, we suspect that avoided pollution would not change the overall cost-benefit calculus by much.

personnel costs required to administer the program. The one-time variable cost of recruiting customers is lower under the opt-out programs than under the opt-in. As we note in section 3, more effort was invested in recruiting customers assigned to the opt-in group.

Net benefits are reported in the final column of Table 6. We estimate that both opt-out programs would be cost-effective. The CPP opt-in program is estimated to be marginally cost-effective. The TOU opt-in program, which led to much smaller demand reductions than the CPP program, is projected to incur costs in excess of savings.

[TABLE 6 HERE]

7. Explanations for the Default Effect

In addition to assessing the program outcomes from the perspective of the utility, we are also interested in understanding *why* customers are predisposed to choose the default option. Some explanations for default effects presume known preferences and well-informed choices. Under alternative explanations, defaulting inattentive or uninformed customers to a new pricing regime encourages customers to learn about a new experience and "construct" their preferences. These different rationalizations of a default effect can have very different implications for consumer welfare.

Prior studies have identified several potential explanations for default effects but have made little progress identifying precisely which mechanisms are at work in a given setting. We are uniquely positioned to investigate alternative explanations for the default effect as we have detailed information about the determinants of the initial choice, together with rich data on follow-on behavior, as well as responses to survey questions on attitudes towards time-varying pricing.

7.1. Standard Economic Model

As a starting point, we begin with a standard economic model that assumes consumers make informed decisions based on known preferences. Within this framework, costs incurred to switch away from the default choice can give rise to a default effect. In what follows, we use this model to generate qualitative predictions that can be evaluated empirically. We also show how, under some reasonable assumptions, we can estimate a lower bound on consumers' willingness to pay for the new pricing program.

Suppose that consumers choose the electricity price structure P to maximize utility subject to a budget constraint:

$$\underset{P}{\text{maximize}} : u(e\,(P)) + V(P, 1, Y) \tag{3}$$

The first term captures disutility from effort, which is only expended if the customer actively switches to a different pricing plan. The second term captures the indirect utility from future consumption, which is a function of electricity prices, the price of all other goods (normalized to 1), and income Y. For notational ease, we will refer to this indirect utility component as $V(P)$.

Let $\bar{P}$ denote the vector of electricity prices under the uniform price regime and let $\tilde{P}$ denote the vector of time-varying prices. Let d denote the default choice. This model can generate a default effect if switching away from the default option incurs some cost (i.e., $u(e(P)) < 0$). To see this, note that if $d = \overline{P}$, the consumer opts out if $V(\overline{P}) < V(\widetilde{P}) + u(e)$. In contrast, if $d = \widetilde{P}$, the consumer opts out if $V(\overline{P}) + u(e) > V(\widetilde{P})$.

Figure 6 illustrates a stylized application of this modeling framework. In both panels, the vertical axis measures the difference in indirect utility: $V(\tilde{P})-V(\bar{P})$. The figures plot a hypothetical distribution of these indirect utility differences. For expositional ease, switching costs $u(e)$ are assumed to be constant across all customers and independent of $V(\tilde{P}) - V(\bar{P})$.

Within this stylized framework, the top panel illustrates participation choices if the default is $d = \bar{P}$ and customers can chose to switch to $\tilde{P}$ (i.e., the opt-in treatment). A qualitative prediction is that only those customers with the largest indirect utility gains (i.e., gains that exceed the switching costs) will opt-in. Thus, the customers who actively switch to the time-varying rate lie within the blue shaded area. Taken together, these customers (the always takers) incur switching costs E; the net gain in utility is area F. Customers represented by area D would have higher utility if they switched to time-varying prices, but the utility gain does not offset the switching costs, so they remain on the flat rate, represented by the red shaded area. Customers represented in areas A, B, and C would experience utility losses switching to the new pricing structure, so they do not switch.

The bottom panel reflects the alternative scenario where $d = \tilde{P}$. The model predicts that consumers who actively opt-out of the program are those with the largest indirect utility losses. The share of customers participating in the time-varying rate (shaded blue) is now much larger. The so-called never takers (red) now incur a switching cost represented by area B to avoid a utility loss under time-varying prices. A share of the complacents experience a negative impact on utility represented by area C, but this utility cost is smaller than the switching cost they would need to incur. Always takers avoid switching costs (area E) and some complacents experience higher level of utility under the time-varying price regime (area D).

[FIGURE 6 HERE]

In this stylized illustration, it is straightforward to show that total consumer welfare is maximized by setting $d = \widetilde{P}$. The welfare gain from switching the default to $\widetilde{P}$ from $\bar{P}$ is $ED - CB$. Of course, in our applied setting, we cannot directly observe the utility each household associates with alternative pricing regimes. To quantitatively estimate the welfare implications of switching the default choice, we would need to explicitly specify the form of the utility function in equation 3. Rather than impose this degree of structure, we introduce a set of weaker assumptions which allow us to empirically evaluate the qualitative predictions of the model and estimate a lower bound on consumers' welfare loss from switching.

To draw empirically testable implications out of the model, we continue to assume that the consumer has well-defined preferences over electricity pricing programs, and that the consumer chooses the program that maximizes her utility. We note that the welfare impacts of switching to a time-varying electricity rate can manifest in three ways. First, any change in electricity expenditures affect residual savings or expenditures on other goods. Second, any re-optimization of energy consumption patterns in response to the price change (such as turning up the thermostat on a hot day) will affect the level of utility derived from energy consumption. Finally, re-optimization of energy consumption patterns can require effort through learning or adjustment costs which we denote A.

If we further assume that utility is quasilinear in electricity and all other goods, a monetary measure of the utility change associated with a switch from $\overline{P}$ to $\widetilde{P}$ can be summarized as:

$$V(\widetilde{P}) - V(\overline{P}) = max\left\{\widetilde{P}'\overline{X} - \bar{P}'\overline{X},\ \widetilde{P}'\widetilde{X} - \overline{P}'\overline{X} - A\right\}$$

where $\tilde{X}$ reflects the optimal vector of electricity consumption under time-varying prices and $\overline{X}$ reflects the optimal vector of electricity consumption under uniform pricing. The first argument in brackets measures the change in electricity expenditures associated with switching to time-varying pricing, holding consumption patterns constant. Importantly, this provides an upper bound on the welfare loss (among structural losers), or a lower bound on the welfare gains (among structural winners). The second argument measures the change in electricity expenditures net of adjustment costs in a scenario where the consumer adjusts consumption in response to time-varying pricing. In theory, the consumer will only choose to re-optimize if the benefits exceed the adjustment costs.

We can estimate $\widetilde{P}'\overline{X} - \overline{P}'\overline{X}$, i.e. the lower bound on customers' willingness to pay to switch to the time-varying rate, using rich data from the pre-intervention period. Figure 7 (a)-(d) summarize the distribution of customer-specific estimates of $\widetilde{P}'\overline{X} - \overline{P}'\overline{X}$ in a histogram. These figures show how approximately half of the consumers in the CPP treatments are structural winners. Under some fairly restrictive assumptions, these figures can be interpreted as empirical analogs to Figure 6. As noted above, these $\widetilde{P}'\overline{X} - \overline{P}'\overline{X}$ measures can only be interpreted as estimates of the monetized change in indirect utility in cases where preferences are fixed and well-defined, utility is quasilinear in electricity and all other goods, and consumers do not re-optimize consumption in response to the change in price structure. Releasing the restrictive no-adjustment assumption, Figure 7 bounds the distribution of utility changes under a rank-preservation assumption (i.e., any re-optimization to $\tilde{X}$ does not change the rank order of utility changes across consumers). This would be violated if, for example, consumers were most likely to reoptimize if they would otherwise pay higher bills under the new pricing ("structural losers" in the terminology used above). In Appendix Table A5, we report versions of equation 2 which allow the response to vary for structural losers and find that, if anything, structural losers are less likely to adjust their consumption.

Having estimated these customer-specific structural gains, we can ask whether the observed participation choices are qualitatively consistent with predictions of the model that assumes well-defined, pre-determined preferences and switching costs. Figure 7 summarizes participation decisions by decile of savings. Similar to Figure 6, we represent the opt-in scenarios in the top two figures and opt-out in the bottom. We use blue shading to represent customers who are participating in the new pricing program and red shading to represent customers who continue to face standard pricing. For example, in Figure 7 panel a, 10% of the households would have experienced losses of more than \$30 over summer 2011 had they been on a CPP rate instead of the flat rate. Of these, however, 15% opted in to the new rate. In general, the patterns depicted in Figure 7 starkly contrast with Figure 6. In all cases, a significant share of the structural losers participate in the new rate and even some of the households that stand to gain the most from the rate without adjusting their consumption opt-out. These patterns cast doubt on the usefulness of a model that presumes switching costs incurred by fully-informed consumers explain the default effect in this context.

7.2. Alternative Explanations

Several alternative explanations for the default effect could apply in this setting. One potential explanation is rooted in rational inattention, a form of bounded rationality. When information is costly to acquire, consumers may choose to act on incomplete information rather than incur the cost to become perfectly informed. Sallee (2014) argues that it can be rational for consumers to choose among energy-consuming durables, like automobiles or home appliances, without acquiring complete information about energy efficiency. Given the relatively small gains from switching to a time-varying pricing regime, a similar argument could apply here. For many customers, it could be rational to rely on cues, such as default choices, rather than invest in collecting full information about this electricity price plan choice.

A second explanation is predicated on the idea that consumers have inconsistent expectations about their own actions. This type of model would predict procrastination, which could be one explanation for consumers remaining on the default plan when they would prefer not to be. For example, customers assigned to the opt-out group may have intended to opt-out of the plan, but never got around to it. Conversely, customers assigned to the opt-in treatment may have intended to opt in but never acted on that intent. If this procrastination behavior is pervasive, it could explain a significant default effect. And, welfare analysis based on a model that rationalizes the impact of a default switch using switching costs can overestimate the cost of the default switch.

A third perspective, which departs even further from a standard model, posits that preferences are constructed – versus uncovered – by consumers as they weigh and experience alternative options. In this setting, observed choices reveal not only the agent's valuation of the alternatives, but also the processing strategies used to construct the preferred choice. In other words, a consumer's preference for time-varying pricing can depend on how the choice is presented and how she comes to understand it. This perspective introduces some additional heuristic explanations for default effects. For one, people may interpret the default choice as an informative suggestion or endorsement helping to guide an otherwise uninformed choice. Or, the default choice can serve as an anchor or point of reference. If preferences are partly determined by how customers are introduced to the new pricing structure, welfare analysis becomes more complicated. Standard approaches that seek to rationalize default effects using switching costs and information costs may overestimate the role of these costs if, in fact, preferences are learned and constructed.

We cannot definitively distinguish between these alternative explanations in our context. Instead, we investigate heterogeneity in default proclivity, systematic differences in follow-on behavior and some survey results from after the experiment. The patterns we uncover provide suggestive evidence on the mechanisms behind the default effect and the implications for customer utility.

7.2.1. Heterogeneity in Default Sensitivity

Table 7 summarizes household-characteristics for never-takers (i.e., households assigned to the opt-out group who actively opt-out), always-takers (i.e., households assigned to the opt-in group who actively opt-in) and imputed values for complacents. To calculate the summary statistics for complacents, we leverage the random assignment across opt-in and opt-out groups which implies that the share of always-takers, never-takers, and complacents will be the same in expectation across the two groups.[20] The three columns on the right summarize statistical significance levels (p-values for the t- or z-test on differences) for each pairwise comparison. The top of the table applies to the CPP treatments and the bottom to TOU.

With respect to average usage, the ratio of peak to off-peak consumption, and electricity bills during summer months, there are very few statistically significant differences across the groups in either the CPP or TOU settings. The indicators for structural winner, summarized in the fourth and fifth rows of both the top and bottom panels, suggest that never-takers were statistically significantly *more* likely than complacents to be structural winners for TOU, which is the opposite of what a switching cost-based model would suggest. Several of the other differences are similarly the opposite sign from what a switching cost model would predict, and are nearly statistically significant.

"My Account" and "My Account logins" reflect actions that customers could proactively take to monitor their consumption in the pre-treatment period. Customers who have historically engaged with these pre-existing information programs are more likely to take an active choice and either opt-in or opt-out. This is true for both CPP and TOU treatments. In both cases, the differences between complacents and always takers as well as between complacents and never takers are statistically significant for both My Account and the number of logins (which provides a measure of how frequently a customer accesses her usage information). If we interpret these variables as proxies for attentiveness, we find that complacent households have historically been significantly less attentive to their electricity consumption. This could reflect that members of the complacent group incur higher costs to engage and monitor their use in general. The lack of engagement with the existing programs could also raise the costs of making an active choice about enrolling in time-varying pricing.

The "low income" indicator summarizes participation in the utility's low-income electricity pricing program. We find that consumers enrolled in the low-income rate are significantly more likely to opt in to time-varying pricing programs and somewhat less likely to opt-out, though the second difference

[20]Specifically, we calculate the mean of each variable for the complacents as follows: $\mu_C = (\mu_{Out} - p_{In} * \mu_{In}) / (p_{Out} - p_{In})$ where μ_{Out} and p_{Out} are the means and proportions for all participants in the opt-out group and μ_{In} and $p_{In}$are the means and proportions for all participants in the opt-in group.

is not statistically significant for CPP. We note that households must proactively sign up for this rate, so the indicator captures the response among relatively attentive and engaged low-income households.

[TABLE 7 HERE]

In sum, we find systematic differences in the extent to which customers have historically been engaged in monitoring their electricity consumption, with complacent households significantly less engaged than other households in the sample. This is consistent with the default effect reflecting inattention (rational or otherwise). The average projected gain or loss from switching to a time-varying rate is quite small (average gains among winners, and average loss among losers, are on the order of $15 over an entire summer). Given that gathering information about consumption patterns and alternative rate structures to make an informed decision requires time and effort, inattention to these savings could be rational.

7.2.2. Heterogeneity in Follow-on Behavior

We also test for heterogeneity in the response to time-varying prices along several dimensions. We first estimate a specification of equation 2 that includes an interaction between the participation indicator and several of the household characteristics summarized in Table 7. Note that the direct effects of these variables on electricity consumption are absorbed by the customer fixed effect.

The top panel of Table 8 reports interactions with the "My Account" indicator. The coefficients on the interaction terms are negative in 11 out of the 12 cases and statistically significant in 6 of those 11. Customers who had signed up for "My Account" prior to the study, and are presumably more attentive to their energy consumption, reduced consumption by significantly more on average during both event and non-event peak hours. Moreover, the average demand response on non-event days among CPP customers comprises a smaller share of the event-day response among these customers. This is consistent with the idea that less attentive customers will rely to a larger extent on modifications that do not require sustained attention. The most striking differences are found among complacents. The coefficients on the interaction term are all negative and larger than the coefficients on the treatment variable alone, though only statistically significant for the CPP group during event hours (column (3)). We note that the responses of complacents enrolled in "My Account" appear more similar to always takers than for complacents who have not activated "My Account". For TOU, the effects are large for complacents, even proportionately larger than for always takers, but the point estimates are small, so they are not statistically significant.

The middle panel of Table 8 tests for systematic differences in price responsiveness among consumers enrolled in the low-income rate. The results indicate that always takers on the low-income rate are significantly less responsive during event and non-event hours for both the CPP and the TOU treatments. This indicates that low-income enrolled customers that actively opted in did not provide as much peak savings.[21] Among complacents, the average demand response among consumers on the low-income rate is also smaller during critical events, although the differences are not statistically significant. The demand response of low-income customers who were susceptible to the default effect – and may be of particular interest to regulators – is statistically indistinguishable from the other complacent households.

Since our study period includes two years of post-intervention data, we can analyze how electricity demand response to the time-varying rates evolves over time. In particular, we can test for differences in this evolution across customers who actively opted in and the complacent households who were nudged in by the opt-out encouragement. We modify equation 2 to include an interaction between the treatment indicator and an indicator for the second summer. The bottom panel of Table 8 summarizes the estimation results. For the CPP treatments, the interaction term is positive for the always takers in the opt-in group (columns 1 and 4) and negative for the complacents (columns 3 and 6). Three out of four of the coefficients are statistically significant.[22] This pattern suggests that demand response is attenuating over time among always takers. In contrast, the average demand response is increasing over time among complacents. This could be due to a growing number of complacents responding over time, or an increasing demand response from those complacent customers who had been actively responding in the first summer.

[TABLE 8 HERE]

In sum, we find that as complacent customers gain experience with the new pricing regime, they mount a more significant demand response. Recall from Section 5.2. that complacents are no more likely than always takers to exit the program after gaining some experience. These results are consistent with the complacents gradually learning about and acclimating to the time-varying rate, and less consistent with a scenario in which complacents had well-formed preferences for the rates, knew they would dislike it but elect to remain on account of high switching costs.

[21] This results holds when we estimate specifications using log consumption. See Table A3.

[22] The results are not as pronounced for the TOU treatment, although columns 1 and 4 suggest that the always takers are responding less over time.

7.2.3. Survey Results

Another source of evidence on households' preferences and decision processes is a set of follow-up surveys that SMUD conducted after the pricing program ended. The survey was sent to all households enrolled on the CPP and TOU pricing plans and a subset of the control group. While the survey respondents are by no means a random subset of the larger sample, the responses can provide some insight into consumers' motivations and sentiments about the pricing programs. The opt-out participants were less likely to respond to the survey – 26% for opt-out (N=566) versus 36% for opt-in (N=183), consistent with the general finding thus far that complacents tend to be less engaged and less responsive. Also, only 60% of the respondents from the opt-out groups demonstrate that they understood the time-varying rates they were paying, compared to around 85% of the respondents from the opt-in group.

Survey responses generally suggest that customers are not averse to the new pricing plans. In both the opt-in and opt-out groups, fewer than 7% disagree with the statement, "I want to stay on my pricing plan." More of the opt-in customers strongly agree with that statement and more of the opt-out customers express, "no opinion," perhaps indicative of their complacency. Similarly, across both groups, almost 90% of respondents are either "Very satisfied" or "Somewhat satisfied" with their current pricing plan, with no statistically significant differences across those two categories by group. In contrast, only 80% of the control group respondents are "very" or "somewhat" satisfied with the standard rate.

Overall, the results in this section suggest that customers who are more engaged with utility programs are more likely to make an active choice and either opt in to or opt out of the time-varying pricing programs. Customers who were expected to have lower bills on the program without changing their behavior (so-called "structural winners") were no more likely to enroll in the program, even if they were engaged in utility programs. We find these patterns inconsistent with explanations for the default effect that rely on consumers performing well-informed, cost-benefit calculations before making their choice and more consistent with other explanations, such as inattention and possibly some form of constructed preferences.

Once on time-varying pricing, consumers who were more attentive are also more likely to respond to the prices, although we still see significant reductions by the less attentive consumers in both the always taker and complacent populations. We also see convergence between always takers and complacents in the second summer, which we take as evidence that nudged consumers acclimated to the new pricing regimes. Finally, at least among consumers who responded to the survey, there seems to be general acceptance of time-varying pricing. In sum, we see these results as consistent with a scenario where consumers are nudged onto the rates, perhaps because they are not paying attention, and once on the rates, they learn to adjust to them and some even prefer them to standard rates.

8. Conclusion

The default effect is one of the most powerful and consistent behavioral phenomena in economics, with examples documented across many settings, including health care, personal finance and internet marketing. This paper studies this phenomenon in a new context – time-varying pricing programs for electricity. Residential customers served by a large municipal utility in the Sacramento area were randomly allocated to one of three groups: (1) a treatment group in which they were offered the chance to opt in to a time-varying pricing program; (2) a treatment group that was defaulted on to time-varying pricing unless they opted out; and (3) a control group. We document stark evidence of a default effect, with only about 20% of customers opting into the new pricing programs and over 90% staying on the programs when it was the default option. This holds for both Critical Peak Pricing and Time-of-Use programs.

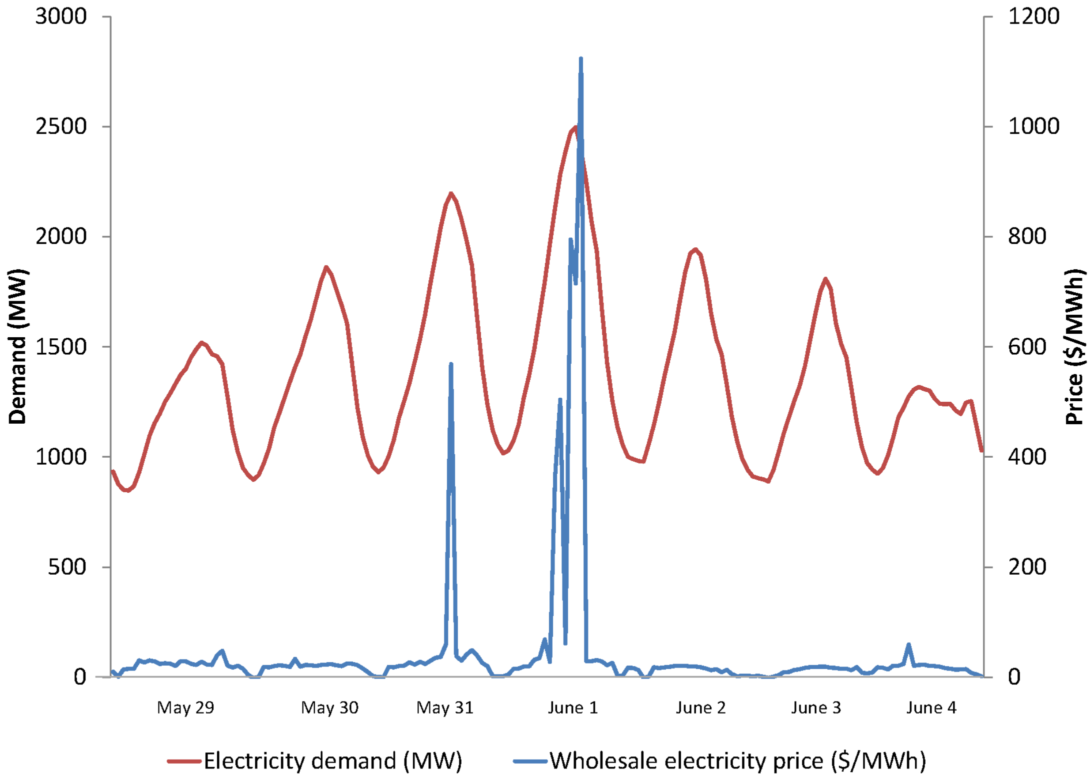
Our study offers several innovations relative to the existing literature on default effects. First, in addition to observing the initial decision that was directly manipulated by the default effect, we also collect detailed data on follow-on behavior. We distinguish between follow-on behavior that modifies the original choice, such as opting out of the time-varying pricing program once it has begun, and behavior that is conditional on, but distinct from, the original choice. In our case, the latter involves adjusting electricity consumption in response to time-varying electric prices. We argue that this conditional behavior can be equally, if not more, important than the original choice. To our knowledge, ours is the first study to identify and study this form of follow-on behavior.

We find that consumers do adjust electricity consumption in response to the time-varying prices, even if they did not actively select them. In particular, the complacents in our study (i.e., consumers who would not have actively enrolled in the pricing program but did not opt out) reduced their consumption during Critical Peak Pricing periods by about 10%, when the price of electricity increased by nearly a factor of 10. Always takers, who actively selected the rates, reduced consumption by more than 25%, although over time, the always takers respond less and the complacents respond more.

Our second innovation is to analyze the initial decisions and follow-on behavior across different groups in our study in order to draw inferences about the likely explanations for the default effect in our context. Our findings cast doubt on explanations for the default effect based on high switching costs. We argue that the data are more consistent with explanations that feature consumers who are not paying attention to the initial choice, but come to understand it and like it.

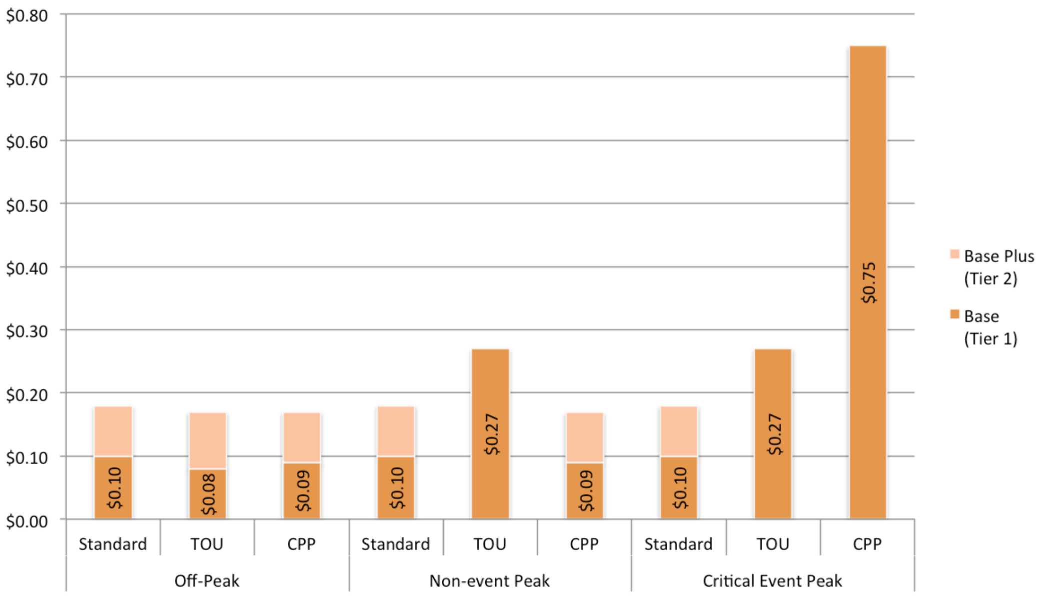
In sum, we find that placing households onto time-varying pricing by default can lead to significantly more customers on time-varying pricing and, more importantly, significantly higher responses to price changes, all without evidence of significant welfare losses. We expect that future work can similarly use follow-on behavior to draw inferences about default effects.

Figure 1: Hourly electricity demand (SMUD) and wholesale electricity price (CAISO)



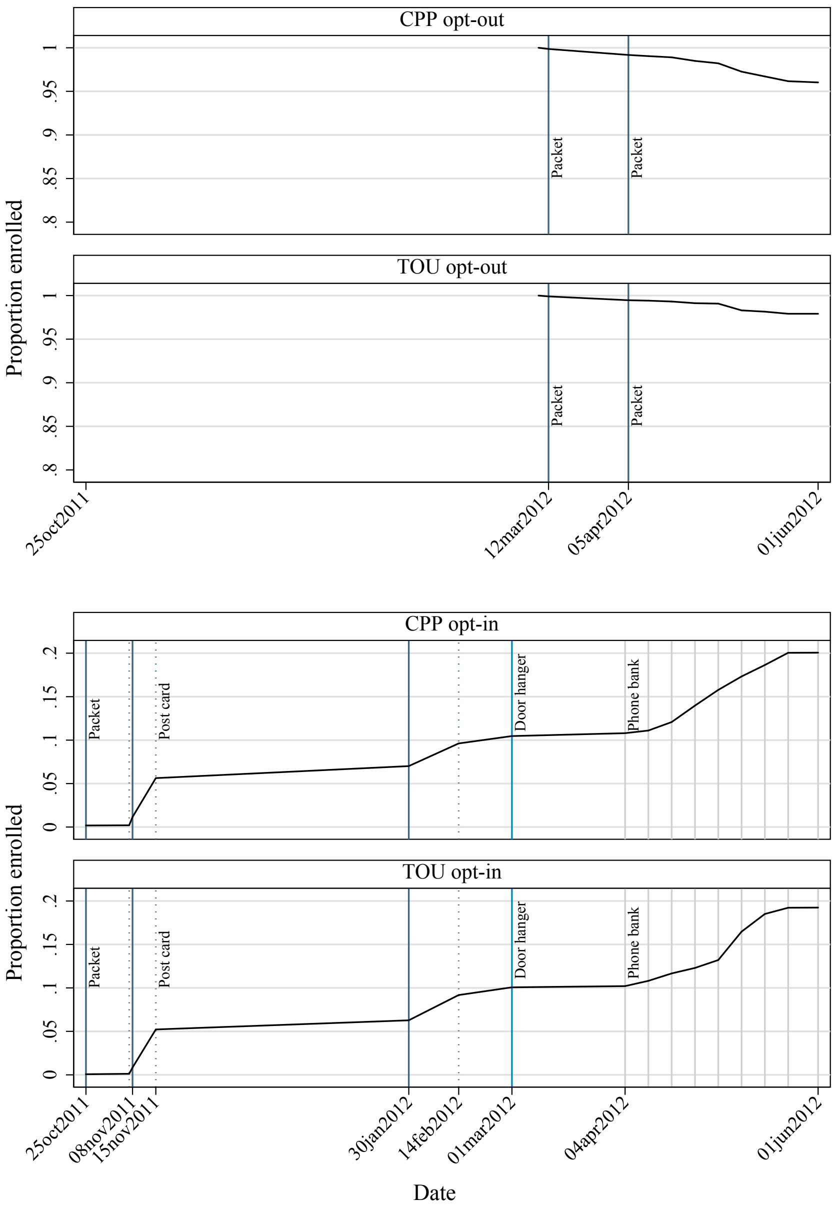
Note: Fluctuations of hourly electricity demand and wholesale spot prices over a week in June, 2011. Wholesale spot prices reported by the California independent system operator (CAISO).

Figure 2: Electricity rate structures



Notes: SMUD electricity rate structures in place during the treatment period. On the base rate, customers are charged $0.1016 for the first 700 kWh in the billing period, with additional usage billed at $0.1830. Participants on the TOU rate were charged an on-peak price of $0.27/kWh between the hours of 4 PM and 7 PM on weekdays, excluding holidays. For all other hours, participants were charged $0.0846/kWh for the first 700 kWh in each billing period, with any additional usage billed at $0.1660/kWh. On the CPP rate, participants were charged a price of $0.75/kWh during CPP event hours. There were 12 CPP events caller per summer on weekdays during the hours of 4 PM and 7 PM on weekdays. For all other hours, participants were charged $0.0851/kWh for the first 700 kWh in each billing period, with any additional usage billed at $0.1665/kWh.

Figure 3: Encouragement efforts



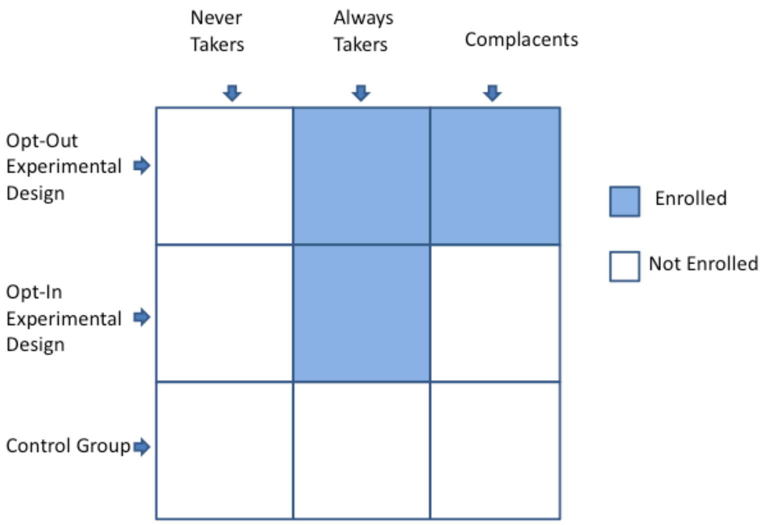
Notes: Pre-period encouragement efforts and enrollment proportion. For opt-out groups, vertical lines indicate dates on which packets were mailed out to the households. For opt-in groups, the first three solid vertical lines are dates on which packets were mailed out, the three dotted vertical lines indicate dates on which follow-up post cards were mailed out, and the final solid vertical line depicts distribution of door hangers on March 1st, 2012. Gray vertical lines between April 4th and June 1st, 2012 indicate phone bank campaign, when calls went out on almost a daily basis. The solid decreasing (increasing) lines in each figure represent the proportion of households in the opt-out (opt-in) that remained enrolled (chose to enroll) in treatment over the course of the recruitment efforts.

Table 1: Comparison of means by treatment assignment

	Controls	CPP		TOU	
		Opt-in (AT)	Opt-out (AT+C)	Opt-in (AT)	Opt-out (AT+C)
Average kWh per day	26.81	26.98 (0.200)	27.19 (0.590)	26.68 (0.169)	26.53 (0.354)
Average kWh during peak hours	5.560	5.595 (0.0433)	5.682 (0.128)	5.538 (0.0367)	5.509 (0.0769)
Peak to off peak ratio	0.203	0.203 (0.000610)	0.203 (0.00185)	0.203 (0.000535)	0.203 (0.00112)
Paperless	0.209	0.209 (0.00465)	0.204 (0.0141)	0.208 (0.00407)	0.193 (0.00850)
Your Account	0.425	0.430 (0.00565)	0.442 (0.0172)	0.432 (0.00495)	0.419 (0.0103)
EAPR	0.194	0.196 (0.00452)	0.210 (0.0137)	0.200 (0.00397)	0.200 (0.00828)
Households	45,839	9,190	846	12,735	2,407

Notes: Table compares household characteristics and pre-period usage statistics across control and treatment groups. Cells contain group means, t-statistics (in parentheses) obtained from a two-sample t-test between treated group and control group. Daily usage is the average per-customer electricity usage during the pre-period summer. Peak to off-peak ratio is the average hourly consumption during peak periods (4-7pm on weekdays) divided by the hourly kWh used during non-peak times during the pre-period. Bill amounts reflect monthly bills. Structural winner is an indicator variable for whether the household would have experienced reduced bills in the pre-period summer had they been enrolled in either the CPP or TOU pricing plans. My Account is an indicator variable equal to one if the customer has enrolled in the online My Account program. My Account logins are the count of logins conditional on having logged in at least once. Paperless indicates that the household elected to receive electronic bills. Low income indicates households had enrolled in the low income rate. Households are eligible for the low income rate if their income does not exceed 200 percent of the federal poverty level.

Figure 4: Identification of always takers, complacents, and never takers



Notes: Figure describes enrollment choice of different customer types under different experimental groups. Rows indicate the three groups into which customers in our sample were randomly assigned: opt-out, opt-in, and control. Columns signify types of customers (never takers, always takers, and complacents). Shading indicates that the customer type enrolls in time-based pricing program under the associated experimental group.

Table 2: Participation rates

	Initial participation		End-line participation		Attrition rate
CPP opt-in (AT)	0.203	(1,589)	0.189	(1,169)	0.068
CPP opt-out (AT+C)	0.962	(703)	0.894	(538)	0.071
TOU opt-in (AT)	0.195	(2,115)	0.181	(1,551)	0.072
TOU opt-out (AT+C)	0.979	(2,021)	0.926	(1,508)	0.055

Notes: Participation rates at beginning and end of enrollment period. Proportions are the count of enrolled customers divided by the count of total customers in each group, counts are the count of enrolled customers. Initial participation reflects the beginning of the treatment period (June 1st, 2012), while endline participation reflects rates at the end of the treatment period (September 30th, 2013). Enrollment is counted if the customer entered the program (either by opting in or by being defaulted in) and did not opt-out before the given date. Customers who moved away are removed from both the count of enrolled customers and the count of total customers on the date they move. The attrition rate is the percentage change between initial and end-line participation.

Table 3: Intent to treat effects

	Critical event		Non-event peak	
	Opt-in	Opt-out	Opt-in	Opt-out
Encouragement (CPP)	-0.129^{***}	-0.305^{***}	-0.029^{***}	-0.094^{***}
	(0.010)	(0.037)	(0.006)	(0.020)
Mean usage (kW)	2.49	2.5	1.8	1.8
Customers	55,028	46,684	55,028	46,684
Customer-hours	4,832,874	4,104,263	31,198,201	26,495,612
Encouragement (TOU)	-0.091^{***}	-0.130^{***}	-0.054^{***}	-0.100^{***}
	(0.008)	(0.019)	(0.006)	(0.013)
Mean usage (kW)	2.49	2.5	1.8	1.8
Customers	55,028	46,684	55,028	46,684
Customer-hours	4,832,874	4,104,263	31,198,201	26,495,612

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$, standard errors clustered by customer.

Notes: Table estimates impact of encouragement assignment on average hourly electricity usage in kilowatts, irrespective of enrollment status. To estimate the critical event hour effects, data include 4-7pm during simulated CPP events in 2011 (hottest 12 non-holiday weekdays) and 4-7pm during actual CPP events in 2012-2013. To estimate the peak period non-event hour effects, data include 4-7pm on all non-holiday weekdays during the 2011, 2012 and 2013 summers, excluding simulated CPP event days in 2011 and excluding actual CPP event days in 2012 and 2013. Intent to treat effects are identified by comparing the opt-in and opt-out experimental groups to the control group. Intent to treat effects are estimated using ordinary least squares. All regressions include customer and hour of sample fixed effects.

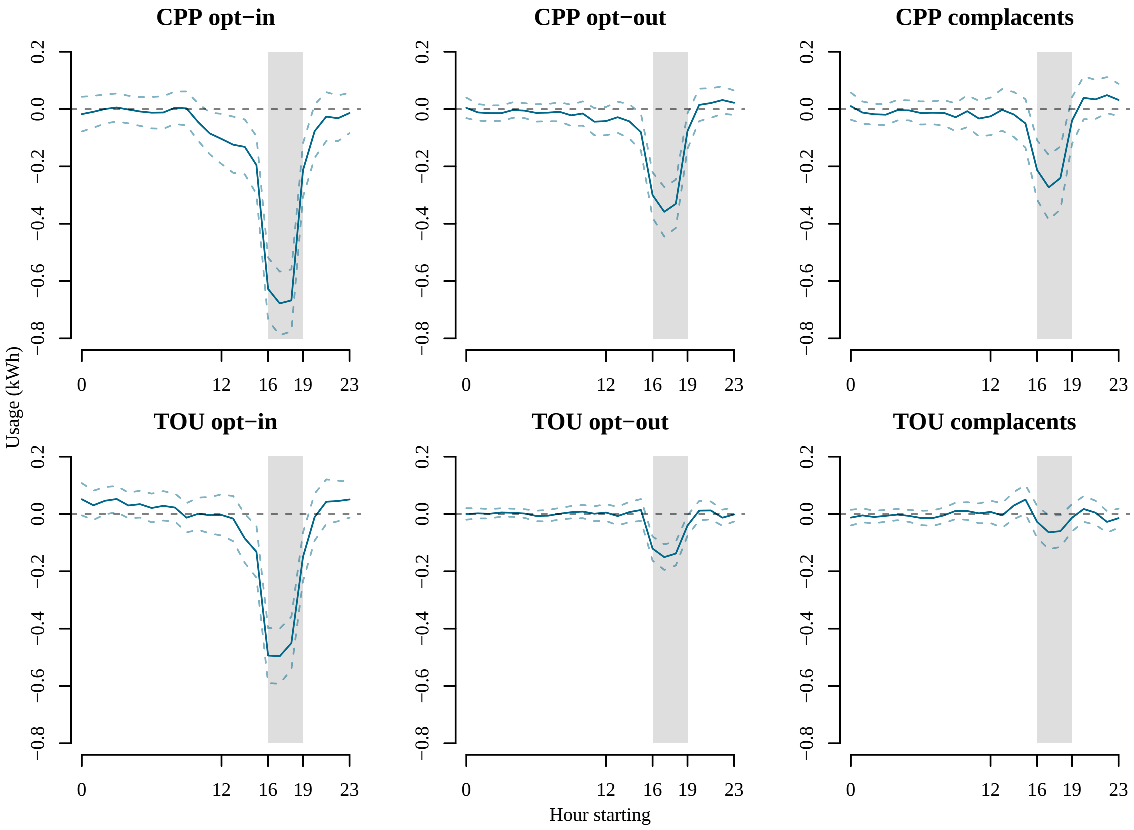
Table 4: Average treatment effects

	Critical event hours			Non-event day peak hours		
	Opt-in (AT)	Opt-out (AT+C)	Complacents (C)	Opt-in (AT)	Opt-out (AT+C)	Complacents (C)
Treatment (CPP)	-0.658^{***}	-0.330^{***}	-0.242^{***}	-0.146^{***}	-0.101^{***}	-0.089^{***}
	(0.051)	(0.040)	(0.053)	(0.031)	(0.022)	(0.028)
Mean usage (kW)	2.49	2.5	2.44	1.8	1.8	1.79
Customers	55,028	46,684	10,036	55,028	46,684	10,036
Customer-hours	4,832,874	4,104,263	880,075	31,198,201	26,495,612	5,679,023
Treatment (TOU)	-0.480^{***}	-0.136^{***}	-0.051^{*}	-0.287^{***}	-0.105^{***}	-0.059^{***}
	(0.044)	(0.020)	(0.027)	(0.029)	(0.014)	(0.018)
Mean usage (kW)	2.49	2.49	2.43	1.79	1.79	1.75
Customers	58,573	48,245	15,142	58,573	48,245	15,142
Customer-hours	5,141,976	4,240,163	1,325,077	33,195,961	27,374,276	8,555,447

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$, standard errors clustered by customer.

Notes: Table estimates impact of enrollment on average hourly electricity usage in kilowatts. AT stands for always takers, C stands for complacents. Sample for critical event hours includes hours between 4pm and 7pm during simulated CPP events in 2011 (hottest 12 non-holiday weekdays between June and September) and actual CPP events in 2012-2013. Sample for non-event day peak hours include hours between 4pm and 7pm of non-holiday, non-CPP event weekdays during the 2011-2013 summers (June to September). Opt-in and opt-out effects estimated by comparing the opt-in and opt-out experimental groups, respectively, to the control group. Complacent effect estimated by comparing the opt-out experimental group to the opt-in experimental group. Treatment effects estimated using two-stage least squares, with randomized encouragement into treatment used as an instrument for treatment enrollment. All regressions include customer and hour of sample fixed effects.

Figure 5: Event day average treatment effects by hour



Notes: Figure depicts hourly impacts of enrollment on electricity usage in kilowatts during event days. Sample for critical event hours includes hours between 4pm and 7pm during simulated CPP events in 2011 (hottest 12 non-holiday weekdays between June and September) and actual CPP events in 2012-2013. Opt-in and opt-out effects estimated by comparing the opt-in and opt-out experimental groups, respectively, to the control group. Complacent effect estimated by comparing the opt-out experimental group to the opt-in experimental group. Treatment effects estimated using two-stage least squares, with randomized encouragement into treatment used as an instrument for treatment enrollment. Dashed lines indicate the 95 percent confidence interval of the estimates with standard errors clustered by customer. The vertical bars indicate the peak period, between 4pm and 7pm.

Table 5: Bill impacts of enrollment

	Opt-in (AT)	Opt-out (AT+C)	Complacents (C)
Treatment (CPP)	-6.515^{***}	-4.499^{***}	-3.121^{**}
	(2.358)	(1.428)	(1.485)
Mean bill ($)	114	114	114
Customers	55,029	46,685	10,036
Customer-months	552,087	468,843	100,552
Treatment (TOU)	−2.816	-1.985^{**}	−1.423
	(2.196)	(0.872)	(0.935)
Mean bill ($)	114	114	113
Customers	58,574	48,246	15,142
Customer-months	587,406	484,364	151,392

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$, standard errors clustered by customer.

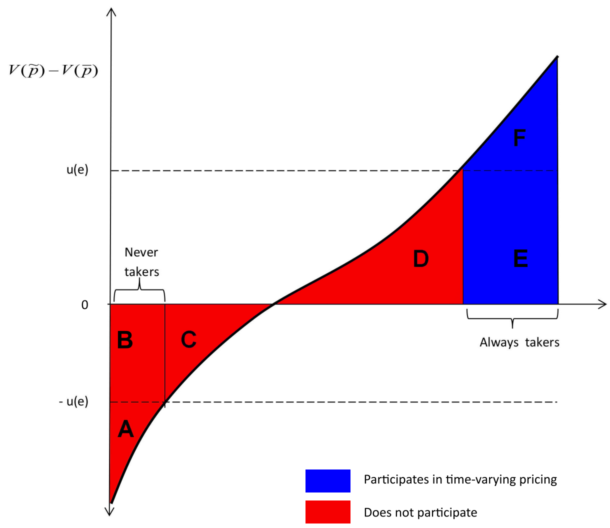
Notes: Table documents impact of treamtment enrollment on monthly bill. Sample composed of summer months. AT stands for always takers, C stands for complacents. Opt-in and opt-out effects estimated by comparing the opt-in and opt-out experimental groups, respectively, to the control group. Complacent effect estimated by comparing the opt-out experimental group to the opt-in experimental group. Treatment effects estimated using two-stage least squares, with randomized encouragement into treatment used as an instrument for treatment enrollment. Treatment effects estimated using two-stage least squares, with randomized encouragement into treatment used as an instrument for treatment enrollment. All regressions include customer and month of sample fixed effects.

Table 6: Cost-effectiveness

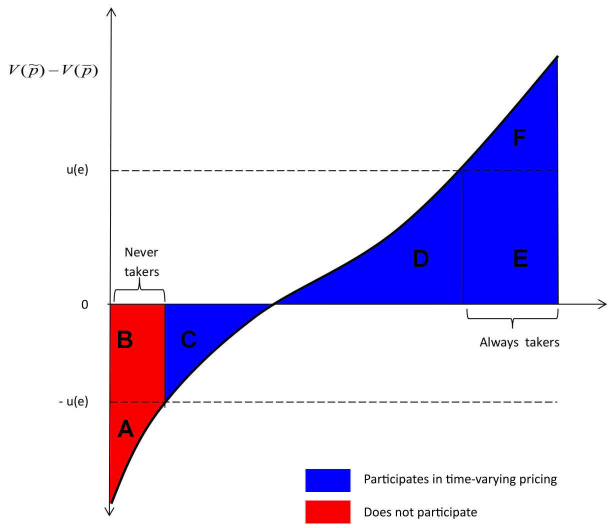
	Benefits		Costs				Benefits - Costs
	Avoided Capacity	Avoided Energy	One-time Fixed Costs	One-time Variable Costs	Recurring Annual Total Costs	10-year NPV	
CPP opt-in	44.0	0.9	1.4	31.0	0.9	36.5	8.4
CPP opt-out	92.1	2.1	1.4	21.0	3.1	38.8	55.4
TOU opt-in	27.0	5.0	0.8	30.0	0.5	32.5	-0.5
TOU opt-out	41.8	7.3	0.8	18.5	1.3	26.1	23.0

Notes: Table estimates cost-effectiveness of each treatment group. All figures in millions of dollars and assume the program is scaled to SMUD's whole residential customer base and run for 10 years. See Appendix section 5 for details.

Figure 6: Program participation under a switching cost model



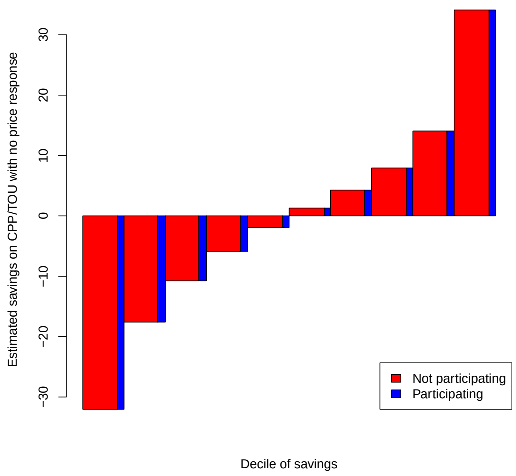
a) Opt-in



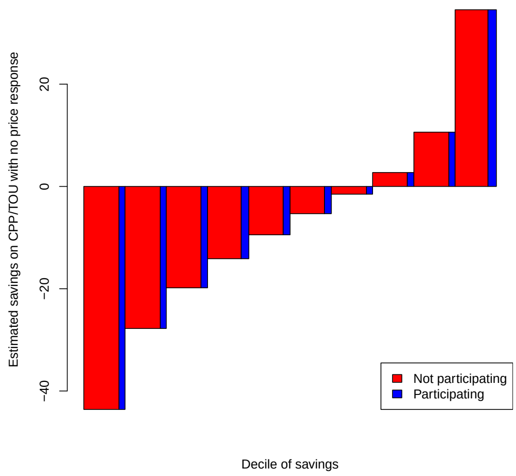
b) Opt-out

Notes: Figures depict predictions of enrollment choices under switching cost model. The vertical axis measures the utility gain to the consumer from adopting time-varying pricing, and $u(e)$ captures the effort costs of switching away from the default pricing regime.

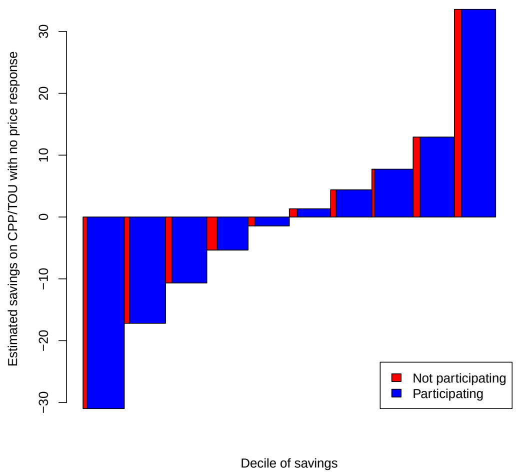
Figure 7: Program participation by estimated savings (Empirical)



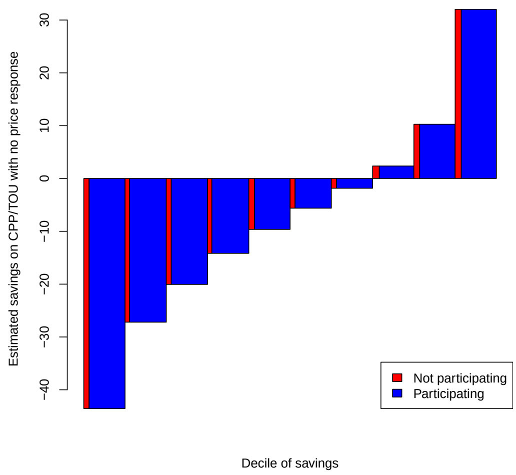
a) CPP opt-in



b) TOU opt-in



c) CPP opt-out



d) TOU opt-out

Notes: Figures document customer enrollment by estimated savings. Vertical axis measures the predicted savings in dollars per household under time-varying pricing compared to standard pricing, based on 2011 consumption. Households grouped by decile of predicted savings.

Table 7: Household characteristics by customer type

	AT	C	NT	AT-C	AT-NT	C-NT
CPP households						
Daily usage	27	27	27	[0.81]	[0.95]	[0.98]
	(16)	(18)	(25)			
Peak to off-peak	1.77	1.78	1.79	[0.62]	[0.76]	[0.99]
	(0.56)	(0.57)	(0.55)			
Bill amount	106	110	113	[0.42]	[0.59]	[0.83]
	(77)	(90)	(132)			
Structural winner (CPP)	0.50	0.52	0.49	[0.34]	[0.87]	[0.51]
	(0.50)	(0.50)	(0.50)			
Structural winner (TOU)	0.35	0.34	0.33	[0.73]	[0.54]	[0.70]
	(0.48)	(0.48)	(0.47)			
My Account	0.54	0.42	0.52	[0.00]	[0.64]	[0.03]
	(0.50)	(0.49)	(0.50)			
My Account logins	9.16	6.65	11.81	[0.00]	[0.30]	[0.04]
	(23.00)	(2.86)	(28.35)			
Paperless	0.24	0.19	0.18	[0.02]	[0.12]	[0.80]
	(0.43)	(0.40)	(0.39)			
Low income	0.29	0.19	0.15	[0.00]	[0.00]	[0.32]
	(0.45)	(0.40)	(0.36)			
TOU households						
Daily usage	27	26	27	[0.22]	[0.70]	[0.33]
	(16)	(17)	(18)			
Peak to off-peak	1.74	1.78	1.73	[0.03]	[0.79]	[0.12]
	(0.54)	(0.63)	(0.52)			
Bill amount	107	108	115	[0.81]	[0.19]	[0.25]
	(81)	(85)	(98)			
Structural winner (CPP)	0.53	0.50	0.57	[0.05]	[0.14]	[0.01]
	(0.50)	(0.50)	(0.50)			
Structural winner (TOU)	0.35	0.33	0.39	[0.17]	[0.13]	[0.02]
	(0.48)	(0.47)	(0.49)			
My Account	0.53	0.39	0.48	[0.00]	[0.07]	[0.01]
	(0.50)	(0.49)	(0.50)			
My Account logins	8.25	5.91	10.79	[0.00]	[0.05]	[0.00]
	(25.90)	(4.36)	(20.66)			
Paperless	0.24	0.18	0.22	[0.00]	[0.52]	[0.11]
	(0.43)	(0.38)	(0.42)			
Low income	0.29	0.18	0.11	[0.00]	[0.00]	[0.00]
	(0.46)	(0.40)	(0.32)			

Notes: Table compares household characteristics by customer types. AT, C, and NT indicate always takers, complacents, and never takers, respectively. Columns (1) through (3) are means with standard deviations in parentheses, where (1) and (3) are sample means and standard deviations and (2) is computed from a comparison of the participants in the opt-out and opt-in groups. Columns (4) through (6) show p-values in brackets for differences between listed groups.

Table 8: Heterogenous treatment effects (My Account, low income, second year)

	Critical event hours			Non-event day peak hours		
	Opt-in (AT)	Opt-out (AT+C)	Complacents (C)	Opt-in (AT)	Opt-out (AT+C)	Complacents (C)
My Account						
Treatment (CPP)	−0.600***	−0.225***	−0.151***	−0.152***	−0.077***	−0.063**
	(0.080)	(0.045)	(0.056)	(0.049)	(0.026)	(0.032)
× My Account	−0.108	−0.251***	−0.238**	0.012	−0.057	−0.067
	(0.104)	(0.085)	(0.117)	(0.063)	(0.046)	(0.062)
Treatment (TOU)	−0.336***	−0.080***	−0.032	−0.204***	−0.065***	−0.039*
	(0.070)	(0.024)	(0.030)	(0.046)	(0.017)	(0.021)
× My Account	−0.274***	−0.143***	−0.055	−0.157***	−0.099***	−0.058
	(0.089)	(0.043)	(0.059)	(0.059)	(0.030)	(0.040)
Low income						
Treatment (CPP)	−0.815***	−0.370***	−0.267***	−0.181***	−0.096***	−0.075**
	(0.066)	(0.047)	(0.060)	(0.040)	(0.025)	(0.032)
× Low income	0.543***	0.176**	0.104	0.122**	−0.023	−0.076
	(0.098)	(0.089)	(0.125)	(0.062)	(0.051)	(0.072)
Treatment (TOU)	−0.547***	−0.148***	−0.061**	−0.321***	−0.111***	−0.063***
	(0.056)	(0.024)	(0.031)	(0.037)	(0.017)	(0.021)
× Low income	0.227***	0.055	0.051	0.117**	0.026	0.020
	(0.086)	(0.043)	(0.061)	(0.057)	(0.030)	(0.042)
Year 2						
Treatment (CPP)	−0.714***	−0.298***	−0.186***	−0.161***	−0.079***	−0.057**
	(0.054)	(0.043)	(0.056)	(0.031)	(0.022)	(0.029)
× Year 2	0.126**	−0.069*	−0.124**	0.036	−0.051**	−0.075**
	(0.054)	(0.037)	(0.049)	(0.035)	(0.023)	(0.030)
Treatment (TOU)	−0.545***	−0.156***	−0.058**	−0.310***	−0.112***	−0.062***
	(0.046)	(0.021)	(0.028)	(0.029)	(0.014)	(0.018)
× Year 2	0.146***	0.044**	0.017	0.056*	0.018	0.007
	(0.049)	(0.020)	(0.027)	(0.033)	(0.013)	(0.017)

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$, standard errors clustered by customer.

Notes: Table estimates heterogenous treatment impacts on hourly energy usage in kW by enrollment in My Account, low income indicator, and second year of program. My Account is an indicator variable equal to one if the customer has enrolled in the online My Account program. Low income is households enrolled in the low income rate. Year 2 is the second year of treatment period. For columns 1, 2, 4, and 5, regressors are instrumented with indicators for encouragement group and its interaction with the indicator variable for structural winners. Sample for columns 1, 2, 4, and 5 is composed of the control group and given treatment group. For columns 3 and 6, the instruments are enrollment into opt-out group and its interaction with the indicator variable for structural winners and sample includes only opt-in and opt-out treatment groups. Event hours include simulated critical peak events in 2011 and actual events in 2012 and 2013. Non-event peak day hours include all peak hours excluding critical event hours. All models include customer and hour of sample fixed effects, plus an interaction between the post-treatment period and given dimension of heterogeneity. Standard errors clustered by customer in parentheses.

References

Abadie, A. and S. Gay (2006). The impact of presumed consent legislation on cadaveric organ donation: A cross-country study. *Journal of Health Economics 25*(4), 599–620.

Allcott, H. and J. B. Kessler (2015). The welfare effects of nudges: A case study of energy use social comparisons. *NBER Working Paper*.

Barkan, R. and J. R. Busemeyer (2003). Modeling dynamic inconsistency with a changing reference point. *Journal of Behavioral Decision Making 16*(4), 235–255.

Bellman, S., E. J. Johnson, and G. L. Lohse (2001). To opt-in or opt-out?: It depends on the question. *Communications of the ACM 44*(2), 25–27.

Bernheim, B. D., A. Fradkin, and I. Popov (2015). The Welfare Economics of Default Options in 401 (k) Plans. *The American Economic Review 105*(9), 2798–2837.

Beshears, J., J. J. Choi, D. Laibson, and B. C. Madrian (2009). the importance of default options for retirement saving outcomes: Evidence from the United States. In *Social Security Policy in a Changing Environment*, pp. 167–195. University of Chicago Press.

Boiteux, M. (1964a). The Choice of Plant and Equipment for the Production of Electricity. In J. R. Nelson (Ed.), *Marginal Cost Pricing in Practice*, pp. 199–214. Englewood, NJ: Prentice-Hall.

Boiteux, M. (1964b). Marginal Cost Pricing. In J. R. Nelson (Ed.), *Marginal Cost Pricing in Practice*, pp. 51–58. Englewood, NJ: Prentice-Hall.

Borenstein, S. and S. Holland (2005). On the Efficiency of Competitive Markets with Time-Invariant Retail Prices. *RAND Journal of Economics 36*(3), 469–93.

Cappers, P. and S. Sheer (2016). American recovery and reinvestment act of 2009: Final report on customer acceptance, retention, and response to time-based rates from consumer behavior studies. Lbnl-1007279, LBNL.

Carroll, G. D., J. J. Choi, D. Laibson, B. C. Madrian, and A. Metrick (2009, November). Optimal Defaults and Active Decisions. *The Quarterly Journal of Economics*, 1639–1674.

Choi, J. J., D. Laibson, B. C. Madrian, and A. Metrick (2002). Defined contribution pensions: Plan rules, participant choices, and the path of least resistance. In James M. Poterba (Ed.), *Tax Policy and the Economy, Volume 16*, pp. 67–114. Cambridge, MA: MIT Press.

Choi, J. J., D. Laibson, B. C. Madrian, and A. Metrick (2004). For better or for worse: Default effects and 401 (k) savings behavior. In *Perspectives on the Economics of Aging*, pp. 81–126. University of Chicago Press.

Department of Energy (2012, July). Smart grid investment grant program. Staff Report, United States Department of Energy.

EPRI (2012, October). Understanding Electric Utility Consumers - Summary Report: What We Know and What We Need to Know. Final Report, Electric Power Research Institution (EPRI).

FERC (2014, December). 2014 Assessment of Demand Response and Advanced Metering. Staff Report, U.S. Federal Energy Regulatory Commission (FERC).

Handel, B. R. (2013). Adverse selection and inertia in health insurance markets: When nudging hurts. *The American Economic Review 103*(7), 2643–2682.

Handel, B. R. and J. T. Kolstad (2015). Health insurance for "humans": Information frictions, plan choice, and consumer welfare. *The American Economic Review 105*(8), 2449–2500.

Hoeffler, S. and D. Ariely (1999). Constructing stable preferences: A look into dimensions of experience and their impact on preference stability. *Journal of Consumer Psychology 8*(2), 113–139.

Institute for Electric Innovation (2017). Electric Company Smart Meter Deployments: Foundations for a Smart Grid. Technical report, The Edison Foundation.

Jimenez, L., J. Potter, and S. George (2013). SmartPricing Options Interim Evaluation. Technical report, U.S. Department of Energy and Lawrence Berkeley National Laboratory.

Johnson, E. J., S. Bellman, and G. L. Lohse (2002, February). Defaults, Framing and Privacy: Why Opting In-Opting Out. *Marketing Letters 13*(1), 5–15.

Johnson, E. J. and D. Goldstein (2003, November). Do Defaults Save Lives? *Science 302*(5649), 1338–1339.

Johnson, E. J., J. Hershey, J. Meszaros, and H. Kunreuther (1993). Framing, probability distortions, and insurance decisions. *Journal of Risk and Uncertainty 7*, 35–51.

Ketcham, J. D., N. V. Kuminoff, and C. A. Powers (2016). Estimating the heterogeneous welfare effects of choice architecture: An application to the medicare prescription drug insurance market. *NBER Working Paper*.

Kowalski, A. E. (2016). Doing more when you're running late: Applying marginal treatment effect methods to examine treatment effect heterogeneity in experiments. *NBER Working Paper*.

Kressel, L. M. and G. B. Chapman (2007). The default effect in end-of-life medical treatment preferences. *Medical Decision Making 27*(3), 299–310.

Madrian, B. C. and D. F. Shea (2001). The Power of Suggestion: Inertia in 401(k) Participation and Savings Behavior. *The Quarterly Journal of Economics 116*(4), 1149–1187.

Park, C. W., S. Y. Jun, and D. J. MacInnis (2000). Choosing what I want versus rejecting what I do not want: An application of decision framing to product option choice decisions. *Journal of Marketing Research 37*(2), 187–202.

Pichert, D. and K. V. Katsikopoulos (2007). Green defaults: Information presentation and pro-environmental behaviour. *Journal of Environmental Psychology 28*(1), 63–73.

Potter, J. M., S. S. George, and L. R. Jimenez (2014). Smartpricing options final evaluation. Technical report.

Sallee, J. M. (2014). Rational inattention and energy efficiency. *The Journal of Law and Economics 57*(3), 781–820.

Simon, D., D. C. Krawczyk, A. Bleicher, and K. J. Holyoak (2008). The transience of constructed preferences. *Journal of Behavioral Decision Making 21*(1), 1–14.

Sims, C. A. (2005). Rational Inattention: A Research Agenda. *Discussion Paper Series 1: Economic Studies 34*.

Wang, W. and T. Ida (2017). Default Effect Versus Active Decision: Evidence from a Field Experiment in Los Alamos. *Kyoto University Discussion Paper No. E-14-010*.

APPENDIX

A. Load Shape Balance across Treatment Groups

Table 1 in the main text discusses balance in covariates between control and treatment groups. Because we analyze consumption across hours of the day, we are also concerned about balance in hourly consumption profiles. Figure A1 plots each treatment group's hourly electricity consumption overlaid with control group consumption, obtained from a regression of electricity consumption on a set of indicator variables for each hour. The left side of the figure compares customers who were offered the opportunity to opt-in to either the CPP or TOU treatment to control customers, while the right side compares customers who were defaulted on to either the CPP or TOU plan to the same control customers. The graph highlights the variation in electricity consumption over the day, from a low below .75 kWh in the middle of the night to a peak nearly three times as high at 5PM. This consumption profile is typical across electricity consumers around the country, although SMUD customers' peak consumption tends to be slightly later than for customers of other utilities.

The graph also highlights that we cannot reject that both sets of treated households had statistically identical consumption profiles to the control households. The graphs in Figure A2 show the differences between treated and control, highlighting that these are well within the 95 percent confidence intervals for all hours. The standard errors for the CPP opt-out group are notably larger since that group had one tenth as many households.

B. Alternative Specifications

Tables A1, A2, and A3 report results similar to those in Tables 3, 4 and 8 in the text using the log of hourly consumption as the dependent variable. The results are very consistent across specifications: the ITT estimate is about twice as large in the CPP opt-out treatment compared to the CPP opt-in.

Table A4 reports results similar to those in Table 4 in the text using only post-treatment period data.

Table A5 reports specifications similar to those in Table 8, where the interaction term is for households that were structural winners. The coefficients on the interaction terms are all either negative or insignificant, suggesting that structural winners if anything reacted more to changes in prices.

C. Assumptions Underlying the LATE Estimates

We take a standard approach to identifying the local average treatment effects using our two encouragement instruments (i.e., the opt-in offer and the opt-out offer, respectively). This section explains how we leverage this research design to estimate local average treatment effects in different sub-groups of our study sample.

Let $D_i = 1$ if the individual participates in the dynamic pricing program. Let $D_i = 0$ if the individual remains in the standard pricing regime. Let $Z_i = 1$ if the individual was assigned to the opt-in encouragement treatment, let $Z_i = 2$ if the individual was assigned to the opt-out; otherwise $Z_i = 0$.

Conceptually, we define four sub-populations:

- Never takers (NT): Do not opt in if $Z_i = 1$. Opt out if $Z_i = 2$.
- Complacents (C): Do not opt in if $Z_i = 1$. Do not opt out if $Z_i = 2$.
- Always takers (AT): Opt in if $Z_i = 1$. Do not opt out if $Z_i = 2$.
- Defiers (D): Opt in if $Z_i = 1$. Opt out if $Z_i = 2$.

To identify the LATE separately for the opt-in and opt-out interventions, respectively, we make the following assumptions:

- **Unconfoundedness**: We assume that the assignment of the encouragement intervention Z_i is independent of/orthogonal to other determinants of energy consumption. This assumption is satisfied (in expectation) by our experimental research design.
- **Stable unit treatment values:** Electricity consumption at household *i* is affected by the participation status of household *i* but not the participation decisions of other households.
- **Exclusion restriction:** Our encouragement intervention affects energy consumption only indirectly through the effect on pricing program participation.
- **Monotonicity**: Our encouragement intervention weakly increases (and never decreases) the likelihood of participation in the pricing program. This implies that there are no defiers.

Let π^{NT}, π^{C}, and π^{AT}, denote the population proportions of never takers, complacents, and always takers, respectively. Let $Y_i(D_i = 1)$ and $Y_i(D_i = 0)$ define the potential electricity consumption outcomes associated with consumer *i* conditioning on participation in the dynamic pricing program.

Given the exclusion restriction, these potential outcomes need not condition on the encouragement intervention.

With the opt-in design, the average electricity consumption among households assigned to the control group ($Z_i = 0$) is:

$$E[Y_i|Z_i = 0] = \pi^{NT}E[Y_i(0)|NT] + \pi^{C}E[Y_i(0)|C] + \pi^{AT}E[Y_i(0)|AT].$$

The average consumption among households assigned to the opt-in encouragement:

$$E[Y_i|Z_i = 1] = \pi^{NT}E[Y_i(0)|NT] + \pi^{C}E[Y_i(0)|C] + \pi^{AT}E[Y_i(1)|AT].$$

Mechanically, it is straightforward to construct an estimate of the effect of the pricing program on average consumption among always takers by taking the difference in these two expectations and dividing by π^{AT}:

$$LATE^{AT} = \frac{E[Y_i|Z_i = 0] - E[Y_i|Z_i = 1]}{\pi^{AT}} = E[Y_i(0)|AT] - E[Y_i(1)|AT],$$

where $\pi^{AT}$is estimated by the share of participants in the encouraged group. We take a similar approach using the opt-out design to construct an estimate of the local average treatment effect in the combined AT and C groups:

To isolate the average treatment effect in the complacent population, we compare outcomes across the two groups assigned to $Z_i = 1$ and $Z_i = 2$, respectively. Taking the difference across these two groups and dividing by π^C yields:

$$LATE^{C} = \frac{E[Y_i|Z_i = 1] - E[Y_i|Z_i = 2]}{\pi^{C}} = E[Y_i(0)|C] - E[Y_i(1)|C].$$

The estimate of π^C is obtained by taking the difference in program participation across the opt-in and opt-out treatments.

If our encouragement intervention affects electricity consumption directly, this will violate the exclusion restriction and confound our ability to identify these local average treatment effects. The exclusion restriction would be violated, for example, if the encouragement (i.e., the dynamic price offers) increased the salience of energy use. In this scenario, potential outcomes should be represented by $Y_i(D_i, Z_i)$. Taking the opt-in design as an example, the local average treatment effect among always takers is now more accurately estimated as:

$$LATE^{AT} = \frac{E[Y_i(0,0)|AT] - E[Y_i(0,1)|AT]}{\pi^{AT}} - \frac{\Delta_C}{\pi^{AT}} - \frac{\Delta_{NT}}{\pi^{AT}},$$

where $\Delta_C = E[Y_i(0,0)|C] - E[Y_i(0,1)|C]$ and $\Delta_{NT} = E[Y_i(0,0)|NT] - E[Y_i(0,1)|NT]$. If these encouragement-induced changes in electricity consumption among non-participants are not equal to zero, they will bias our LATE estimates.

We cannot estimate these Δ terms directly. However, we can bound the bias from these terms by comparing consumption patterns at households that did not participate in the dynamic pricing program across encouraged and unencouraged groups. These differences are difficult to interpret as they compare electricity consumption across different subsets of the non-participant population, but they do provide some sense of how large the bias from violating the exclusion restriction might be.

We re-estimate equation 1 using only those households who did not participate in dynamic pricing. Table A6 summarizes these comparisons. For the opt-in experiments, these results represent the difference in average consumption among households assigned to the control group and the average consumption among all non-participants who received the opt-in offer (i.e., complacents and never takers). For the opt-out experiments, we compare consumption across all households assigned to the control group and the never-takers in the encouraged group.

Some of these differences are statistically different from zero. For example, we estimate a significant difference of -0.025 across encouraged non-participants and unencouraged in the opt-in TOU experiment. It seems likely that some of this difference is driven by differences in composition - we are comparing consumption across all households in the control group with consumption of never-takers and complacents in the encouraged group. However, if we interpret this difference as entirely caused by the opt-out intervention, this would imply that our local average treatment effect estimate of energy reduction by the always taker group overstates the true effect by $\frac{0.025}{0.19} = 0.13$.

Also, our estimates of average treatment effects for complacents (see Table 4, Figure 5, Table 5 and Table 8) assume that always takers who actively enroll in the pricing programs under the opt-in treatment do not behave differently than always takers who are defaulted onto the programs through the opt-out treatment. In other words, we are assuming that $E[Y_i(1,1)|AT] = E[Y_i(1,2)|AT]$. Again, we cannot verify this assumption directly, but we can use the recruit-and-delay treatment group who were encouraged to opt-in to the TOU program but only placed on the pricing schedule in 2014 after our sampling frame (i.e., delayed), rather than in 2012. (This group is introduced in footnote 7 in the main text.) This allows us to estimate $E[Y_i(0,0)] - E[Y_i(0,1)|AT]$, and provides insight on customers who actively enrolled in the program (i.e., are always takers) but did not immediately face time-varying prices.

We re-estimate equation 2 using this group and find that the customers who opted-in but for whom time-based pricing was delayed reduced their usage by a statistically significant 0.08 kwh on average during the 2012 and 2013 summers, despite experiencing an identical price schedule to the control group (see Table A7). If we assume that this effect was due entirely to the act of enrolling, then it suggests our assumption is violated. However, another likely explanation for this result is that some customers were not fully informed about the delay in their start date. In order to investigate this possibility, we conduct two tests. First, we examine whether the treatment effect declined more from the first to the second summer of treatment, which would indicate that households who were previously misinformed about their 2014 start date became aware by the second summer that they were not yet on time-varying prices and reduced their energy saving behaviors accordingly. We find that this is the case: Table A7, column 2 shows that there is about a 59% (0.065/0.11) reduction in savings between years 1 and 2, larger than the 18% (0.056/0.31) reduction in the comparable non-delayed group seen in Table 8, third panel, fourth column. Our second test of treatment start date confusion examines whether the TOU opt-in and opt-out groups reduced usage in the two months prior to the treatment period start date on June 1, 2012. Table A8 documents this test, which demonstrates that both opt-in and opt-out households did reduce usage relative to the control group even before the treatment began. The opt-in households reduced slightly more, although the point estimate for the opt-in effect is not statistically significant at conventional levels. Finally, in the survey, customers in the recruit and delay group were much more likely to think (wrongly) that they were on the time-of-use rate compared to the control group.

Overall, these results suggest that 0.08 is likely an overestimate of the extent to which actively enrolling influenced energy consumption. As discussed in the main text, this assumption only affects our estimates of the complacents' behavior, and, to the extent actively enrolling leads to reductions, our main estimates understate the true complacent response. Finally, we note that the complacents' response does not enter into the cost-benefit calculations in Section 6.

D. Modeling Attrition out of the Program

As reported in Table 2, approximately 6-7% of the customers on the dynamic pricing programs opted to leave the program at some point during the two-year study. Figure A3 reports Kaplan-Meier survival estimates for each of the four treatment groups. The vertical orange lines indicate critical event days and the vertical blue line indicates the date on which the second summer reminder letter was sent out to all study participants letting them know that the rate would start again. We see some attrition from all four groups before the event days started, slightly more attrition from the CPP groups throughout the first summer, and then a relatively big drop after the reminder.

To gain more insight into attrition timing, we model the propensity for customers to leave the dynamic pricing programs once enrolled using an accelerated failure time (AFT) model. We elected to use an AFT model instead of a proportional hazard model as it better accommodates the impact of specific events, such as the critical peak pricing days. In the AFT, the exponential of the estimated coefficient on a variable indicates the "acceleration factor" in the influence on that variable on the survival time. The results of the hazard analysis are presented in Table A9.

One might expect that customers who actively opted in to the new rates would be less likely to later change their minds and opt-out. In fact, the attrition rates are similar across opt-in and opt-out for the TOU rates, and the opt-in customers were even quicker to get out of the rates than the opt-out in the CPP case. In particular, the CPP opt-out group had a survival time (i.e., time remaining in treatment before dropping out) that was 40 percent higher than (calculated as exp(0.339)) the opt-in group, which is the omitted category, although the difference is not statistically significant. This could reflect the fact that opt-in customers are self-selected to have low switching costs.

As for other customer-level impacts, there is some evidence that low-income customers were less likely to drop out of the study quickly relative to non-EAPR customers. Structural winners tended to remain in the study longer than those that were not structural winners, although the difference is not statistically significant. Customers with "Your Account" were no more likely to drop out of the study more quickly.

In the case of effects over time, the second summer reminder had a strong effect that accelerated the rate of drop-outs (reduced the survival time) across all the treatment groups. The occurrence of CPP event days enters the model in the following way: there is an indicator variable included for CPP event days for the two CPP treatment groups ("CPP event date"). In addition, the variable "CPP event date count in each summer" is a variable that increases by one each occurrence of a CPP event date within each summer. So, it is equal to 1 on the first occurrence of a CPP event both in the first and second summer, and is equal to 2 for the second occurrence of a CPP event within each summer, etc. The results for CPP event days indicate that for the opt-in CPP treatment group, the experience of CPP event days reduced the survival time in the study by slightly less than the reminder. However, this effect was attenuated over the course of more CPP events within each summer. For the CPP opt-out treatment group, however, the effect of experiencing a CPP event at all is close to zero (the sum of the coefficient and the interaction), and the effect of CPP events appears to increases the rate of drop-outs slightly over multiple events. Finally, we tested whether there was any disproportional additional effect of experiencing a string of consecutive (two or three in a row) events. There does not appear to be a discernible effect of experiencing multiple CPP event beyond the baseline CPP event effect for either CPP treatment group.

The bottom rows of the table list the number of participants and the number of dropouts for each treatment group. As emphasized in the main text, we find the attrition results suggestive but are hesitant to put too much emphasis on them given the relatively small number of dropouts.

E. Cost-Benefit Analysis

This section describes the cost-benefit calculations reported in Section 6. Many of the assumptions used in our calculations are summarized in Potter et al. (2014), a consulting report that provided, among other things, a cost-benefit calculation of several components of the SMUD program. Other assumptions are based on personal communications with SMUD employees and their consultants.

E.1. Benefits

At a high level, reduced demand during CPP and TOU peak hours avoids two types of expenses – the energy associated with generating electricity during these hours and the expected cost of adding new capacity to meet peak demand, where the expectation is taken over the probability that demand in a particular hour would drive capacity expansion decisions. The components of the benefit calculations are summarized in Figure A4.

Consider the first row, reflecting capacity benefits. The first box represents assumptions on the cost of adding a new peaking plant. Our calculations are based on proprietary information provided by SMUD and summarized in Potter et al. (2014). As reported by Potter et al., the costs "range from roughly $50 to $80/kW-year in the first few forecast years and increase to around $125/kW-year by the end of the forecast period" (p. 112, Potter et al. 2014). These costs are slightly lower than other estimates of generation capacity costs from Northern California. For example, the California Public Utilities Commission (CPUC) publishes capacity values for assessing the cost effectiveness of demand response programs. The "Generation Capacity Values" range from $174 to $209/kW-year for 2012-

14, considerably higher than the numbers SMUD uses. Notably, SMUD did not include the capacity costs associated with the transmission and distribution system. According to the CPUC model, those can account for approximately 25% of the capacity benefits of a peak demand reduction program, so SMUD's decision likely understates the benefits of the program. The values represented by the second box, "# of Enrolled Customers on Time-Variant Pricing Plans," reflect participation rates, summarized in Table 2, multiplied by 600,000, an estimate of the number of customers SMUD will have in 2018. We assumed a customer attrition rate of approximately 7% per year. As shown in Table 2, attrition rates over the 16 months the program operated were approximately 5.5 to 7 percent. We converted these to annual attrition rates and then added 2% to account for customers moving out of SMUD's service territory, assuming that customers who moved within the service territory would remain on the rate.

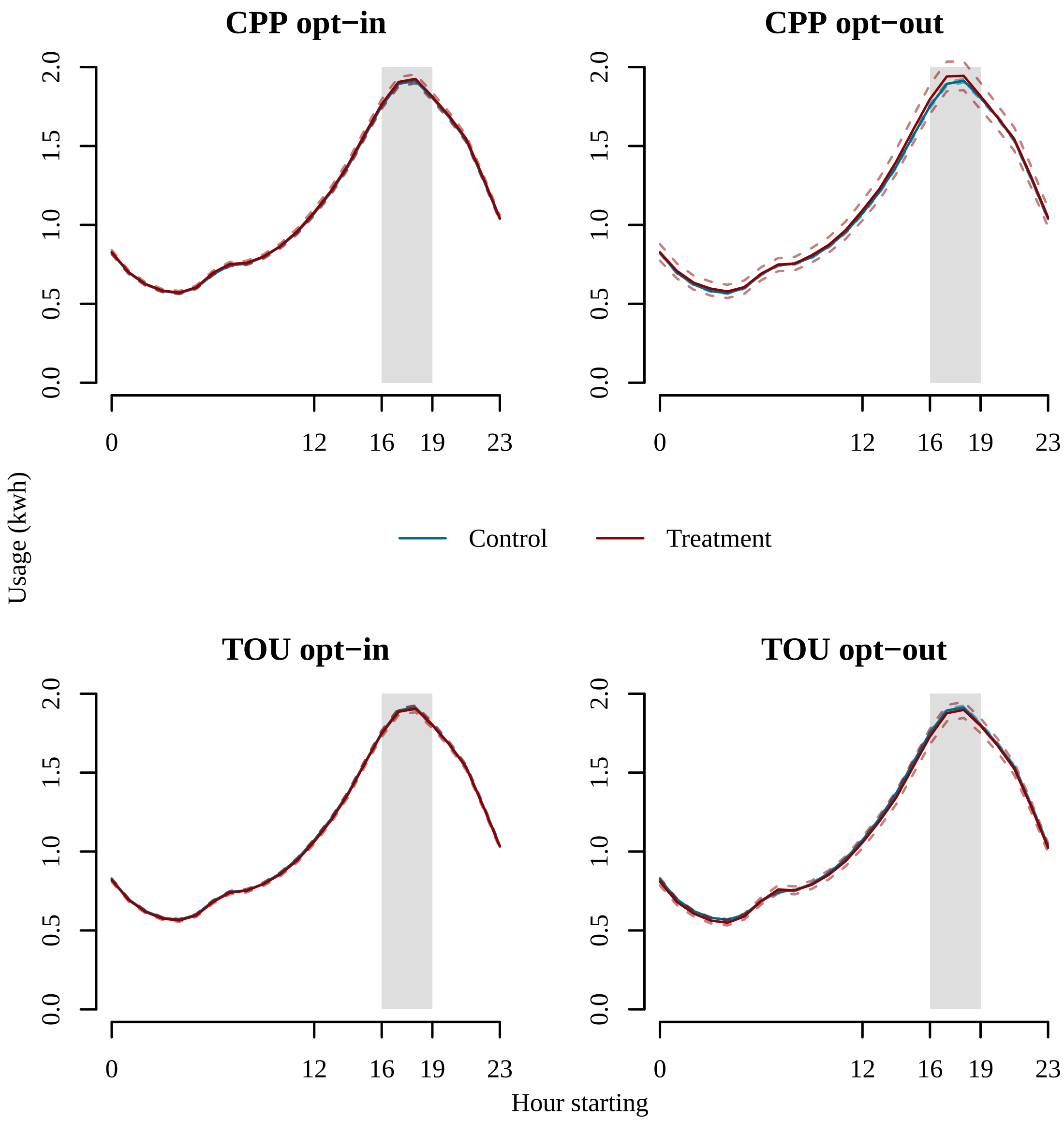
The values represented by the third box, "Average Reduction by Enrolled Customer by Hour and Month" are the LATE coefficients summarized in Table 4. Potter et al. (2014) estimated separate LATE effects for each hour of the program and provide suggestive evidence that customers reduce more when day are hotter. Hotter days also have higher "Capacity Risk Allocation" values, so this likely explains why the numbers in Potter et al. (2014) are slightly higher than ours.

The "Capacity Risk Allocation by Hour and Month" figures are based on proprietary values provided by SMUD. They are based on a simulation model which estimates the probability that demand exceeds supply on SMUD's system across any of the hours on representative weekend days and weekdays for each month of the year (called the "loss of load probability.") These values are then normalized to sum to one across all hours of the year. We use the sum of the normalized values in hours targeted by the CPP and TOU rates. Finally, following Potter et al. (2014), we assume a 7.1% nominal discount rate and a 4.5% real discount rate.

E.2. Costs

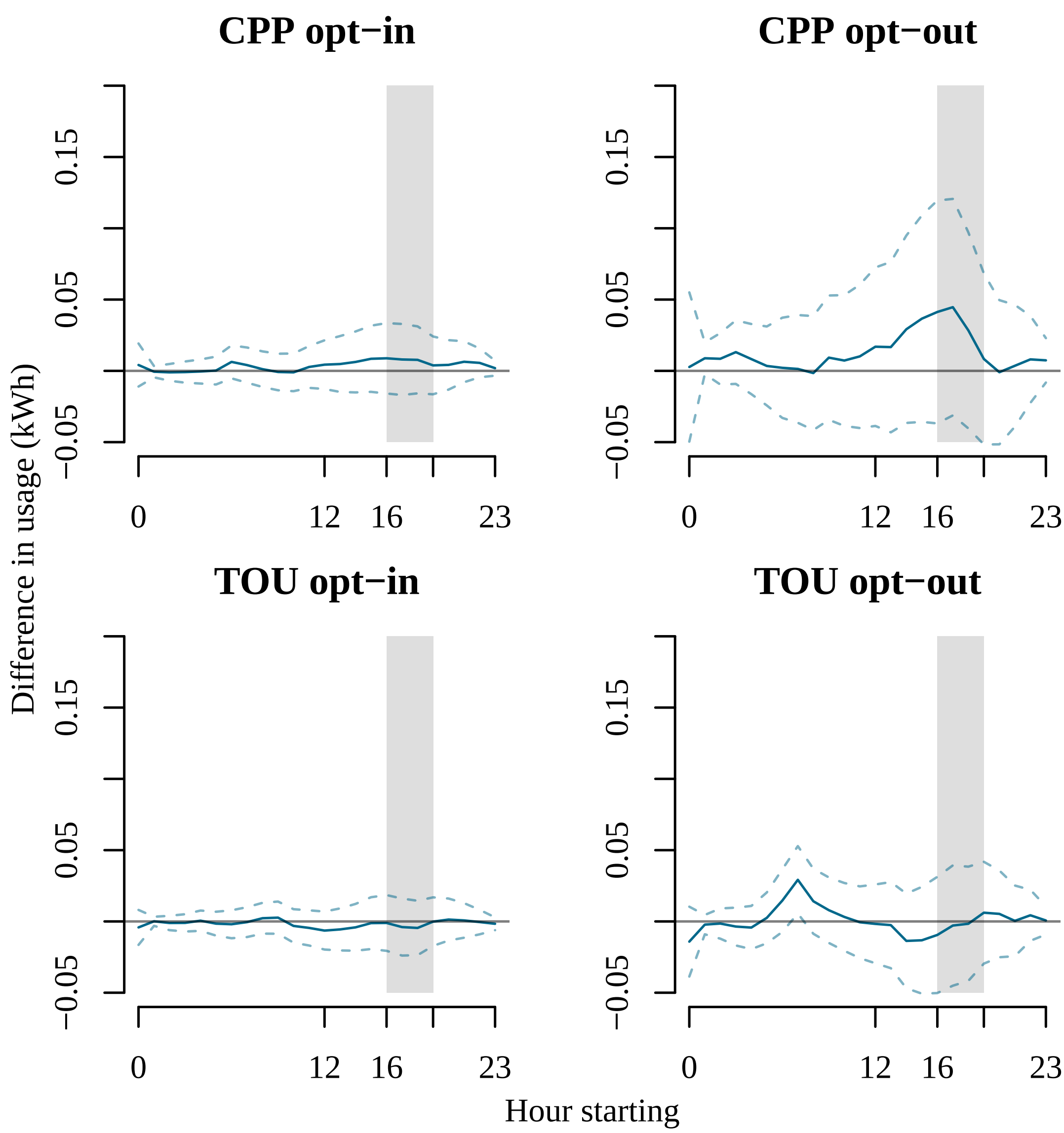
Table 6 summarizes one-time fixed costs, one-time variable costs and recurring fixed and variable costs. One-time fixed costs do not vary with enrollment and include items such as IT costs to adjust the billing system and initial market research costs. One-time variable costs primarily include the customer acquisition costs, including the in-home devices offered to customers as part of the recruitment. Note that Potter et al. (2014) model opt-in programs that do not include outbound calls to enroll customers, while we include the costs of the calls, as well as the customers recruited through them. Our objectives are different from theirs, as they were modeling a hypothetical program that SMUD might run in the future, while we are modeling the program that was actually run. Recurring annual fixed and variable costs include personnel costs required to administer the program and costs associate with customer support and equipment monitoring. They go down slightly over time with attrition from the program.

Figure A1: Pre-treatment electricity usage



Notes: Figure depicts average pre-treatment weekday electricity usage in kW. Panels plot average treatment group hourly electricity consumption overlaid with control group consumption, with coefficients and standard errors clustered by household obtained from a regression of electricity consumption on a set of indicator variables for each hour. Dashed lines indicate 95% confidence intervals.

Figure A2: Difference between treatment and control groups' electricity consumption prior to treatment



Notes: Figure depicts average difference in pre-treatment weekday electricity usage in kW between treatment and control groups. Lines represent regression coefficients from interactions between hourly indicator variables and a treatment indicator. Dashed lines indicate 95% confidence intervals, clustered by household. Vertical bars indicate peak period, between 4pm and 7pm.

Table A1: Intent to treat effects (logged outcome)

	Critical event		Non-event peak	
	Opt-in	Opt-out	Opt-in	Opt-out
Encouragement (CPP)	-0.083^{***}	-0.173^{***}	-0.021^{***}	-0.055^{***}
	(0.007)	(0.022)	(0.005)	(0.014)
Mean usage (kW)	2.5	2.5	1.8	1.8
Customers	55,024	46,680	55,028	46,684
Customer-hours	4,824,157	4,097,167	31,141,456	26,448,932
Encouragement (TOU)	-0.052^{***}	-0.073^{***}	-0.036^{***}	-0.059^{***}
	(0.005)	(0.012)	(0.004)	(0.010)
Mean usage (kW)	2.5	2.5	1.8	1.8
Customers	55,024	46,680	55,028	46,684
Customer-hours	4,824,157	4,097,167	31,141,456	26,448,932

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$, standard errors clustered by customer.

Notes: Replicates Table 3 with log(Usage) as outcome variable, coefficients are proportion change in consumption.

Table A2: Average treatment effects (logged outcome)

	Critical event hours			Non-event day peak hours		
	Opt-in (AT)	Opt-out (AT+C)	Complacents (C)	Opt-in (AT)	Opt-out (AT+C)	Complacents (C)
Treatment (CPP)	−0.424***	−0.187***	−0.124***	−0.106***	−0.059***	−0.046**
	(0.032)	(0.024)	(0.031)	(0.024)	(0.015)	(0.020)
Mean usage (kW)	2.5	2.5	2.44	1.8	1.8	1.79
Customers	55,024	46,680	10,036	55,028	46,684	10,036
Customer-hours	4,824,157	4,097,167	878,222	31,141,456	26,448,932	5,667,680
Treatment (TOU)	−0.275***	−0.077***	−0.028*	−0.190***	−0.062***	−0.030**
	(0.028)	(0.012)	(0.016)	(0.022)	(0.010)	(0.013)
Mean usage (kW)	2.49	2.5	2.44	1.79	1.79	1.75
Customers	58,569	48,241	15,142	58,573	48,245	15,142
Customer-hours	5,133,166	4,232,869	1,322,933	33,137,047	27,326,082	8,540,421

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$, standard errors clustered by customer.

Notes: Replicates Table 4 with log(Usage) as outcome variable, coefficients are proportion change in consumption.

Table A3: Heterogeneity: My Account, low income, year 2 (logged outcome)

	Critical event hours			Non-event day peak hours		
	Opt-in (AT)	Opt-out (AT+C)	Complacents (C)	Opt-in (AT)	Opt-out (AT+C)	Complacents (C)
My Account						
Treatment (CPP)	−0.386***	−0.139***	−0.089**	−0.124***	−0.053***	−0.039
	(0.052)	(0.028)	(0.035)	(0.039)	(0.019)	(0.024)
× My Account	−0.071	−0.117**	−0.090	0.032	−0.015	−0.019
	(0.065)	(0.049)	(0.067)	(0.049)	(0.031)	(0.042)
Treatment (TOU)	−0.200***	−0.050***	−0.021	−0.130***	−0.043***	−0.026*
	(0.044)	(0.015)	(0.019)	(0.034)	(0.012)	(0.015)
× My Account	−0.143**	−0.070***	−0.019	−0.113**	−0.047**	−0.011
	(0.056)	(0.026)	(0.036)	(0.044)	(0.022)	(0.029)
Low income						
Treatment (CPP)	−0.504***	−0.219***	−0.152***	−0.122***	−0.059***	−0.043**
	(0.040)	(0.028)	(0.035)	(0.030)	(0.017)	(0.022)
× Low income	0.275***	0.136***	0.129*	0.056	−0.003	−0.017
	(0.065)	(0.051)	(0.072)	(0.049)	(0.036)	(0.052)
Treatment (TOU)	−0.306***	−0.084***	−0.035*	−0.210***	−0.067***	−0.035**
	(0.035)	(0.014)	(0.018)	(0.027)	(0.012)	(0.015)
× Low income	0.102*	0.032	0.039	0.069	0.021	0.025
	(0.056)	(0.028)	(0.040)	(0.044)	(0.023)	(0.033)
Year 2						
Treatment (CPP)	−0.453***	−0.169***	−0.092***	−0.114***	−0.044***	−0.026
	(0.034)	(0.026)	(0.033)	(0.024)	(0.016)	(0.021)
× Year 2	0.065**	−0.041*	−0.071**	0.019	−0.034**	−0.049**
	(0.033)	(0.022)	(0.029)	(0.025)	(0.017)	(0.022)
Treatment (TOU)	−0.305***	−0.083***	−0.027	−0.206***	−0.065***	−0.030**
	(0.029)	(0.013)	(0.018)	(0.022)	(0.010)	(0.013)
× Year 2	0.066**	0.013	−0.001	0.040*	0.008	−0.001
	(0.031)	(0.013)	(0.017)	(0.024)	(0.010)	(0.013)

* p < 0.05, ** p < 0.01, *** p < 0.001, standard errors clustered by customer.

Notes: Replicates Table 8 with log(Usage) as outcome variable, coefficients are proportion change in consumption.

Table A4: Intent to treat effects (post-treatment period only)

	Critical event		Non-event peak	
	Opt-in	Opt-out	Opt-in	Opt-out
Encouragement (CPP)	−0.100***	−0.291***	−0.012	−0.073
	(0.023)	(0.061)	(0.017)	(0.047)
Mean usage (kW)	2.51	2.52	1.82	1.82
Customers	46,024	39,086	47,155	40,054
Customer-hours	2,855,231	2,426,418	18,751,449	15,935,568
Encouragement (TOU)	−0.089***	−0.193***	−0.061***	−0.143***
	(0.017)	(0.035)	(0.013)	(0.026)
Mean usage (kW)	2.51	2.52	1.82	1.82
Customers	46,024	39,086	47,155	40,054
Customer-hours	2,855,231	2,426,418	18,751,449	15,935,568

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$, standard errors clustered by customer.

Notes: Replicates Table 3 using only post-treatment period data. The estimating equation is identical, except that customer-specific fixed effects are no longer included due to the exclusion of pre-treatment period data.

Table A5: Heterogeneity: Structural winnership

	Critical event hours			Non-event day peak hours		
	Opt-in (AT)	Opt-out (AT+C)	Complacents (C)	Opt-in (AT)	Opt-out (AT+C)	Complacents (C)
Treatment (CPP)	−0.675***	−0.350***	−0.183***	−0.063	−0.058**	−0.036
	(0.071)	(0.054)	(0.057)	(0.042)	(0.027)	(0.028)
× Structural winner	0.036	0.039	−0.172	−0.172***	−0.086**	−0.153**
	(0.100)	(0.079)	(0.121)	(0.063)	(0.043)	(0.067)
Customers	55,027	46,683	10,034	55,027	46,683	10,034
Customer-hours	4,832,838	4,104,227	880,003	31,197,979	26,495,390	5,678,579
Treatment (TOU)	−0.414***	−0.100***	−0.022	−0.252***	−0.085***	−0.044**
	(0.050)	(0.023)	(0.030)	(0.033)	(0.016)	(0.021)
× Structural winner	−0.190*	−0.108**	−0.087	−0.099	−0.061*	−0.048
	(0.098)	(0.047)	(0.062)	(0.065)	(0.032)	(0.042)
Customers	58,569	48,245	15,141	58,569	48,245	15,141
Customer-hours	5,141,799	4,240,163	1,325,041	33,194,848	27,374,276	8,555,225

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$, standard errors clustered by customer.

Notes: Replicates Table 8 with indicator for structural winner as dimension of heterogeneity. Structural winner is a binary variable equal to one for customers whose pre-treatment period bills would have been lower under CPP/TOU price schedules than their bills under the flat rate, and zero otherwise.

Table A6: Exclusion restriction test

	Event hours		Non-event hours	
	Opt-in	Opt-out	Opt-in	Opt-out
CPP decliners	−0.0122	−0.0656	−0.00105	0.00348
	(0.00932)	(0.0965)	(0.00664)	(0.0588)
Mean kWh	2.45	2.44	1.76	1.76
Customer-hours	5,952,779	5,133,850	28,902,219	24,951,126
TOU decliners	−0.0242**	−0.0755	−0.0109	−0.0812
	(0.00823)	(0.0761)	(0.00582)	(0.0590)
Mean kWh	2.44	2.44	1.76	1.76
Customer-hours	6,283,694	5,141,176	30,504,109	24,987,786

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$, standard errors clustered by customer.

Notes: Table estimates effect of encouragement on usage of households who did not enroll in treatment. Table specification similar to Table 3, but sample includes control customers and encouraged customers who did not enroll in the treatment by not opting in or opting out, depending on whether they were in the opt-in or opt-out treatments, respectively. Bound of bias rows calculate the potential bias $\frac{(1-P)}{P}\beta$ (where P is the proportion enrollment for that group) in Table 4 as a result of the estimated encouragement effects on non-enrolling customers under the assumption that selection does not bias the given estimates.

Table A7: Recruit-and-delay LATE

Treatment (TOU)	-0.083^{***}	-0.111^{***}
	(0.028)	(0.028)
$\times$ 2013		0.065^{**}
		(0.031)
Mean usage (kW)	1.8	1.8
Customers	58,532	58,532
Customer-hours	33,188,035	33,188,035

* p < 0.05, ** p < 0.01, *** p < 0.001, standard errors clustered by customer.

Notes: Table estimates impact of treatment on usage for recruit-and-delay households (RITTD). Dependent variable is usage in kwh. Sampling frame is summer weekday peak hours from 2011-2013 and includes control group and TOU opt-in recruit-and-delay households. Regressions include household and hour of sample fixed effects, standard errors clustered by household.

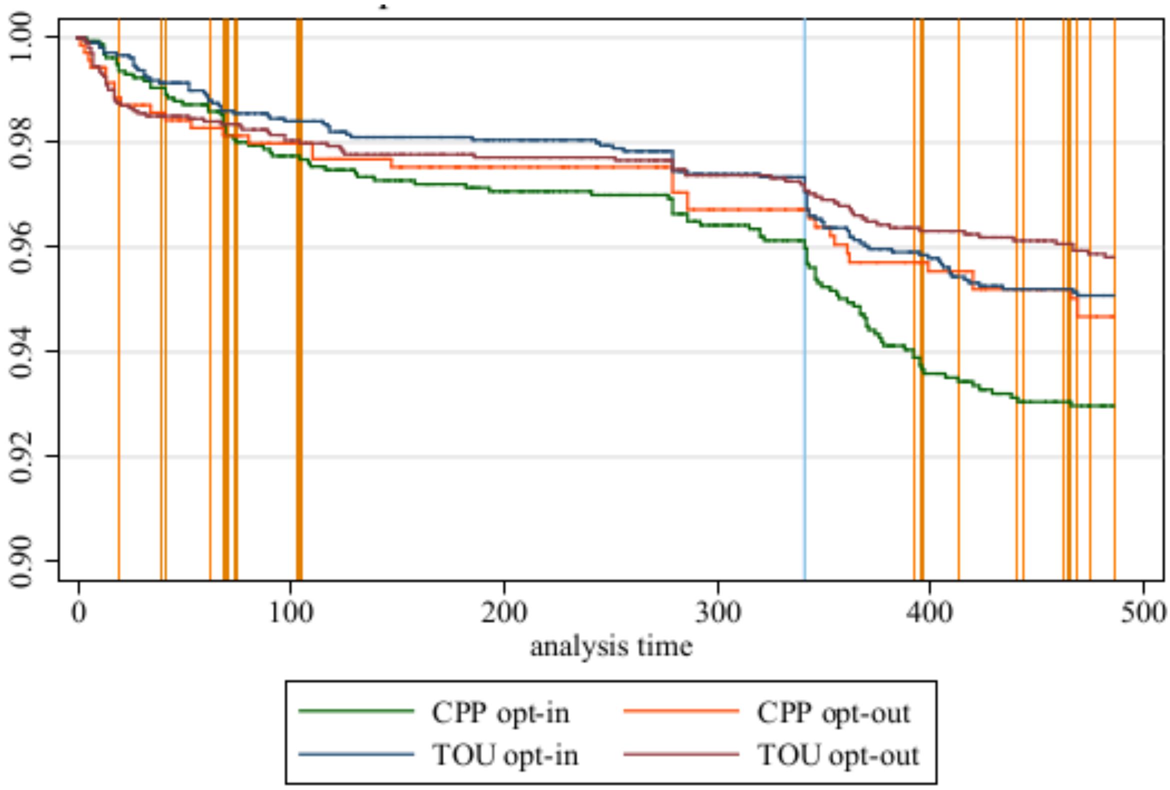
Table A8: April-May LATE impacts

Treatment (TOU)	-0.060	-0.039^{**}
	(0.039)	(0.016)
Mean usage (kW)	1.05	1.06
Customers	52,153	42,991
Customer-hours	6,748,730	5,564,183

* p < 0.05, ** p < 0.01, *** p < 0.001, standard errors clustered by customer.

Notes: Table estimates effect of treatment on pre-treatment period usage. Dependent variable is usage in kwh. Sampling frame is April and May weekday peak hours in 2012 and includes control group and the given treatment group. Regressions include hour of sample fixed effects, standard errors clustered by household.

Figure A3: Kaplan-Meier Survival Analysis



Notes: Kaplan-Meier survival estimates for each of the four treatment groups. Declining solid line is the proportion of households enrolled at the beginning of the treatment period who remain enrolled over time. Vertical orange lines indicate critical event days and the vertical blue line indicates the date on which the second summer reminder letter was sent out to all study participants letting them know that the rate would start again.

Table A9: Hazard Analysis - Accelerated Failure Time (AFT) Weibull Model

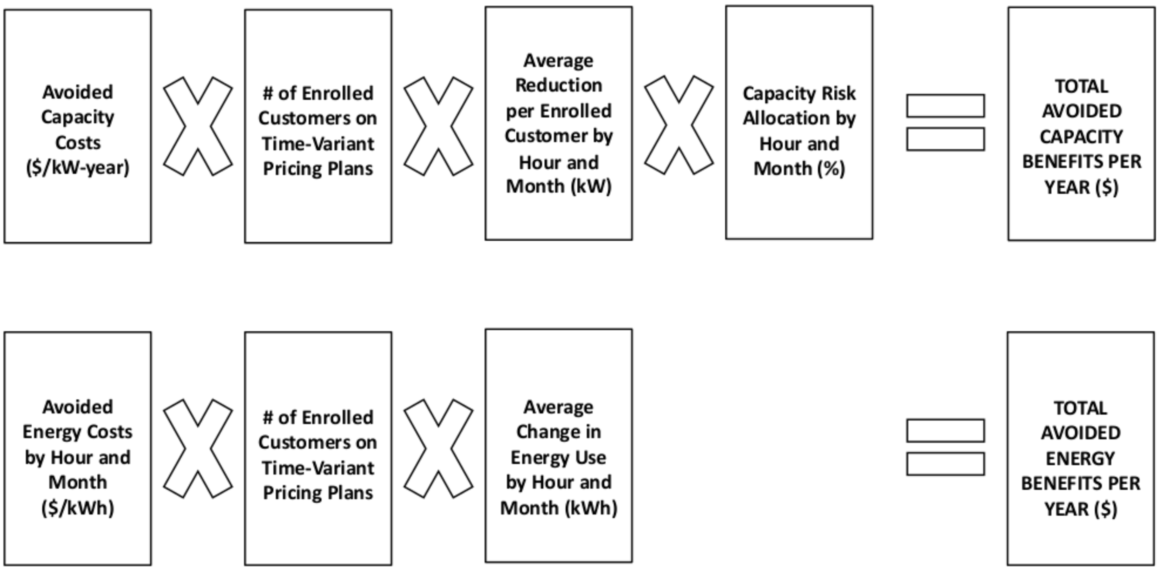
	Estimate	s.e.
Model Estimates		
TOU opt-in	0.340	(0.213)
TOU opt-out	0.561**	(0.226)
CPP opt-out	0.339	(0.301)
Low Income (EAPR)	0.514**	(0.202)
Structural winner	0.278	(0.172)
Your account	-0.0438	(0.162)
Second summer reminder date	-4.252***	(0.563)
CPP event date	-3.088***	(0.609)
CPP opt-out × CPP event date	2.406	(1.618)
CPP event date count in each summer	0.208**	(0.106)
CPP opt-out × CPP event date count in each summer	-0.352*	(0.210)
Final event in a string of consecutive event dates	-0.0937	(0.673)
Constant	9.869***	(0.292)
ln(p)	-0.328***	(0.0575)
Observations	2,690,168	

Drop out counts	Number of households	Number of drop outs
TOU opt-in	2110	92
TOU opt-out	2019	77
CPP opt-in	1585	101
CPP opt-out	701	35

* p < 0.05, ** p < 0.01, *** p < 0.001.

Notes: Top panel in table estimates predictors of time in treatment using an Accelerated Failure Time (AFT) specification, assuming a Weibull distribution parameterized by p. An estimate greater than zero indicates time in the program is extended (reduction in drop-out rate), while a number smaller than zero indicates that the time in the program is reduced (increase in drop-out rate). The omitted category is the CPP opt-in group. Bottom panel counts enrolled households and drop outs by treatment group.

Figure A4: Measuring Benefits of Time-Varying Pricing



Notes: Schematic of estimated net benefits of time-varying pricing programs used in Table 6. Source is Potter et. al. (2014), Figure 10-1.