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Towards Understanding the Role of Price in Residential Electricity Choices: Evidence from a Natural Experiment

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Are Residential Electricity Consumers Utility Maximizers? Evidence from a Natural Experiment*

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Abstract

We examine a choice setting in which residential electricity consumers may respond to incentives other than contemporaneous prices. We test predictions from the standard model of utility maximization using data from a natural field experiment that exposed some households to a change in their electricity rates. Households *reduce* electricity usage in response to a decrease in electricity prices, suggesting that factors aside from price influence customer choice. An understanding of household behavior in energy markets is essential for the effective implementation of climate change mitigation policy. Documenting this and similar results is a necessary step in achieving such an understanding.

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1 Introduction

A major policy goal in coming decades is to reduce greenhouse gas emissions. Most economists, including ourselves, favor market-based approaches due to their ability to achieve an emissions target cost-effectively. The strength of such instruments relies partly on the assumption that consumers will respond optimally to prices; but a growing body of evidence suggests that within the energy choice setting, consumer behavior does not strictly adhere to the predictions derived from standard models. Understanding when and under what conditions households respond to prices as standard theory would predict is crucial to achieve climate change mitigation objectives and efficiency in energy markets.

In this paper, we document an instance in which households did not respond to a retail electricity price intervention as standard theory would predict. Our empirical setting offers a unique opportunity to test whether consumers are static utility maximizers, and we find conclusively that in this instance they were not. While we are left to speculate about the exact reason for this deviation, our results suggest that there may be risk in adhering too ideologically to price interventions in terms of missing policy goals or achieving them only imperfectly or inefficiently. An assertion that price incentives always work can be disproven by the counter-example we provide.

This result should not be entirely surprising. The theory of "bounded rationality" has long predicted that it may be rational for consumers to be imperfectly informed or to not deploy full cognitive effort in the face of information acquisition or cognition costs (Simon (1955)), leading to outcomes that appear sub-optimal. People may also be motivated by intrinsic forces in addition to extrinsic (financial) incentives. This concept has been long-accepted by psychologists and sociologists, and has recently entered the economics domain in Benabou and Tirole (2003). In the residential electricity choice setting non-monetary incentives such as moral license or pressure to conform to social norms can dominate financial incentives. Voluntary enrollees in a carbon offset and green electricity programs increase their electricity consumption despite also facing higher prices (Harding and Rapson (2013) and Jacobsen, Kotchen, and Vandenbergh (2012)), and customers informed of their neighbors' electricity usage respond by using less themselves (Allcott (2011)). Altruism and green identity also play important roles, with environmental concerns becoming a relevant aspect of consumer decisions (as in Kotchen and Moore (2007)).

These results may help reconcile the variety of price elasticities of demand that have been reported for residential electricity customers (Alberini, Gans, and Velez-Lopez (2011), Fell, Li, and Paul (2012), Ito (forthcoming), Reiss and White (2005)). Consumers grapple with the complexity of the electricity choice setting. Features such as multi-tiered pricing structures (as explored by Reiss and White (2005)) or noisy signals about consumption may limit customers' ability to respond to prices. Consumers facing an increasing-block electricity rate structure appear to respond more to average price than marginal price (Ito (forthcoming)), and high frequency information about real-time consumption increases the price elasticity of electricity demand (Jessoe and Rapson (forthcoming)). Interventions that make prices (expenditure) more salient may meaningfully influence household electricity usage. Residential consumers have been shown to conserve electricity immediately after receiving their electricity bill (Gilbert and Graff-Zivin (2013)). Thus, price elasticity may be affected by several very specific aspects of the respective setting that are usually impossible to observe or control for in empirical analyses, potentially accounting for the heterogeneity of estimates found in the literature. It is for this reason that findings like those of Faruqui and Sergici (2010) – who provide a meta-analysis of a number of examples in which price interventions do indeed appear to have led to desired reductions – can be misleading: while they may indicate that prices often work, they cannot prove that prices always work; and they can provide only limited guidance as to the circumstances under which prices might work best.

We partnered with an electricity distribution company (EDC) in the northeast US to evaluate a large-scale mandatory residential time-of-use (TOU) program that forced households to switch irrevocably from a flat rate tariff to a TOU tariff after breaching a monthly usage threshold.[1] The setting gives rise to a regression discontinuity framework in which we compare outcomes of households just above the usage threshold to those of households falling just below the cutoff. Due to customers' inability to perfectly control monthly usage, in the neighborhood of the usage threshold assignment to the TOU rate is as good as random. The large-scale deployment of the program exhibits a high density around the threshold, creating a large sample of treatment and control households on which to test our hypothesis.

In the first summer months of the program in 2008, TOU rates were low relative to the flat rate alternative. Whereas the standard formulation of TOU prices is for the on-peak rate to be substantially higher than the flat rate and the off-peak rate substantially lower, in our setting TOU households faced on-peak rates in June to September of 2008 that were either lower than the relevant flat rate, or only slightly higher.[2] Off-peak rates were correspondingly even lower. The financial incentives for TOU households are clear: total electricity use in those months – regardless of substitution patterns across on-peak and off-peak hours – should increase.

We find the opposite. TOU customers *reduced* total electricity consumption, as measured by our estimates of the treatment effect at the threshold. These results are inconsistent with static utility maximization. It is clear that households are responding to incentives other than contemporaneous prices. There are ways to rationalize their choices. We discuss these

[1]TOU electricity pricing divides electricity use into two blocks according to the time of day at which electricity is consumed, and applies a higher rate to the block corresponding to historically high-cost times. It is a small step towards aligning retail electricity prices with marginal production costs. It is also the most common corrective measure used by electricity regulators to achieve such an alignment, due largely to the crucial advantage of being easy for consumers to understand and, in principle, respond to.

[2]Customers may purchase the generation component of their electricity services from either our EDC partner or an alternate supplier. This choice affects the relative on-peak and off-peak prices (the "TOU gradient"). In the discussion below we demonstrate why this does not affect our conclusions.

but do not perform formal tests, which our data cannot support. Nevertheless, the simple documentation of this result is an important step towards understanding energy demand behavior, and how future policy interventions might be improved.

The paper is organized as follows: in Section 2 we present a review of the standard theoretical framework for demand in this setting; Section 3 describes the program design, which forms the basis for our empirical setting; we explain how the setting can be viewed as a natural experiment and provide a description of our data in Section 4; treatment effects are reported in Section 5 and interpreted in Section 6; and Section 7 concludes.

2 Theoretical Framework

Several studies assessing early TOU experiments characterized consumer preferences using a framework that has since become the standard for modeling short-run household electricity demand by time of use. This framework, presented in detail in Aigner and Poirier (1979), was first used by Hausman, Kinnucan, and McFadden (1979) and Caves and Christensen (1980) to estimate the on-peak and off-peak price elasticities corresponding to TOU experiments in Connecticut and Wisconsin, respectively. We will rely on this framework as a baseline to define rational household responses to TOU pricing in a static optimization setting.

Following Hausman et al. (1979), we specify a household's monthly utility function as

$$U = U(x^{on}, x^{off}, \mathbf{y}), \tag{1}$$

where x^{on} and x^{off} are the household's monthly on-peak and off-peak electricity usage, respectively, and $\mathbf{y}$ is a vector of all other goods.[3] We then make a weak separability

[3] An important assumption underlying this utility specification is that the stock of electricity-using appliances is fixed. Therefore, x^{on} and x^{off} should be thought of as derived electricity demand based on demand for household services that use these appliances and the times of day that the household prefers to consume such services.

assumption so that utility can be characterized as

$$U = W(f(x^{on}, x^{off}), y), \tag{2}$$

where $f(x^{on}, x^{off})$ represents a homogeneous of degree one Hicksian aggregation of on-peak and off-peak electricity consumption, and y is a Hicksian composite of all the goods in $\mathbf{y}$. Normalizing the price of y to unity permits y to be interpreted simply as expenditure on all goods besides electricity.

The weak separability condition allows the household's monthly maximization problem to be decomposed into two levels. One level represents the household's choice of how much to spend on total electricity usage, where the remainder of its (fixed) income is spent on all other goods. The other level describes the household's choice of how to allocate electricity consumption across on-peak and off-peak hours, which depends only on electricity rates.

The choice of total electricity usage, $X \equiv x^{on} + x^{off}$, will depend on an aggregated price of electricity p given by

$$p = p^{on}s^{on} + p^{off}s^{off}, \tag{3}$$

where s^{on} and s^{off} are the shares of on-peak and off-peak usage in total usage as determined in the allocation level of the maximization problem.[4] These shares sum to unity and depend only on the parameters of $f()$. Therefore, the aggregated price of electricity is a weighted average of the on-peak rate p^{on} and the off-peak rate p^{off}.

Thus, conditional on the optimal choices of x^{on} and x^{off} from the allocation level of the maximization problem, total electricity consumption can be viewed as being determined by a straightforward utility-maximizing division of total income between good X, with price

[4]The aggregated price in our empirical setting will also include a small adjustment for a fixed monthly charge and for the increasing-block structure of the non-TOU rate. This will be discussed in more detail in the following section.

p, and expenditure on all other goods. Once properties of $W()$ are pinned down, deriving predictions concerning total electricity consumption is accomplished as in any two-good setting characterized by these properties. For example, if $W()$ is such that X is a normal good, the model clearly predicts that a drop in p will lead unambiguously to an increase in the quantity demanded of X.

We use this theoretical framework and its predictions on total electricity consumption as a backdrop when discussing the structure of electricity rates faced by the households in our dataset in the following section, and when discussing our empirical results in the interpretation section below. Of course, the model is also capable of generating predictions concerning load shifting, i.e. the substitution of electricity usage across on-peak and off-peak times. However, we do not discuss these predictions, as our dataset, which we will introduce in Section 4 below, does not provide us with the means to investigate them empirically.

3 Program Design

Beginning in 2006, an electric distribution company in northeastern United States implemented a mandatory time-of-use (TOU) program for residential customers. Prior to the introduction of this program, most residential customers were billed according to a seasonal flat rate, with the price of electricity varying seasonally but remaining constant within a day. Approximately 12% of customers chose to be placed instead on a seasonal TOU rate, with the price of electricity varying seasonally and within a day. In the analysis that follows, we exclude these voluntary adopters.

Under the policy, when a residential customer's electricity usage in any 30-day billing period exceeded a pre-determined threshold, the customer was automatically placed onto TOU pricing. Beginning November 2006, a household was to be placed on TOU pricing by January of 2008 if usage in any 30-day billing period exceeded 4000 kWh. This threshold applied until December 31, 2007. The threshold was lowered to 3000 kWh in 2008 and to 2000 kWh in 2009. The present study focuses on households that crossed the 4000 kWh threshold due to the unusual rate change that occurred at that time, to which we now turn.

The residential TOU rate plan charges a high per-kWh rate at on-peak times (noon through 8pm on weekdays) and a low per-kWh rate at off-peak times (all other times and days). Table 1 shows the TOU rates that were in effect over the period of our analysis, and compares them to the corresponding non-TOU rates. In our study, the summer non-TOU tariff had an increasing-block structure, with the first 500 kWh of usage in a billing month charged at a base "headblock" per-kWh rate and the remaining usage in that billing month charged at a higher "tailblock" per-kWh rate.

Given this increasing-block structure for the flat rate and the fact that all of the households in our analysis exceed 500 kWh in total electricity consumption in every month, the non-TOU monthly budget constraint can be expressed as

$$p^t(X - 500) + p^h 500 + g = E, \tag{4}$$

where E is total electricity expenditure, p^t is the tailblock rate, p^h is the headblock rate, and g is a fixed monthly charge. Noting once again that total electricity consumption is simply the sum of on-peak and off-peak consumption, this can be re-written as

$$p^t x^{on} + p^t x^{off} - (p^t - p^h)500 + g = E, \tag{5}$$

which emphasizes the fact that the marginal rate faced by non-TOU customers in *both* on-peak and off-peak hours is the tailblock rate. Meanwhile, the TOU monthly budget constraint is given by

$$p^{on} x^{on} + p^{off} x^{off} + g = E, \tag{6}$$

where the fixed monthly charge g is the same as that for non-TOU customers in all months.

Within the theoretical framework presented in the previous section, total electricity expenditure is defined as the product of the aggregated electricity price and total electricity consumption, or $E \equiv pX$. Inserting this definition into equations (5) and (6) and dividing by total consumption gives expressions for the aggregated non-TOU and TOU electricity prices,

$$p_N = p^t + \phi_N \tag{7}$$

and

$$p_T = p^{on}s^{on} + p^{off}s^{off} + \phi_T, \tag{8}$$

where the subscript $s \in \{N, T\}$ refers to non-TOU and TOU respectively, and the ϕ_s are small constants based on the fixed charge and the headblock adjustment.

We can now link the rates in Table 1 – and thus the change in the aggregated electricity price experienced by a household that was switched from the flat rate to the TOU rate – to predictions generated by the theoretical framework. Setting $\phi_T = \phi_N$ as a convenient approximation for now, it is clear that $p_T < p_N$ if $p^t > p^{on} > p^{off}$, which was the case with the unbundled rates in Table 1 throughout the summer of 2008.[5] Further, $p_T < p_N$ as well if $p^{on} > p^t >^{off}$ and s^{on} is sufficiently small. Therefore, as a first approximation, Table 1 indicates that households that were switched to TOU in 2008 experienced a decrease in the aggregated price of electricity that they faced compared to households that remained on the

[5]The unbundled rates include delivery and distribution charges only. They reflect the on-peak/off-peak gradient faced by all customers that chose to pay the generation rates of alternate suppliers, though the absolute level of the all-inclusive rates depends on the specific alternate supplier that a given household was served by, which we do not observe. The bundled rates are the all-inclusive rates that were faced by all customers that chose the EDC as their supplier, which includes about 45% of the EDC's overall customer base. All customers had the EDC in question as distributor, as there are no alternative distributors in the region.

flat rate. As discussed in the previous section, the standard theoretical framework would hence predict that households that were switched to TOU would increase their total electricity consumption. In the interpretation section below, we will demonstrate more formally that these households did indeed face a lower electricity price, but that their response – a decrease in total electricity consumption – cannot be reconciled with this or other predictions of the standard model.[6]

4 Experimental Setting and Data

In this section we explain in detail how the TOU program we study gives rise to a regression discontinuity design, and discuss some nuances of our empirical setting. We then describe the billing data used to identify the effect of mandatory TOU pricing on total usage and total bills.

The key feature of the regression discontinuity design in general is that assignment to the treatment group is triggered by crossing some threshold. In our setting, this occurs when monthly usage exceeds a pre-determined level. For estimated treatment effects to be valid, it must be the case that within the neighborhood of this threshold, assignment to TOU is effectively random. This will occur if some idiosyncratic factors push some individuals over the threshold but not others, or as described by Lee and Lemieux (2010), households lack precise control over the "forcing variable". We define the forcing variable in our context, according to the rules of the program design discussed above, to be maximum monthly electricity usage between November 2006 and December 2007 net of the 4000 kWh threshold. We will define and discuss this forcing variable more formally in the following section when

[6]The summer of 2008 is the only period in which households faced such a clear price reduction when being switched to TOU. By 2009, the EDC had a more standard TOU pricing scheme, with the on-peak and off-peak rates straddling the non-TOU flat rate. The change in the aggregate price of electricity for households switched to TOU by virtue of crossing either the 3000 kWh or 2000 kWh threshold in more recent years therefore cannot be determined as unequivocally as it can in the present case.

presenting our empirical specifications.

It seems reasonable to assume that households have only imprecise control over their exact electricity usage in any billing period, since precise control would likely require sophisticated equipment for monitoring and regulating usage. The validity of this assumption can be assessed more formally by examining the distribution of the forcing variable. If there were "bunching" in the density of this variable just below the crossing threshold, this might indicate that households could manipulate usage to avoid crossing the TOU threshold. Figure 1 demonstrates that there is no such bunching in our setting, and thus provides supporting evidence that crossing the threshold is random. We will therefore proceed to interpret differences in outcomes between individuals on either side of the threshold as causal effects.

However, one feature of the program – the varying lag across households between crossing a threshold and receiving the TOU treatment – complicates the regression discontinuity design, and in turn affects how the magnitude of these causal effects can be interpreted. To frame the issue, we divide the time period of our analysis into three sub-periods: the pre-experiment period; the qualification period; and the treatment period. The pre-experiment period is defined as the set of months preceding the introduction of the mandatory TOU rule. The qualification period is defined as November 2006 through December 2007, the months during which a household, should it exceed 4000 kWh, would eventually be assigned to TOU pricing. No household was actually assigned to TOU pricing until February 2008.[7] Thus, up until this month there is no difference between crossers and non-crossers in the propensity to be treated. However, not all qualifying households were switched at this point, and indeed, some were not switched for several more months.[8] Therefore, the propensity to be treated

[7]There were some households that had previously adopted TOU on a voluntary basis, but again, voluntary adopters have been excluded from the analysis. This was done because such self-selection into treatment would invalidate the experimental design.

[8]The long delays between crossing and switching and the failure to switch some qualifying households altogether mainly occur because of technical and administrative difficulties associated with installing requisite metering equipment. Households suffering from a serious illness or other life threatening situation necessitating the use of specialized electrical devices could apply for exemption from the program. We observe a

did not immediately jump to 100% in February 2008. The treatment period, the focus of our analysis, comprises June - September 2008, months when most households that crossed the threshold ("crossers") should have been switched onto TOU. We choose these months to be the treatment period because households on TOU faced an unambiguously lower per kWh rate (net generation) than households on the non-TOU rate during this period.

Another nuance in our setting is that customers would also qualify to be switched to TOU if they breached a lower (3000 kWh) threshold in any month in 2008. This implies that it is possible for some households who never crossed the 4000 kWh threshold to nonetheless be on TOU in the later months of 2008. The joint effect of these two features is that the propensity to be treated increases over time for *both* groups, and thus that the difference in this propensity across groups will be substantial for a limited window only. The fact that "control" households may become treated in greater numbers in the later months of 2008 is another reason that we terminate the treatment period after September 2008.

It follows that, unlike in a canonical "sharp" regression discontinuity setting, in our setting crossing the TOU usage threshold is not a perfect determinant of being in the treatment group in any given month. Instead, the empirical setting should be viewed as having been generated by the Fuzzy Regression Discontinuity (FRD) design, where the "fuzziness" refers to the imperfection of the crosser/non-crosser distinction as a predictor of TOU status in a given experimental month. While the FRD design allows us to interpret differences in outcomes between crossers and non-crossers as causal treatment effects, we must adjust their magnitudes for the propensity for each group to be treated. These treatment effects can be estimated consistently only for treatment months in which a sufficiently high proportion of crossers is on TOU relative to the proportion of non-crossers on TOU (i.e. in which the

small number of crossers that were switched to TOU but eventually allowed to revert to a non-TOU, and interpret this to be the result of the granting of a medical exemption. These households have been removed from the analysis. It is possible that some of the crossers that were never switched to TOU were granted a medical exemption pre-emptively, but we cannot observe this.

crosser/non-crosser distinction is a strong instrument for treatment status).

Before turning to a more precise discussion of how we implement the estimation of these treatment effects, we describe the billing data and present summary statistics. Monthly billing data beginning in June 2006 on total usage, total expenditure (net of generation) and rate class were provided for a sample of about 35,000 households.[9] Table 2 presents descriptive results at the preferred bandwidth of 600 kWh.[10] The experiment consists of 1,096 households, 34% of which crossed the 4000 kWh threshold at some point in the qualification period. Mean usage and net-of-generation expenditure for this sample amount to 3,309 kWh and $382 in July 2007, though within this bandwidth there is substantial variation in both usage and expenditure.[11]

5 Treatment Effects for Total Usage and Total Bills

5.1 Methods

We begin by comparing crossers to non-crossers along several dimensions, separately for each month in the entire sample. Specifically, we estimate

$$Y_i = \beta_0^{Yt} + \beta_1^{Yt} C_i + \beta_2^{Yt} f\left(\tilde{X}_i\right) + \beta_3^{Yt} C_i \times f\left(\tilde{X}_i\right) + \varepsilon_i^{Yt} \tag{9}$$

individually for each month (t) and for various dependent variables Y. The variable C_i is a

[9] The dataset is comprised of the population of households with usage above 1500 kWh in September 2010. The year 2010 was chosen so that the included households would be most representative of the EDC's current customer base. September was chosen because it corresponded to the annual system peak that year.

[10] Bandwidth will be discussed in the following section and in Section A.1.

[11] Note that Table 2 does not report descriptive statistics on peak and off-peak usage because we do not have data on these variables. As we are relying on billing data, and since utilities have no need to meter usage by time of day if they do not charge time-varying rates, our dataset does not include information on the on-peak/off-peak breakdown of total usage for non-TOU household-months. As discussed in the interpretation section below, this breakdown can be inferred from billing data and rates for TOU household-months; but without information for non-TOU household-months, we are unable to assess the effect of the switch to TOU on this breakdown.

dummy variable indicating whether household i is a crosser. The variable $\tilde{X}_i$ is the "forcing variable" that determines whether household i is a crosser. More precisely, $\tilde{X}_i$ is household i's maximum total usage across all billing periods during the qualification period net of the kWh threshold. Under the rules of the program, if $\tilde{X}_i$ is strictly greater than zero, household i is a crosser and should receive the TOU treatment eventually.[12]

The dependent variables we consider are total usage, total bills, and a dummy variable TOU_{it} indicating whether household i was on TOU pricing (i.e. was treated) in month t. Specification (9) allows for a flexible relation between the outcome variable of interest and the forcing variable through the function $f(\cdot)$, and allows this relation to differ for crossers and non-crossers.[13] The parameter β_1^{Yt} measures the effect of being a crosser on the level of outcome variable Y in month t as the distance from the threshold approaches zero, and is interpreted as the Intent to Treat effect (ITT). These are causal effects by virtue of our assumption – discussed and supported in the previous section – that, as the distance from the threshold approaches zero, a household's crossing status is exogenous.

The fuzzy regression discontinuity treatment effect for outcome Y in any month t in the treatment period for a given experiment is defined as

$$\tau_{FRD}^{Yt} \equiv \frac{\beta_1^{Yt}}{\beta_1^{TOUt}}. \tag{10}$$

That is, the treatment effect for the outcome of interest is the ratio of the ITT for the

[12]Formally, let X_{it} be household i's total electricity usage in month t. Further, let usage on a standardized 30-day-billing-period basis be $\ddot{X}_{it} \equiv X_{it}/d_{it} \times 30$, where d_{it} is the number of total days actually in the billing period corresponding to household i's bill in month t. Then $\tilde{X}_i \equiv \left(\max_{t \in \mathbb{Q}}\{\ddot{X}_{it}\} - \bar{X}\right)$, where $\mathbb{Q}$ is the set of months in the qualification period and $\bar{X}$ is the threshold; and $C_i \equiv \mathbb{1}\left\{\tilde{X}_i > 0\right\}$, where $\mathbb{1}\{\}$ is the indicator function. The households included in these regressions are only those with a value of the forcing variable $\tilde{X}_i$ within a selected bandwidth around zero, i.e. households "close to" the threshold. When presenting our results, we first use a wide bandwidth to visually examine the data and then use an optimal bandwidth for each experiment to estimate the treatment effects.

[13]We first define $f(\cdot)$ as a fourth-order polynomial to visually examine the data, then as linear to estimate the treatment effects. Within the optimal bandwidth, we do not find alternatives to the linear form to qualitatively affect our estimated treatment effects.

outcome of interest to the ITT for the propensity to be treated.[14] It can be estimated by applying two-stage least squares to the following system of equations for any outcome variable Y in a given treatment-period month t:

$$Y_i = \tau_0^{Yt} + \tau_1^{Yt} TOU_i + \tau_2^{Yt} f\left(\tilde{X}_i\right) + \tau_3^{Yt} C_i \times f\left(\tilde{X}_i\right) + \omega_i^{Yt} \tag{11}$$

$$TOU_i = \beta_0^{TOUt} + \beta_1^{TOUt} C_i + \beta_2^{TOUt} f\left(\tilde{X}_i\right) + \beta_3^{TOUt} C_i \times f\left(\tilde{X}_i\right) + \varepsilon_i^{TOUt}, \tag{12}$$

where $\hat{\tau}_{1,2SLS}$ is numerically equivalent to inserting the ITTs estimated via specification (9) into equation (10). Note that we apply two-stage least squares as a computational convenience, not to address endogeneity concerns.[15]

5.2 Preliminary Evidence

We begin by visually examining the propensity to be treated, total billed amount, and total usage on each side of the threshold in July 2008. Specifically, we estimate specification (9), including households within a very wide range around the threshold and allowing the relation between the outcome variable and the forcing variable to have a separate quartic form on each side of the threshold. This provides a first look at whether the relation exhibits a discontinuity at the threshold (i.e. an intent to treat effect), and allows us to diagnose any non-linearities that may complicate the identification of a discontinuity.

Figure 2 shows the estimated propensity to receive the TOU treatment for crossers (house-

[14] See Lee and Lemieux (2010), p. 300 for a discussion of how the FRD treatment effect thus defined is equivalent, under the standard local average treatment effect assumptions, to the average treatment effect for compliers in a potential outcomes framework.

[15] A household's time-of-use status in a given treatment-period month depends on its crossing status in the preceding qualification period and on unobservable factors. However, crossing status is exogenous at the threshold by assumption, and the unobservable factors are ostensibly exogenous issues related to various meter installation and administrative hurdles faced by the utility. We therefore do not consider concerns about endogeneity between TOU status and either total expenditure or total usage to be present.

holds that exceeded the 4000 kWh threshold) and non-crossers. Crossing the threshold is clearly a strong predictor of having received the TOU treatment by July 2008, as illustrated by the dramatic discontinuity at the threshold. However, it is not a perfect indicator, as some non-crossers just to the left of the threshold – i.e. whose maximum 30-day usage during the 4000kWh qualification period was very close but did not exceed the 4000kWh threshold – have a small but positive propensity to be treated. Likewise, a few crossers still had not received the TOU treatment by July 2008.

In Figures 3 and 4, we present the estimated total billed amount and usage, respectively, on each side of the 4000 kWh threshold in July 2008. These graphs illustrate a discontinuity both in expenditure and usage at the threshold, suggesting that a crosser had a substantially lower electricity bill than a non-crosser at the threshold (by about $100). While the relation in Figure 3 exhibits some non-linearity, particularly for very high levels of the forcing variable, these figures provide fairly clear evidence that the difference in usage and expenditure is indeed the result of a discontinuity.

5.3 Treatment Effects

Having provided visual evidence of the discontinuity, we now restrict specification (9) to be linear in the forcing variable and in its interaction with crossing status, and include only households within a narrower, optimally-chosen bandwidth of 600 kWh.[16] We use this form to identify ITTs for each dependent variable for several pre-qualification and qualification months, as well as our treatment months of June - September 2008. To present the results as compactly as possible, we graph time series of the set of estimated coefficients for each

[16]The method used to determine the optimal bandwidth is described in Section A.1. A larger bandwidth leads to more precise estimates of the discontinuity. However, it also means that households further away from the threshold are being used to identify the discontinuity *at* the threshold, which can impart a bias; and this bias can be large if the relation is highly non-linear around the threshold. We choose an optimal bandwidth for a given experiment to apply uniformly for the estimation of all ITTs and treatment effects in each month of the treatment period.

of the three dependent variables. For dependent variable Y, we graph $\hat{\beta}_0^{Yt}$ – the estimate of outcome Y in month t for a non-crosser exactly at the threshold – and $\hat{\beta}_0^{Yt} + \hat{\beta}_1^{Yt}$ – the same for a crosser exactly at the threshold – for every month, also indicating when the difference between the two is statistically significant.

Figure 5 graphs the ITT of the probability that a crosser receives the TOU treatment for each individual month between June 2006 and January 2009.[17] The months between the vertical lines delineate the qualification period, and the months further to the left are the pre-experiment period. This figure illustrates that crossing the TOU threshold is a strong predictor of TOU pricing in the treatment period. In the pre-experiment and qualification periods the propensity to be on time-of-use pricing is zero for both crossers and non-crossers by construction, since we restrict our sample to households that did not receive the treatment during the qualification period.[18] However, by October 2008, the proportion of control households that had been switched to TOU by virtue of crossing the 3000 kWh threshold earlier that year was so high that treatment effects cannot be consistently estimated for this month onwards.

We present the estimated ITTs on the total bill in Figure 6. The large discontinuity illustrated in Figure 3 for July 2008 is also present for the other treatment months, with 95 percent confidence. We also observe that the estimated total bill was nearly identical for crossers and non-crossers throughout the pre-experiment and qualification periods. This balance on pre-determined observables is consistent with the intent to treat being randomly

[17] The bandwidth is symmetric, so encompasses households with a value of the forcing variable between -600 kWh and 600 kWh. Note that the data in Figure 2 have been smoothed for ease of presentation, so that each point represents several households. The point for July 2008 in Figure 5 is based on straight lines of best fit through the first 7-8 points on each side of the threshold in Figure 2.

[18] Households with a value of the forcing variable substantially higher than the upper bandwidth cut-off of 600 kWh are more likely to have crossed the 4000 kWh threshold for the first time early in 2007, and such households were required to have been switched to TOU before the end of 2007. A few of these households were indeed switched in late 2007, but most were not switched until February 2008. The delay in rolling out the program for these larger households (that are not included within the bandwidth we consider in any case) appears to be due to unforeseen technical and administrative issues by the utility.

assigned at the threshold. It also suggests that the large ITTs observed in the summer 2008 are not spuriously caused by systematic difference in summer usage patterns between crossers and non-crossers.

Figure 7 illustrates the estimated ITTs on total electricity usage over time. Total usage was nearly identical between crossers and non-crossers throughout the pre-experiment and qualification periods, providing evidence of another observable along which the two groups are balanced. However, during the treatment periods, there is a significant difference in total usage in June and July 2008, when crossers at the threshold exhibited lower usage than non-crossers at the threshold. We also see some visual evidence of lower usage for TOU households in August and September, though we cannot distinguish these from zero with confidence. The absence of significant differences in total usage between crossers and non-crossers during the pre-experiment and qualification periods indicates that the differences in June and July 2008 are not driven by pre-existing differences between the groups. It also indicates that non-crossers were not purposely restraining their usage during the qualification period to avoid crossing the threshold, which would violate the random assignment assumption.

Table 3 shows the treatment effects, adjusted for the propensity to be treated, on total usage and total bills for each month in the treatment period. To give a better sense of magnitudes, treatment effects are reported as a percentage of the level of the respective variable for non-TOU households at the threshold.[19] We find that the switch to TOU pricing caused economically and statistically significant reductions in electricity expenditure in all treatment months by at least 21% and as much as 30% in July. This is matched by statistically significant declines in total electricity usage in June and July of 9-10%, and noisy declines of 5 and 2 percent in August and September, respectively.

When interpreting the expenditure estimates, it seems natural that electricity expen-

[19] That is, each entry shows $\hat{\tau}_1^{Yt}/\hat{\tau}_0^{Yt} \times 100$ from a separate two-stage least squares application of equations (11)-(12). We discuss the bootstrap methods we employ to estimate the standard errors of these transformed coefficient estimates in Section A.2

diture would decline or remain unchanged since customers on TOU faced lower peak and off-peak rates compared to flat rate households. In contrast, basic intuition suggests that demand for electricity should increase with a reduction in electricity prices; yet we find the opposite to be true. We now investigate the revealed choice behavior more directly.

6 Interpretation

To assist us in digesting the empirical results, we turn to Figures 8 and 9, which provide a simple visual way to evaluate the nature of consumer choice. These figures present graphs of budget frontiers and revealed choices as implied by the empirical results described in Table 3. Each graph presents the consumption bundle chosen by TOU customers, as well as two budget frontiers. These features of the choice setting are derived directly from prices and estimates of behavior in treatment (TOU) and control (non-TOU) *at the threshold*. The TOU consumption bundle is revealed arithmetically from the relationship between total consumption ($\hat{\tau}_0^{kWh_t} + \hat{\tau}_1^{kWh_t}$) and the TOU tariff rates. Budget frontiers are determined by the revealed consumption level at the threshold (from the application of the 2SLS estimation) and relative prices.[20]

The first frontier is based on non-TOU rates and the level of expenditure of the non-TOU household, and has a slope of -1 to reflect equality of peak and off-peak prices. This frontier represents all combinations of on-peak and off-peak usage that sum to the estimated non-TOU total usage at the threshold. Note that any point on the interior of this frontier is unequivocally a drop in total consumption relative to the non-TOU bundle. The second, analogous, frontier is based on TOU rates and the expenditure of the non-TOU household at the threshold. Were expenditure for treated households to remain at the revealed non-TOU

[20] Algebraically, the budget frontiers are expressed by equations (5) and (6), with the values of actual rates and revealed total expenditure inserted where appropriate. The imputation of the TOU consumption bundle is discussed in Section A.3. A technical issue involving an adjustment of calendar-month rates for billing cycles that is necessary for implementing this imputation is discussed in Section A.4.

level, their TOU bundle would reside on this second frontier. Each budget constraint is presented for the months June to September 2008.

We present two different rate types – unbundled in Figure 8 and bundled in Figure 9 – to reflect differences in the TOU gradient between two customer types in our sample. Unbundled rates are paid by customers who have elected to purchase electricity generation from an "alternate supplier" (i.e. not from the regulated electricity distribution company). During the period of analysis, all alternate supplier generation rates were time-invariant,[21] implying that the entire TOU gradient was transmitted through the unbundled price for these customers. On the other hand, bundled rates include generation charges that are paid to the electricity distribution company. In our setting, these generation charges transmit an additional peak/off-peak price gradient. As such, the choice setting is different for customers who have elected to purchase generation from an alternate supplier than for those purchasing exclusively from the regulated utility, so we present budget frontiers separately for each.

Recall that the TOU rates/non-TOU expenditure frontier represents the theoretical consumption possibilities available to a household that is switched to TOU pricing and retains the non-TOU level of electricity expenditure. This frontier describes the set of on-peak/off-peak bundles from which a static utility maximizing household would choose if on-peak and off-peak electricity were the only two goods consumed. The non-TOU bundle lies somewhere on the interior line, and from these figures it is apparent that the chosen TOU bundle was feasible under the non-TOU budget. Thus the original non-TOU bundle is revealed preferred to the TOU bundle. Note that this is true irrespective of the presence of crossing of the budget constraints (which we discuss below). In this simplified two-good setting, each graph reveals an outcome in which treated consumer choices appear to violate the Weak Axiom of Revealed Preference (WARP).

[21] A thorough search of alternate supplier rates by the authors in 2010 confirmed what our utility partners asserted: time-varying generation rates were not offered by alternate suppliers until more recently.

A more realistic interpretation of the setting includes an outside consumption good in addition to both electricity goods, as in the theoretical framework presented in the second section. Here we will distinguish between instances in which the TOU frontier lies completely above the non-TOU frontier and those in which the TOU and non-TOU budget constraints cross. When we allow for the presence of an outside good, the possibility exists that an electricity price change will induce substitution towards the outside good. In regions where the TOU budget is on the exterior of the non-TOU budget constraint, the outside good has become relatively more expensive in treatment. In these cases, a net reduction in electricity consumption and increase in consumption of the outside alternative would imply that the entire bundle of non-electricity household expenditures was a Giffen good. We consider this to be an unsatisfactory explanation with which to reconcile the empirical findings.

In regions where the TOU budget is interior to the non-TOU budget (i.e. in the lower-right region of graphs where the frontiers cross), the story becomes slightly more nuanced. For households with non-TOU bundles in that region, the switch from flat rate to TOU implies a decrease in the relative price of the outside good. In this case, a net decrease in electricity consumption does not require the aggregate good to be a Giffen good. However, the likelihood of a household's chosen peak/off-peak bundle initiating on this region of the non-TOU budget frontier is essentially zero. In months where we observe a cross in the budget frontier, this crossing occurs at an extremely high peak/off-peak ratio. Appealing to an external dataset on the peak-to-off-peak usage ratio for a random sample of customers, we can examine the likelihood that the observed "crossing" ratio falls within the observed range of ratios.[22] With the exception of bundled rates for June, the crossing ratio is much higher than any peak-to-off-peak ratio observed in the data. Even for a customer on a bundled rate in June, the "crossing" ratio is in the 99th percentile of the observed distribution. We hence

[22]This load profile dataset comprises hourly usage data between January 2006 and October 2011 for a random sample of households present for between 2 and 48 months. Figure 12 shows the peak-to-off-peak usage ratio by total usage from this dataset.

consider extreme preferences for on-peak electricity usage to be an unlikely explanation for our results.

The abundance of evidence does not allow much scope for household choice behavior to be consistent with static utility maximization. A two-good view leads immediately to violations of WARP. When we allow for the consumption of an outside good, for static utility maximization to hold, it must be that the composite bundle of non-electricity expenditures is Giffen, or that the initial non-TOU bundle lay on a region of the budget frontier that is inconsistent with observed data.

So where does this leave us? One might conjure several reasonable explanations to rationalize the observed behavior. While we are not able and thus do not strive to test them here, we consider this an important area of future research. We hope that a description of some of these hypotheses will be helpful to readers.

One hypothesis allows for a dynamic consumer – one that is in a sense more sophisticated than the static utility optimizer for which we test earlier. In our setting, the peak price did eventually increase. A consumer correctly expecting this increase in future rates ought to incorporate such expectations into the choice of durable goods investments.[23] That is, if electricity is expected to become more expensive during peak hours of air conditioning demand, a rational, forward-looking consumer will be willing to pay for a more energy-efficient air conditioner *today*. Making such a choice would manifest in lower derived electricity demand for electricity today, which is consistent with behavior in our setting.

Another potential hypothesis that could explain the observed outcome is that households were not only responding to contemporaneous rates, but also engaging in what is becoming known as "intermittent updating". Under this hypothesis, consumers are attentive to choices infrequently, and thus may exhibit behavior that resulted from optimization at some previous time, but which does not correspond to utility maximization in each moment.[24]

[23] See Rapson (2013).

[24] While we remain agnostic about mechanisms, support for the "intermittent updating" hypothesis resides

Intermittent updating may equivalently be thought of as a symptom of "rational inattention", whereby consumers educate themselves about their energy consumption in response to (new) incentives provided by the TOU rate structure.

Finally, there is a growing body of evidence on the importance of "behavioral" considerations in this choice setting. Each treated household received a letter notifying them of their new rate plan, and it is possible that receipt of the letter itself was responsible for the observed treatment effect. There is some evidence from the literature that is consistent with this explanation. For example, households that are informed that their usage is abnormally high tend to engage in behavior that brings them closer to the norm (Allcott (2011)). Since treated households in our setting have been told that they had "high" usage, this information may have induced a response towards conforming to social norms, or perhaps towards attempting to atone for or counteract this high usage.

7 Conclusion

This study exploits a natural experiment to document a setting in which households do not respond to price incentives in the way that standard theory predicts. Despite growing evidence that non-monetary and inter-temporal factors are important in a wide range of environments, there are few well-identified cases of this behavior in environmental economics. Evaluating customer response to price incentives is a necessary step in understanding consumer choice in this setting.

The randomized nature of assignment into TOU pricing that arises from the structure and implementation of the program provides us with an empirical setting to evaluate consumer behavior. Customers were automatically placed on the TOU rate after exceeding the usage threshold, creating an appropriate setting in which to apply a regression discontinuity

in the fact that hundreds of households just below the threshold could have saved substantial amounts by volunteering for TOU, but didn't.

design. This differentiates our research design from most studies of time-varying electricity pricing which rely on framed field experiments in which participants are aware of their participation.[25] Thus, our paper offers a novel estimate of how certain residential consumers behave when exposed to TOU pricing.

The baseline model of consumer behavior employed in this paper is rooted in the traditional framework for modeling consumer electricity choice (Aigner and Poirier (1979), Hausman, Kinnucan, and McFadden (1979) and Caves and Christensen (1980)). And while this framework should serve as a starting point to frame consumer response to electricity prices, we highlight that in some instances it may not describe customer behavior accurately. In these cases, structural estimates based on the classic theoretical framework may lead to misleading conclusions.

Admittedly, the households in our setting are very large, and not representative of the "average" electricity user. On the other hand, the intensity of electricity use which they exhibit makes them a particularly important target for energy conservation efforts. Questions of external validity, though, are almost beside the point since what we observe in this setting is potentially present in many other settings. An assertion that price incentives always work can be disproven by the counter-example we have provided.

We must achieve a more thorough understanding of what drives behavior to inform planners about how to effectively achieve climate change mitigation. If what we observe here suggests that failure of the utility maximization assumption could arise more generally in some settings, then market-based policies designed on the basis of this assumption may fail to achieve the emissions target and/or fail to achieve a given reduction cost effectively. On the other hand, if we knew the mechanism driving consumer behavior, this information could be leveraged to effectively introduce price-based policies (for example, by coupling

[25] We refer here to the taxonomy of field experiments proposed by Harrison and List (2004). Wolak (2006) and Jessoe and Rapson (forthcoming) are examples of recent studies of the effect of time-varying pricing that are based on framed field experiments.

prices with real-time feedback) or non-price interventions. Our findings suggest that there may be a risk in adhering too ideologically to price interventions alone, in terms of missing policy goals or achieving them only imperfectly or inefficiently.

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Tables and Figures

Table 1: Electricity Rates, 2008, Cents per kWh

	Non-TOU		TOU	
	Headblock	Tailblock	On-Peak	Off-Peak
unbundled				
Jun.	7.9	11.8	11.4	7.6
Jul.	8.6	12.6	12.0	8.1
bundled				
Jun.	20.1	24.1	26.2	18.9
Jul.	20.6	24.6	26.5	19.2

Notes: Unbundled rates include distribution, transmission, and delivery charges plus fees only. Bundled rates also include the generation prices that were charged by the utility to those customers opting to keep the utility as both distributor and supplier. About 55% of the customer base opted to pay generation prices charged by alternate suppliers; no alternate suppliers had TOU generation prices, so the unbundled rates represent the relative on-peak/off-peak all-inclusive rates faced by these customers, but not the absolute level. The headblock is the first 500kWh of total usage in the billing month. The July rates stayed in place through September.

Table 2: Summary Statistics, July of the Qualification Period

	Total Usage (kWh)	Total Bill ($)	Crossers (%)	N
4000kWh Experiment (2007)	3,309	382	0.339	1,096
	[751]	[91]	[0.473]	

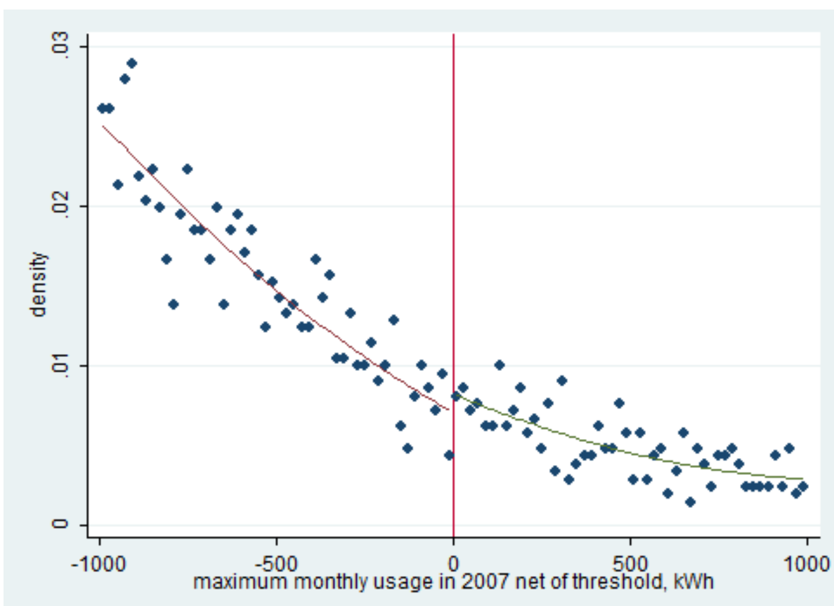
Notes: Standard deviations are in square brackets. The households included for each experiment are those within an optimally-chosen bandwidth around the threshold; see the text for details.

Table 3: Treatment Effects (%), 4000kWh Experiment, 600kWh Bandwidth

	Total Usage		**Total Bill**		*N*
Jun. 2008	-9.24	**	-21.50	***	1,105
	(4.69)		(4.19)		
Jul. 2008	-9.85	***	-30.06	***	1,105
	(3.73)		(2.96)		
Aug. 2008	-5.39		-26.15	***	1,107
	(4.04)		(3.08)		
Sep. 2008	-2.11		-22.31	***	1,095
	(5.82)		(4.36)		

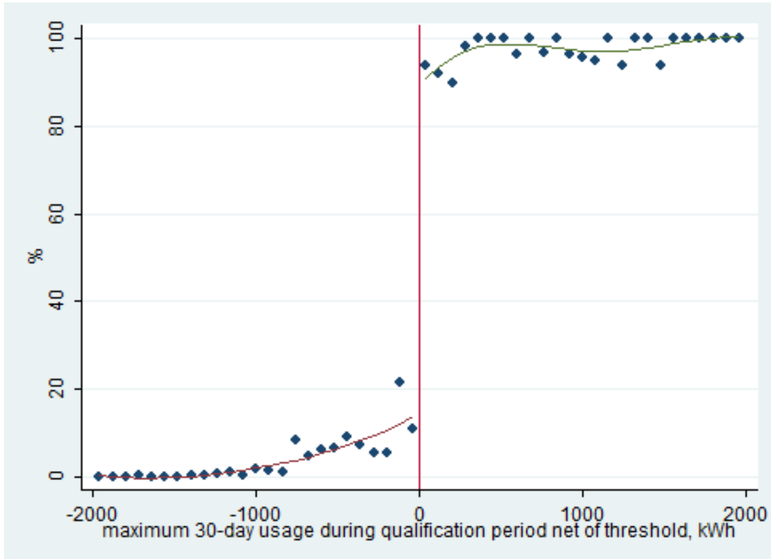
Notes: Standard errors (in parentheses) are based on a non-parametric bootstrap with 1,000 replications. Significance at the 1% (***), 5% (**), and 10% (*) levels is indicated. Each estimate is from a separate regression, and is the estimated TOU treatment effect as a percentage of the estimated non-TOU level at the threshold for the respective dependent variable. Of the 1,105 households included in the regressions for July 2008, 373 are crossers; and the distribution of households is similar in other months.

Figure 1: Density of Forcing/Running Variable



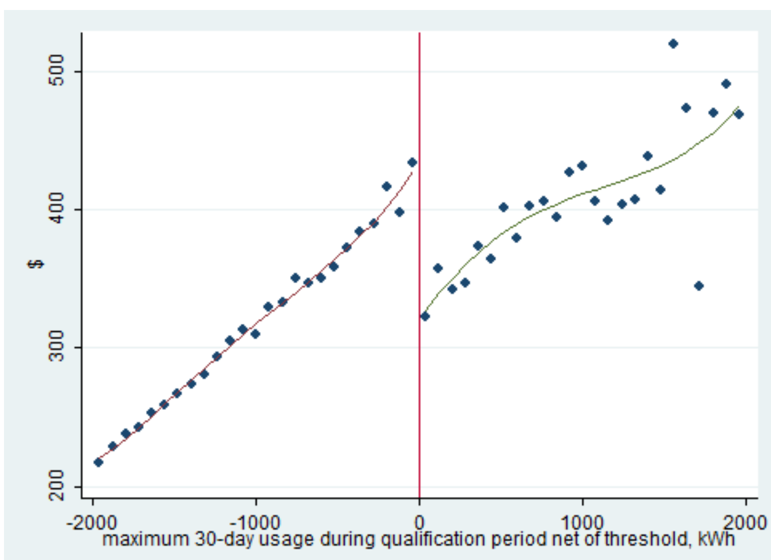
Note: Data are smoothed into bins of width 20kWh. Separate quadratic predictions on each side.

Figure 2: Intent to Treat Effect, Propensity to be Treated, July 2008



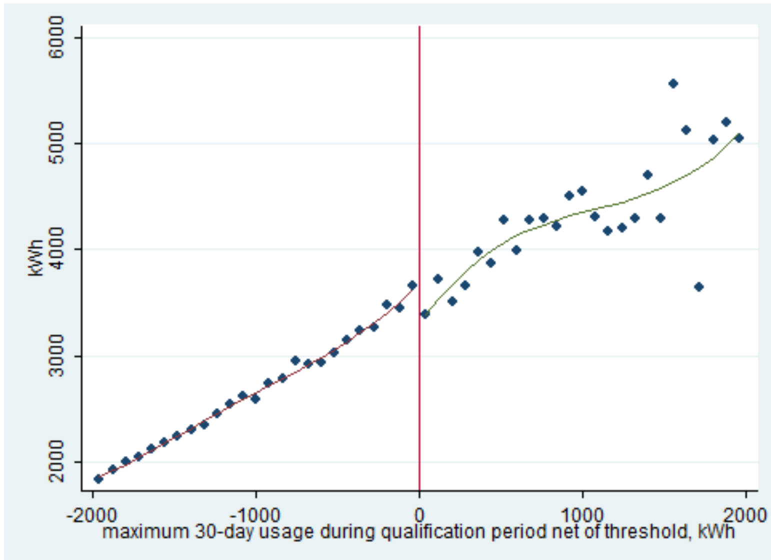
Notes: Data are smoothed into bins of width 80kWh.

Figure 3: Intent to Treat Effect, Total Bill, July 2008



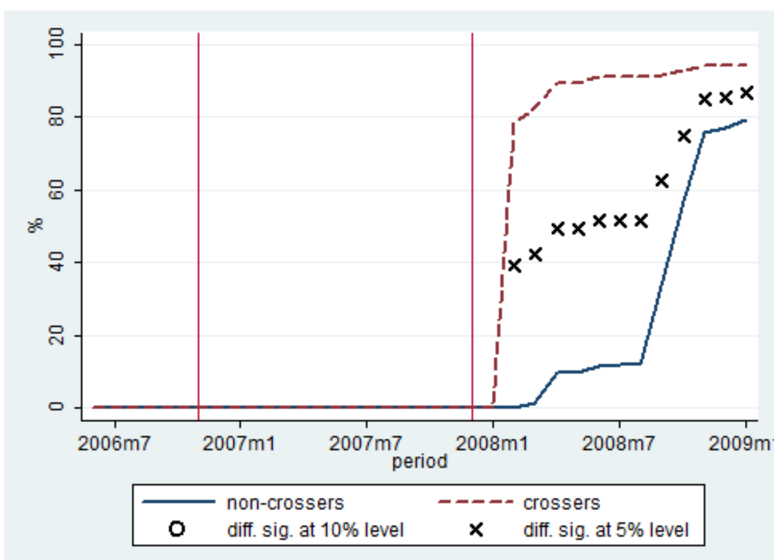
Notes: Data are smoothed into bins of width 80kWh.

Figure 4: Intent to Treat Effect, Total Usage, July 2008



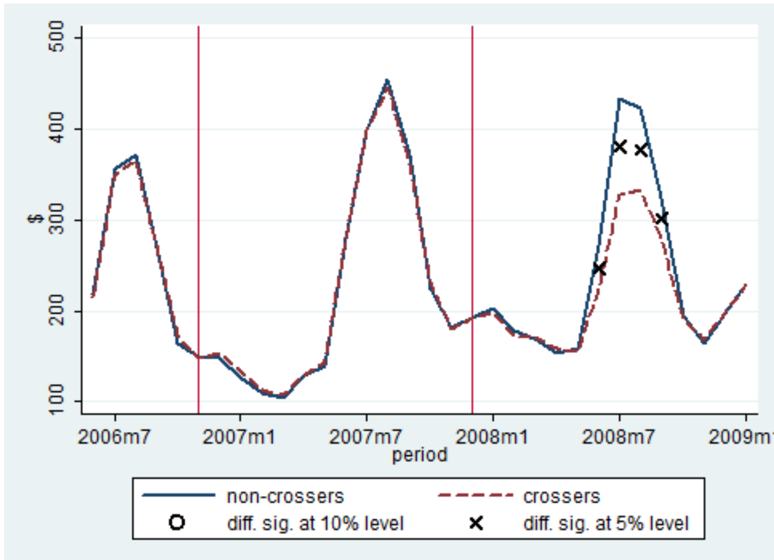
Notes: Data are smoothed into bins of width 80kWh.

Figure 5: Intent to Treat Effects, Propensity to be Treated, 4000kWh Experiment, 600kWh Bandwidth



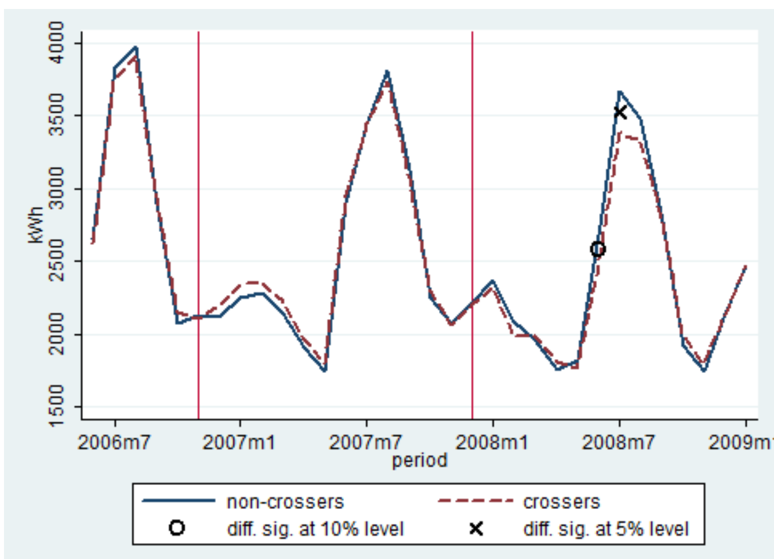
Notes: The qualification period is the set of months between the vertical lines.

Figure 6: Intent to Treat Effects, Total Bill, 600kWh Bandwidth



Notes: The qualification period is the set of months between the vertical lines.

Figure 7: Intent to Treat Effects, Total Usage, 600kWh Bandwidth



Notes: The qualification period is the set of months between the vertical lines.

Figure 8: Budget Lines, Utility Maximization (Unbundled Rates)

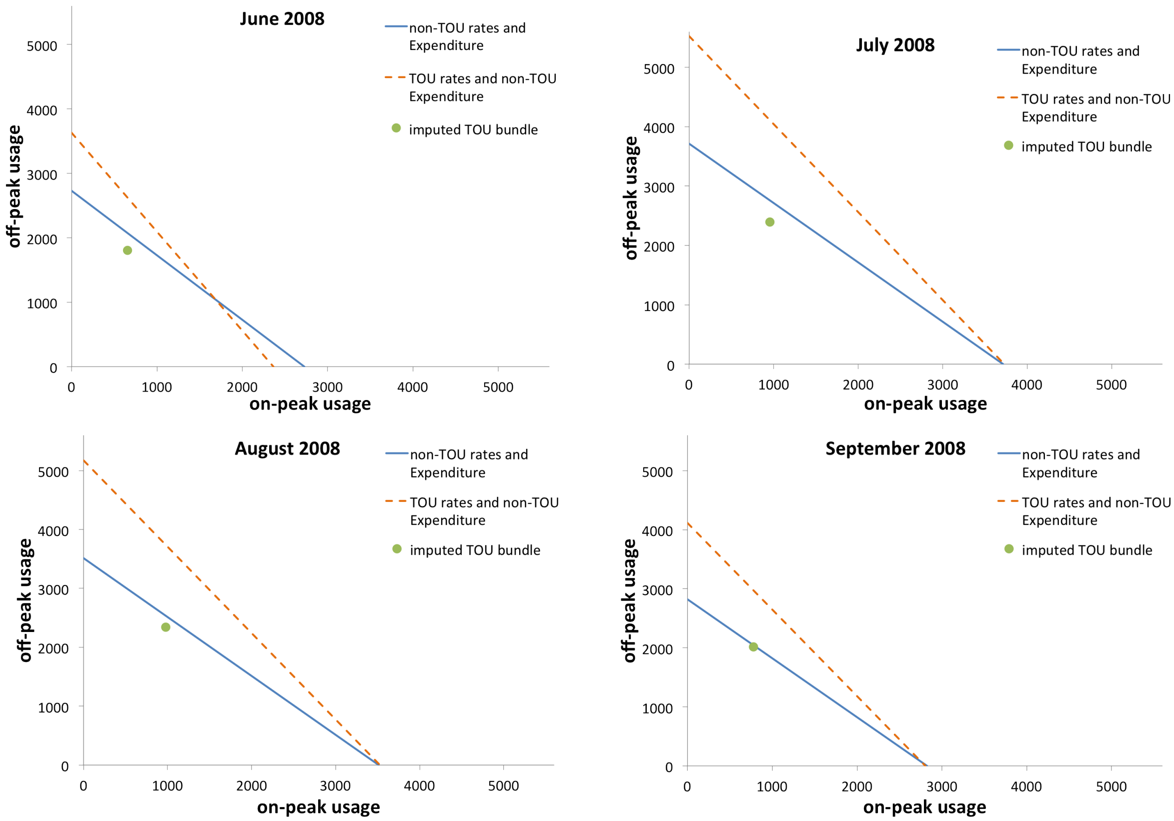


Figure 9: Budget Lines, Utility Maximization (Bundled Rates)

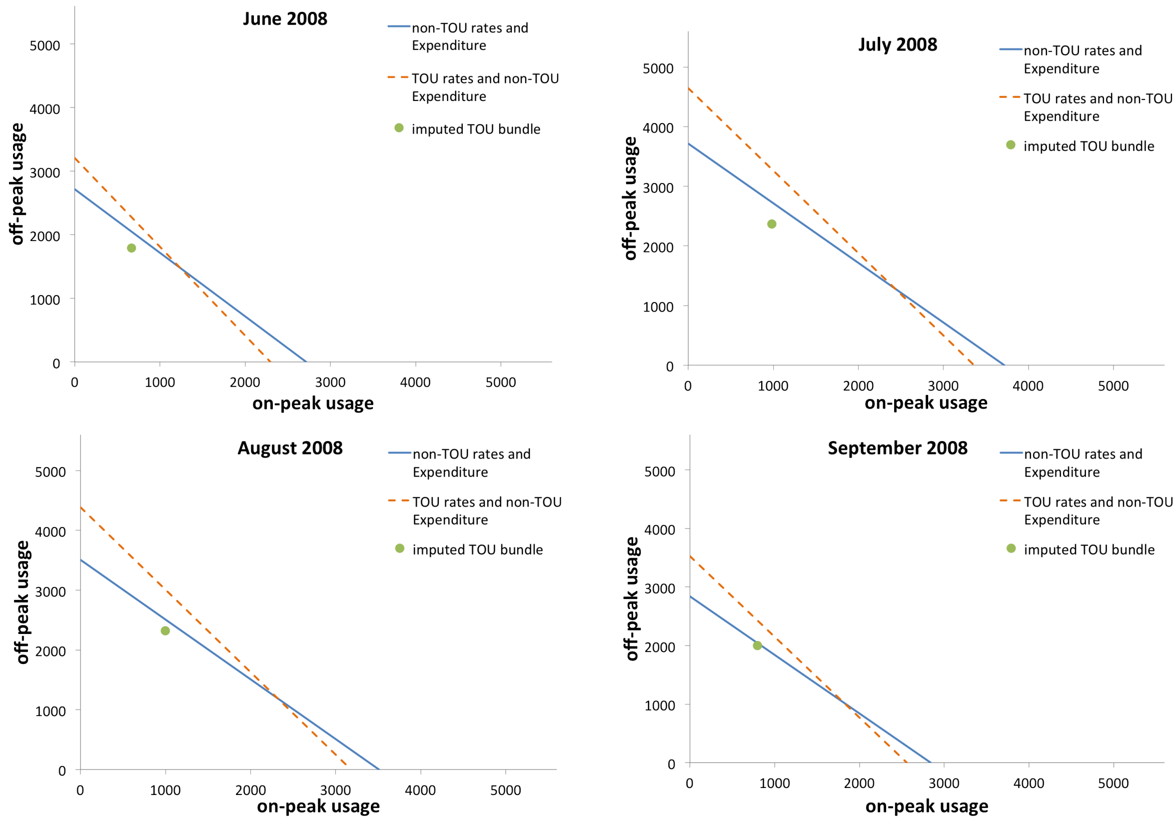


Figure 10: Sensitivity to Bandwidth, Total Usage, July 2008

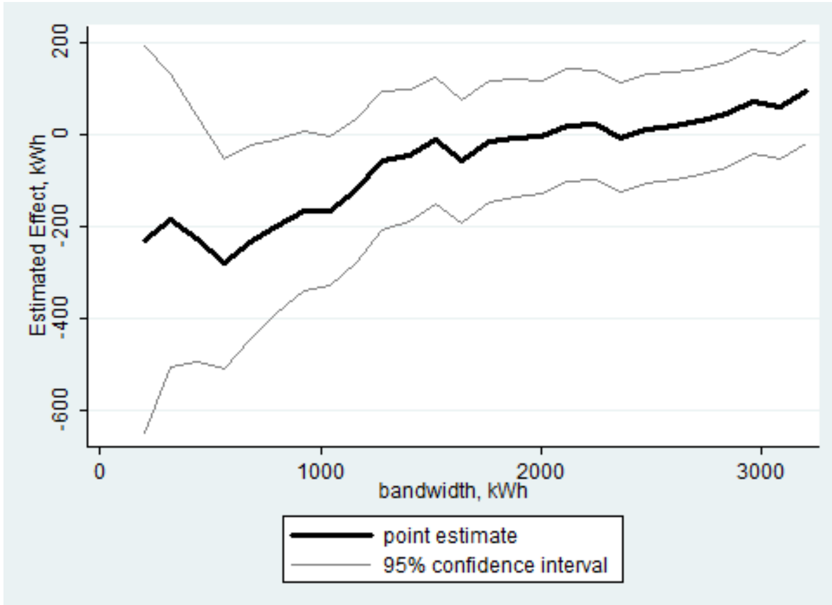


Figure 11: Sensitivity to Bandwidth, Total Bill, July 2008

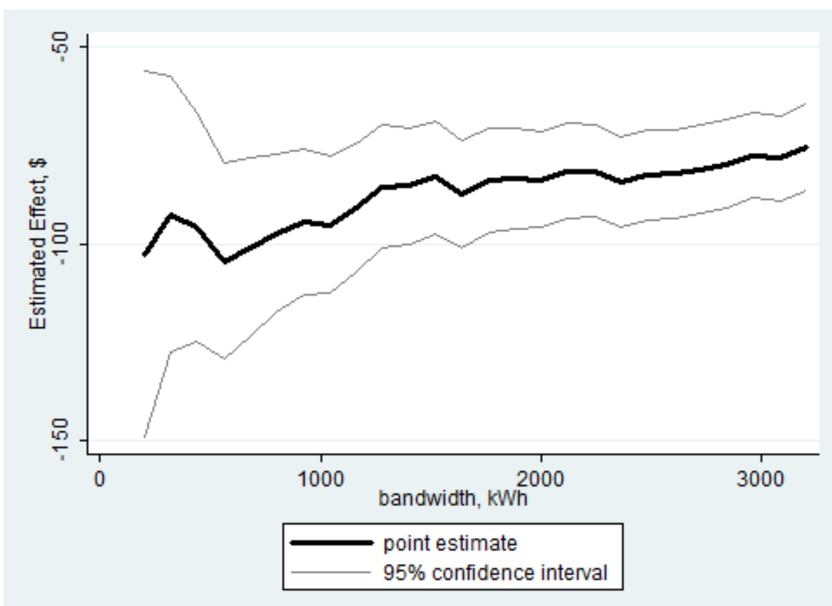
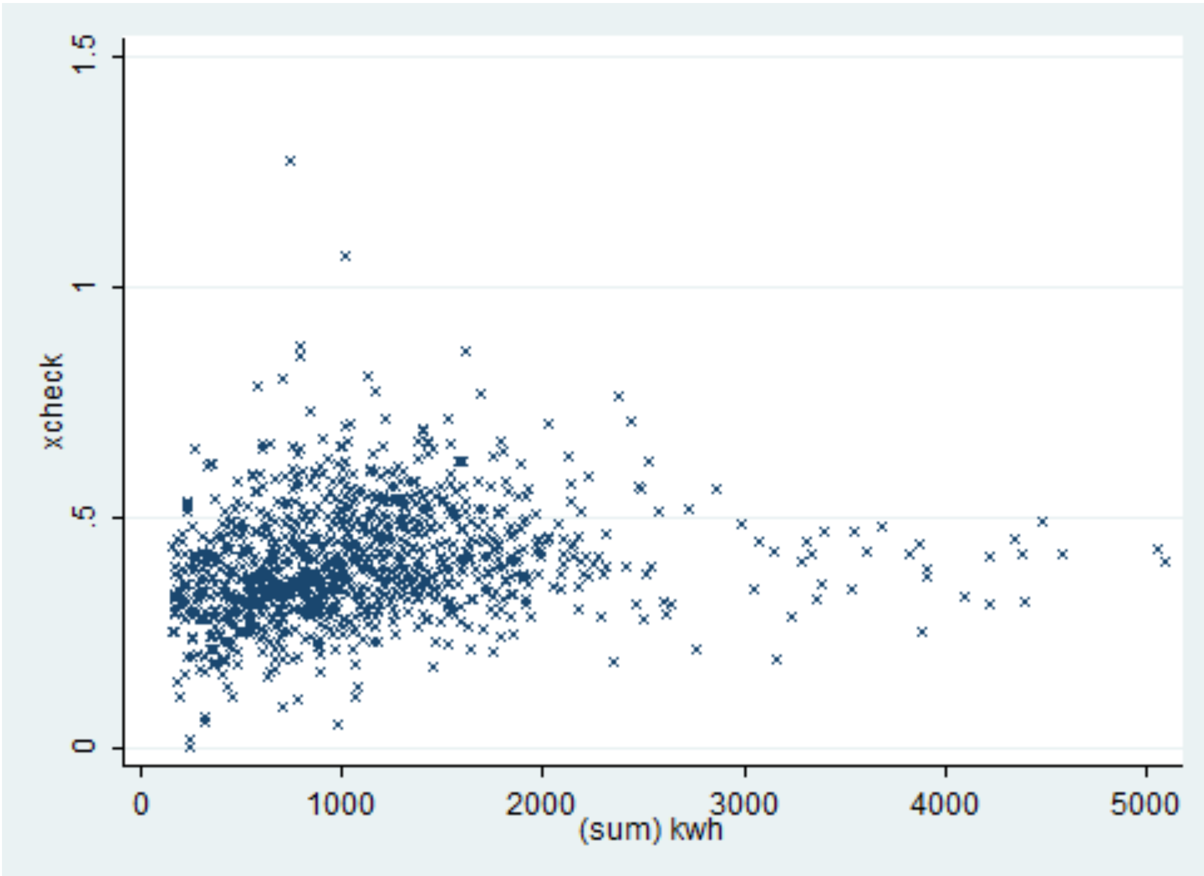


Figure 12: Peak-to-Off-Peak Usage Ratio by Total Usage for Non-TOU Households in the Load Profile Sample, Calendar Month of July, pooled over 2006-2011



A Appendix

A.1 Bandwidth

The trade-off involved with increasing the bandwidth is as follows: on the positive side, the precision of the estimate is improved; on the negative side, a bias is imparted on the estimate of the effect *at* the threshold by including observations further away from the threshold. As discussed by Lee and Lemieux (2010), when the relationship between the forcing variable and the outcome variable is approximately linear on both sides of the threshold, the bias concern becomes less prominent (and, therefore, the optimal bandwidth exercise less useful).

Lee and Lemieux (2010) suggest a plug-in rule-of-thumb bandwidth that we implement in order to derive the optimal bandwidths used in the text and figures. Imbens and Kalyanaraman (2012) provide a completely data-driven approach to selecting an optimal bandwidth, which we have found to produce similar results. In either case, we wish to adopt a uniform bandwidth for every month, dependent variable, and estimator (ITT or treatment effect). To do so, we apply the two-stage rule-of-thumb procedure with a quartic form common to each side of the threshold repeatedly for various treatment months and the ITT specification with total usage and total expenditure as dependent variables. From the set of optimal bandwidth estimates thus produced, we informally choose one in the lower range to apply uniformly in the estimation of all ITTs and treatment effects.

Figures 10 and 11 show the ITT on total usage and the total bill in July 2008 for the 4000kWh experiment, with 95% confidence bounds, for bandwidths ranging from 200 kWh to 3200 kWh. The graphs shows a rapid tightening of the confidence interval and relative stability in the absolute magnitude of the point estimate up to a bandwidth of about 1000kWh. For both usage and total bill, there is a steady decrease in the absolute magnitude of the point estimate and moderate tightening of the confidence interval beyond the 1000 kWh bandwidth. Correspondingly, as shown in Figures 3 and 4, beyond a value of the forcing variable of about 1000 kWh to the right of the threshold, the relation with the usage and bill outcome variables becomes quite non-linear, indicating, along with Figures 10 and 11, that bias is becoming a more prominent concern than precision.

A.2 Bootstrapped Standard Errors

We use nonparametric bootstrap methods to perform statistical inference on the treatment effects for total usage and total bills, which are estimated in levels but reported as percent changes. In this section, we describe the sampling method that we have used. In both notation and procedure, what follows draws upon Cameron and Trivedi (2009).

Let w_i denote the full time series of data for household i, $w_i = (X_i, E_i, TOU_i, C_i, \tilde{X}_i)$ (corresponding to the notation in equation 9, where Y referred generically to either total usage (X), total expenditure (E), or the treatment indicator (TOU)). We draw a bootstrap sample of size N by sampling $w_1, \ldots, w_N$ with replacement at the household level from the subsample of the billing dataset corresponding to the optimal bandwidth restriction. Denoting the bootstrap sample by $w_1^*, \ldots, w_N^*$, we calculate an estimate, $\hat{\theta}^*$, of the vector of parameters of interest, θ, and apply our desired transformation $f(\hat{\theta}^*)$ to these parameter estimates. We repeat this for a total of 1000 separate bootstrap samples. Given the 1000 bootstrap estimates, $f(\hat{\theta}_1^*), \ldots, f(\hat{\theta}_{1000}^*)$, we calculate the bootstrap estimate of the variance-covariance matrix according to

$$\hat{V}_{Boot}(f(\hat{\theta})) = \frac{1}{999} \sum_{b=1}^{1000} \left(f(\hat{\theta}_b^*) - \overline{f(\hat{\theta}^*)} \right) \left(f(\hat{\theta}_b^*) - \overline{f(\hat{\theta}^*)} \right)' \tag{13}$$

where $\overline{f(\hat{\theta}^*)} = \sum_{b=1}^{1000} f(\hat{\theta}_b^*)/1000$.

A.3 Imputation of the TOU Consumption Bundle

We do not observe on-peak and off-peak usage in our billing dataset, but we can use the structure of customers' electric bills to impute a household's on-peak and off-peak usage for months that it is on TOU. When household i is on TOU, its total billed amount E in month t is

$$E_{itT} = p^{on}_{tT} x^{on}_{itT} + p^{off}_{tT} x^{off}_{itT} + g_{tT} \tag{14}$$

where T indicates the TOU pricing regime and x^{on} and x^{off} represent the household's on-peak and off-peak usage respectively. That is, bills depend on a fixed fee g, and on on-peak and off-peak per-kWh charges of p^{on} and p^{off} respectively. Combining this with the fact that on-peak and off-peak usage must sum to the household's observed total usage, X, i.e.

$$X_{its} = x^{on}_{its} + x^{off}_{its} \tag{15}$$

(for either pricing regime $s \in \{T, N\}$), gives two equations in two unknowns. This allows us to solve for on-peak and off-peak usage as functions only of variables that we observe:

$$x^{on}_{itT} = \frac{E_{itT} - g_{tT} - p^{off}_{tT} X_{itT}}{p^{on}_{tT} - p^{off}_{tT}} \quad \text{and} \quad x^{off}_{itT} = \frac{p^{on}_{tT} X_{itT} - g_{tT} - E_{itT}}{p^{on}_{tT} - p^{off}_{tT}}. \tag{16}$$

For the TOU household *at the threshold*, the 2SLS coefficient estimates are inserted as appropriate into these expressions. The corresponding rates must be adjusted to ensure that they reflect the billing cycle that this threshold TOU household is on, which is accomplished as described in the following section.

Note that this imputation is, unfortunately, impossible for non-TOU household-months, as the non-TOU rate is the same for on-peak and off-peak usage, and the non-TOU analogues to the expressions in (16) are hence undefined.

A.4 Billing Cycles

There were 17 distinct billing cycles for residential customers over the period covered by our dataset. Each billing cycle corresponds to a given day of the month (which can change by a couple of days in either direction depending on month and year, due to weekends and holidays) on which the meter is read and the billing period for customers on that billing cycle closes. For customers on billing cycle 1, the total usage and total bill data for “July 2008”, for example, correspond to usage that mostly happened in the calendar month of June; on the other hand, for customers on billing cycle 17, total usage and total bill data for “July 2008” correspond to usage that mostly happened in the calendar month of July. There is thus heterogeneity in our billing data in what “July 2008” (and every other month) refers to. This is relevant because we only have rate information on a calendar-month basis. So the total bill in “July 2008” depends on a weighted average of the rates that were in place in the calendar month of June and those that were in place in the calendar month of July, with the appropriate weight depending on which billing cycle a household is on. We describe here how we retrieve billing cycle weights by household-month, and how we apply the weights thus retrieved to align variables observed on a calendar-month basis with variables observed on a billing-month basis.

We reconstruct the total billed amount for all non-TOU household months based on the observed rates, total usage, and the unknown weight; then solve for the weight that exactly aligns the reconstructed total billed amount with the observed total billed amount for each individual household-month. (We cannot do the same for TOU household-months because we do not observe the on-peak/off-peak breakdown of total usage. We can also not perform the calculation for months in which there was no rate change from the previous month.) A few households chronically had weights outside the sensible 0-1 range in the months for which weights could be calculated, and have been dropped completely from all analysis; a few remaining households occasionally had a month with a nonsensical weight, in which case it was just the single household-month observation that was dropped.

Finally, we calculate average billing cycle weights by billing cycle-month-year group over all household-months we could calculate the initial weights for; fill in the missing month-years (i.e. months across which there were no rate adjustments) with annual averages; then apply the appropriate billing cycle-month-year average to every corresponding non-TOU and TOU household-month. (We observe which billing cycle each household was on in September 2010, and know that households are supposed to always stay on the same billing cycle.)

We need to account for billing cycles in the imputation of on-peak and off-peak usage for the TOU household at the threshold. We align billing-month estimates with calendar-month rates by taking a weighted average of the latter across the relevant months. The weight we use in the calculation must be the billing cycle weight for a TOU household *at the threshold*. This is furnished by once more applying 2SLS estimation to equations (11)-(12), this time with average billing cycle weights as the outcome variable of interest.

We estimate total expenditure based on bundled generation-inclusive rates in a similar fashion. We first impute on-peak and off-peak usage levels for TOU household-months using the method described in the previous section. We then align billing-month usage levels with calendar-month bundled rates *by individual household-month* using average billing cycle weights. Finally, we use the weighted rates and observed usage to estimate what the total generation-inclusive billed amount would have been had the household had the utility as supplier in addition to distributor. Expenditure levels *at the threshold* based on bundled rates are then estimated via the usual application of 2SLS to equations (11)-(12), with these constructed billed amounts as the dependent variable.