Supplemental Material (not copyedited or formatted) for: Brian C. Prest. 2020.
"Peaking Interest: How Awareness Drives the Effectiveness of Time-of-Use Electricity Pricing."
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Web Appendix for *Peaking Interest: How awareness drives the effectiveness of time-of-use electricity pricing*

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A.1 Experimental Details

Figures A.1, A.2, A.3, and A.4 depict the invitation letter, energy usage statement, the refrigerator magnet/stickers, and the in-home electricity monitor.

Customer Supply

National Smart Meter Plan

Mr AB Sample
123 Sample Road
Sample Town
Sample County

Account No: 9999999
MPRN: 99999999

17th November 2008

The National Smart Meter Plan. Make the smart move.

Dear Mr Sample,

I would like to invite you to join the User Trials of the National Smart Meter Plan due to commence shortly. If you choose to take part you will receive €50 credited to your ESB bill as a 'thank you' at the end of the Trial.

What is a Smart Meter?

A Smart Meter is a new electricity meter that:

- Helps you make savings on your bill by helping you reduce the amount of electricity you use
- Provides you with more information on what electricity you use
- Eliminates estimated bills
- Assists you in being energy efficient

In this trial you will be given a Smart Meter. The purpose of the Trial is to see how Smart Meters can help customers reduce the amount of electricity they use. Smart Meters are already in use in other countries and following this trial, will be installed in every home in Ireland.

What happens if you choose to participate?

- ESB Networks will replace your existing electricity meter with a Smart Meter.
- ESB Customer Supply will provide you with information at the start of the trial and at regular intervals throughout
- You will receive €50 credited to your ESB bill at the end of the trial as a 'thank you' for completing a short 10 minute questionnaire at the start and end of the trial.

During the User Trial a number of different ways of improving your energy efficiency enabled by smart metering will be trialled, including 'Time of Use' tariffs (different prices for different times during the day and night). These tariffs and other information will offer you the opportunity to better manage and reduce your electricity bill.

How do you sign up to take part in the Smart Meter User Trials?

- complete and return the tear-off slip at the bottom of this letter
- or phone us on the Lo Call number 1850 21 16 50
- or email us at smartmeter@esb.ie quoting your ESB Customer Supply account number and your name and address.

As this User Trial is limited to 5,000 places nationally, we would ask you to let us know as soon as possible if you wish to participate.

Please note, throughout the trial, all information will remain confidential to you and to ESB Customer Supply.

Want to find out more?

Please read through the enclosed leaflet or, if you prefer, call us on 1850 21 16 50 or email us at smartmeter@esb.ie

Yours sincerely,

Pat Fenlon

Pat Fenlon
General Manager
ESB Customer Supply

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CER

The National Smart Meter Plan is managed by the Commission for Energy Regulation with the support of the Department of Communications, Energy & Natural Resources, Sustainable Energy Ireland, ESB Networks and the electricity and gas industry in Ireland.

Customer Supply

National Smart Meter Plan

Customer Name:	Mr AB Sample	Account No:	9999999
Address:	123 Sample Road Sample Town Sample County	MPRN:	99999999

I agree to participate in the User Trials of The National Smart Meter Plan and to receive information in relation to this Plan from ESB Customer Supply. (Please tick) ☐

My contact number is ______________________

My email address is ______________________

We need your contact number so that ESB Networks can call you to arrange an appointment to install your Smart Meter. Your contact number will be used for account management purposes only. Please note, throughout the trial, all information will remain confidential to you and to ESB Customer Supply.

Figure A.1: Example Invitation Letter

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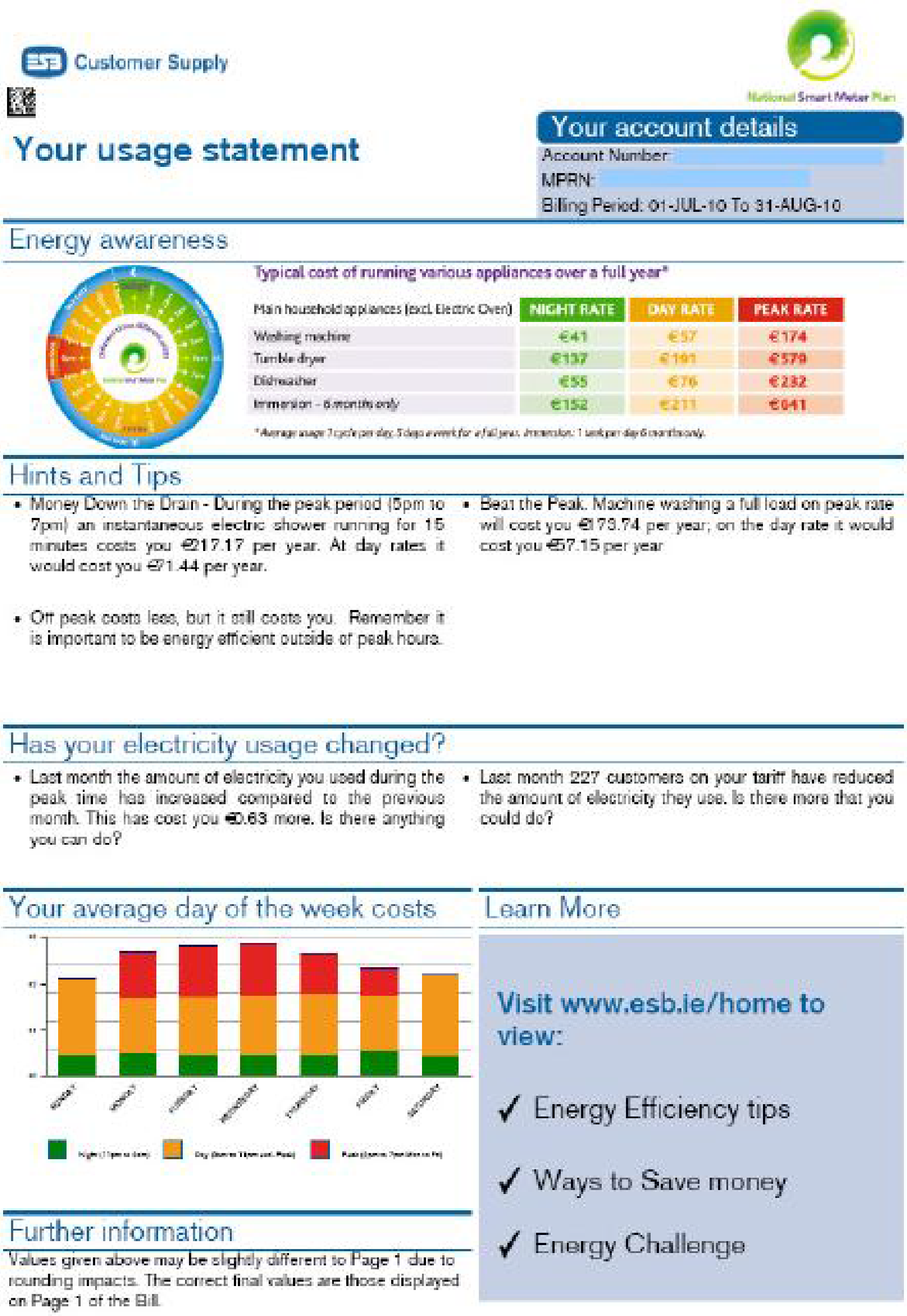

ESB Customer Supply

National Smart Meter Plan

Your usage statement

Your account details

Account Number:
MPRN:
Billing Period: 01-JUL-10 To 31-AUG-10

Energy awareness

Typical cost of running various appliances over a full year*

Main household appliances (excl. Electric Oven)	NIGHT RATE	DAY RATE	PEAK RATE
Washing machine	€41	€57	€174
Tumble dryer	€137	€191	€579
Dishwasher	€55	€76	€232
Immersion - 6 months only	€152	€211	€641

*Average usage 1 cycle per day, 5 days a week for a full year. Immersion: 1 tank per day 6 months only.

Hints and Tips

- Money Down the Drain - During the peak period (5pm to 7pm) an instantaneous electric shower running for 15 minutes costs you €217.17 per year. At day rates it would cost you €71.44 per year.
- Off peak costs less, but it still costs you. Remember it is important to be energy efficient outside of peak hours.
- Beat the Peak. Machine washing a full load on peak rate will cost you €173.74 per year; on the day rate it would cost you €57.15 per year.

Has your electricity usage changed?

- Last month the amount of electricity you used during the peak time has increased compared to the previous month. This has cost you €0.63 more. Is there anything you can do?
- Last month 227 customers on your tariff have reduced the amount of electricity they use. Is there more that you could do?

Your average day of the week costs

Further information

Values given above may be slightly different to Page 1 due to rounding impacts. The correct final values are those displayed on Page 1 of the Bill.

Learn More

Visit www.esb.ie/home to view:

✓ Energy Efficiency tips

✓ Ways to Save money

✓ Energy Challenge

Figure A.2: Energy Usage Statement

Figure A.3: Refrigerator Magnet and Sticker

Figure A.4: In-Home Electricity Display (IHD)

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A.2 Representativeness

An endemic problem with opt-in experiments is selection bias. The self-selected set of households that participate in the study may differ from the population at large. This raises the concern that the self-selected sample may be more responsive than the general population, leading to exaggerated treatment effects. Table A.1 shows the representativeness of the sample by comparing observable characteristics in the sample to (i) those who were invited but did not opt-in (based on a follow-up survey) and (ii) the general population (based on census data). For the former group, once recruitment was completed in July 2009, CER conducted an additional survey of households that were invited to the program but did not opt in. This is called the "non-response survey", since these are households who did not respond to the invitation to the program. The survey included many of the same questions that were asked of the participating households, allowing a comparison of those who did not opt in to the program.

Since the "non-responders" are also a select group (those who did not opt in, but voluntarily participated in a later survey), I also compare the sample to the general population using census data.[1] This offers an additional check on the representativeness of the sample, but there are two limitations to this comparison. First, the census data was mostly collected in 2011 (and certain data were only collected in 2015), whereas the variables in my sample were measured in the 2009 pre-trial survey. Second, the census questions are often worded somewhat differently than in the program survey. For education, the census also includes "education not yet ceased" as an allowable answer, whereas the program survey does not. For social class, the categories are defined differently, requiring some rough aggregation, which is not always cleanly possible. This makes the census data imperfectly comparable, but I include them nonetheless as an additional benchmark.

As seen in Table A.1, the recruited sample is broadly similar to the non-responders and mostly similar to the general population, with a few exceptions. Recruited households are slightly older (e.g., only 8 percent of the sample is between the ages of 26 and 35, compared to 12 percent of non-responders and 22 percent of the general adult population). On age, the sample better resembles non-responders than it does the general population because the invitations were targeted towards older, less transient households to reduce attrition. For the same reason, home-owning households in detached houses are overrepresented as well. Otherwise, the sample is generally representative across social class,[2] education, home age, and appliance ownership. The sample is slightly less likely to have electric space heating (7 percent versus 13 percent of non-responders and 9 percent of the population), but similarly likely to have electric water heating (56 percent versus 59 percent for non-responders), which is a major consumer of electricity in Ireland. In general, the sample is fairly representative on observables. While this allays some concerns about selection issues, it is of course impossible to guarantee representativeness on unobservables, which is a common limitation of voluntary randomized trials such as this one.

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[1] This data is publicly available at www.cso.ie.

[2] As measured by the United Kingdom's National Readership Survey (NRS) social grade. See https://en.wikipedia.org/wiki/NRS_social_grade.

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Table A.1: Sample Representativeness

	Mean			Difference vs. Sample	
	Sample	Non-responders	Census	Non-responders	Census
Demographics					
Age Group: 18-25 (Indicator)	0.003	0.01	0.14	-0.007	-0.14
Age Group: 26-35 (Indicator)	0.08	0.12	0.22	-0.04	-0.14
Age Group: 36-45 (Indicator)	0.20	0.22	0.20	-0.02	-0.0004
Age Group: 46-55 (Indicator)	0.25	0.19	0.17	0.06	0.08
Age Group: 56-65 (Indicator)	0.22	0.20	0.13	0.02	0.09
Age Group: 65+ (Indicator)	0.24	0.25	0.15	-0.01	0.09
Number of Residents in Home	2.73	2.68	2.72	0.04	-0.01
Has Children Under 15 in Home (Indicator)	0.27	0.36	0.34	-0.09	-0.08
Social Class: AB, Manager/Professional (Indicator)	0.15	0.13	na†	0.02	
Social Class: C1, White collar (Indicator)	0.27	0.26	na†	0.01	
Social Class: C2, Skilled manual (Indicator)	0.16	0.17	0.16	-0.01	0.005
Social Class: DE, Unskilled/other (Indicator)	0.39	0.40	0.34†	-0.01	0.05
Social Class: Farmer (Indicator)	0.03	0.04	na†	-0.01	
Education: None (Indicator)	0.01		0.01		-0.0002
Education: Primary (Indicator)	0.12		0.16		-0.05
Education: Secondary (Indicator)	0.45		0.49		-0.04
Education: Third (Indicator)	0.36		0.29		0.07
Education: Refused (Indicator)	0.05		0.04		0.02
Home Characteristics					
Own home (Indicator)	0.94	0.88	0.70	0.06	0.20
Home Age: Less than 5 years (Indicator)	0.05	0.11		-0.06	
Home Age: 5-9 years (Indicator)	0.14	0.12		0.02	
Home Age: 10-29 years (Indicator)	0.29	0.26		0.03	
Home Age: 30-74 years (Indicator)	0.41	0.38		0.03	
Home Age: More than 75 years (Indicator)	0.12	0.13		-0.01	
Home Style: Apartment (Indicator)	0.01	0.03	0.11	-0.02	-0.08
Home Style: Detached/Bungalow (Indicator)	0.54	0.49	0.42	0.06	0.11
Home Style: Semi-Detached (Indicator)	0.31	0.31	0.28	-0.005	0.03
Home Style: Terraced/Townhome (Indicator)	0.14	0.17	0.17	-0.03	-0.03
Number of Bedrooms	3.48	3.25		0.23	
Appliances and Electronics					
Home Heat: Electric (Indicator)	0.07	0.13	0.09	-0.06	-0.01
Water Heat: Electric, Immersion (Indicator)	0.56	0.59		-0.03	
Washing Machine (Indicator)	0.99	0.98	0.98†	0.01	0.01
Tumble Dryer (Indicator)	0.69	0.67	0.65†	0.02	0.04
Dishwasher (Indicator)	0.67	0.58	0.65†	0.09	0.02
Stand alone freezer (Indicator)	0.51	0.55		-0.04	
Cook stove type: Electric (Indicator)	0.70	0.69		0.07	
Internet Access in Home (Indicator)	0.70		0.72		-0.02

†: See notes. Census figures are from 2011 census, the temporally closest census to the time of the trial. Census questions are often structured differently for the population than they are for the program survey in the "Sample" column. For example, the age groups differ (e.g., 15-19 years old), requiring some estimation. In addition, the "Social Class" categories differ, making it impossible to assign certain classes of workers to AB, C1, or both, or to disaggregate DE and Farmers. Census data for washing machines, tumble dryers, and dishwashers are only available for 2015.

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A.3 Overview of Regression Trees, Causal Trees, and Extension

3.1 Overview of Regression Trees and Cross Validation

This section provides an overview of regression trees and cross-validation for readers with little background in these methods.

Regression trees, also referred to as classification and regression trees (CART), recursively partition the covariate space into subsets called "branches." For example, a branch could divide the data into households with a college education versus those without. A branch could also divide the data along continuous variables, such as splitting the data according to whether energy consumption is above or below some threshold (say, 0.2 kWh), where the threshold is chosen to maximize model fit. Within a branch, the predicted outcome variable is simply the sample average of the outcome variable (Y) for observations in that branch. At each step, the variables chosen to split are those that best reduce the in-sample model fit. The algorithm continues to divide the subsetted data along new branches until additional splitting would not sufficiently increase the model fit. The end result is that the data is divided into non-overlapping hyper-rectangles that divide observables into discrete subsets called "leaves". In standard regression trees, every observation then falls into exactly one leaf with its own predicted value, equal to the simple average of the outcome variable for all observations in that leaf.

Trees that are grown in an unrestricted manner will generally overfit the data, leading to poor predictions out of sample. To mitigate this, trees are "pruned" using cross-validation. This involves removing branches to produce a smaller, parsimonious tree. The optimal tree size is determined by cross validation.

In cross validation, the data is randomly divided into K sets (typically 5-10) of approximately equal size. For each set, the modeler sets aside that data and uses the remaining observations (called the training set) to fit trees of varying sizes (from a null model of no splits at all to the fully grown tree). Each model is used to predict the outcome variable in the hold-out set (also called the test set). The out-of-sample predictive performance of each model is assessed by computing the root mean square error (RMSE) of the predicted outcome value versus its true value in the test set. This process is repeated for each of the K hold-out sets, and the K test RMSEs for each model are then averaged, providing a measure of out-of-sample performance of models of varying complexity. Highly complex models tend to overfit the data, leading to large mean test RMSEs and are generally rejected by cross-validation. The model with the optimal level of complexity according to test RMSEs is then chosen. Finally, this model is fit on the full data. This leads to a parsimonious tree that is simple to interpret and has superior out-of-sample prediction properties.

3.2 Overview of Causal Trees

The CT estimator uses the Rubin Causal Model (RCM) as a framework. The RCM postulates two potential outcomes, $Y_i(1)$ and $Y_i(0)$ (e.g., electricity consumption), for each unit i (e.g., household): one where the household is treated ($W_i = 1$) and another in the case where the household is not treated ($W_i = 0$). Since a household cannot be both treated and untreated simultaneously, the econometrician observes only one of these two potential outcomes:

$$Y_i^{obs} = Y_i(W_i) = \begin{cases} Y_i(1) & \text{if } W_i = 1 \\ Y_i(0) & \text{if } W_i = 0 \end{cases} \tag{1}$$

The true treatment effect is then $Y_i(1) - Y_i(0)$. Much of the applied causal inference literature involves estimating the average of this treatment effect (LATE), denoted τ:

$$\tau = \mathbb{E}[Y_i(1) - Y_i(0)]. \tag{2}$$

This typically is estimated using the sample analogue,

$$\hat{\tau} \equiv \frac{1}{n_T} \sum_{i \in \mathcal{S}_T} Y_i - \frac{1}{n_C} \sum_{i \in \mathcal{S}_C} Y_i. \tag{3}$$

where $\mathcal{S}_T$ and $\mathcal{S}_C$ represent the treatment and control groups, respectively, and $n_T = |\mathcal{S}_T|$ and $n_C = |\mathcal{S}_C|$ are the number of households in the treatment and control groups, respectively.[3]

Whereas this represents the *overall* average treatment effect on treated households (the local average treatment effect, or LATE), the Athey and Imbens (2016) algorithm (hereafter referred to as the causal tree (CT) algorithm) focuses on estimating heterogeneity in this effect. Specifically, it estimates *conditional* average treatment effects (CATE) — that is, the treatment effect conditional on a particular value of observables (X_i). The CATE is defined as the treatment effect *as a function of x*,

$$\tau(x) = \mathbb{E}[Y_i(1) - Y_i(0) | X_i = x]. \tag{4}$$

An example of a conditional average treatment effect is the average effect among college-educated women over the age of 65. With many different observables X_i, some of which may be continuous or interact with each other, estimating $\tau(x)$ is a daunting task, particularly if one wishes to estimate it non-parametrically.

The CT algorithm uses regression trees to estimate $\tau(x)$ in a parsimonious yet non-parametric manner. In particular, the CT algorithm recursively splits the data along covariates into hyper-rectangles, estimating the treatment effect separately within each subset, called "leaves." This is called growing the tree, as described in the previous subsection. Once the splits are found, estimating the CATEs ($\tau(x)$) is straightforward. Given a tree (denoted Π), the CATE for an individual with characteristics x is simply the difference in conditional means for treatment and control outcomes falling in the same leaf as x (denoted $\ell(x, \Pi)$):

$$\hat{\tau}(x) \equiv \hat{\mu}(x; S_T, \Pi) - \hat{\mu}(x; S_C, \Pi), \tag{5}$$

where $\hat{\mu}(x; S, \Pi)$ is simply the conditional mean of observed outcomes in group $\mathcal{S}$ (i.e., treatment or control) in the leaf of tree Π where x falls.

$$\hat{\mu}(x; S, \Pi) \equiv \frac{1}{|\{i \in \mathcal{S} : X_i \in \ell(x, \Pi)\}|} \sum_{i \in \mathcal{S}: X_i \in \ell(x, \Pi)} Y_i. \tag{6}$$

[3] The consistency of this estimator requires assumptions on balance, propensity score weighting, a difference-in-differences technique, or other similar method. I ignore this distinction here for expositional purposes, but I use the difference-in-differences estimator to ensure consistency.

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This is analogous to simply computing the LATE for a each subgroup in the data, where the tree determines the subgroups.

The key innovations in Athey and Imbens (2016) are that it solves two practical difficulties in growing the tree. The first is how to grow the tree to begin with, which is far simpler for standard regression trees that are meant to predict a single outcome variable, Y_i, than for treatment effects τ. The second is how to determine the optimal size of the tree, since cross-validation techniques require the researcher to know the ground truth in the outcome of interest. In standard regression problems, the outcome of interest is observed (the outcome, Y_i), but in causal inference it is not (the treatment effect, τ).

Athey and Imbens (2016) solves these problems in multiple ways, but the simplest way is through a simple transformation of the outcome variable, Y. Denoting the overall probability of treatment $p = n_T/(n_T + n_C) = n_T/n$, they transform the outcome variable as follows:

$$Y_i^* \equiv Y_i \frac{W_i - p}{p(1-p)} = \begin{cases} Y_i/p & \text{if } W_i = 1 \\ -Y_i/(1-p) & \text{if } W_i = 0 \end{cases} \tag{7}$$

As a result of this transformation, the simple average of Y_i^* is equal to the LATE:

$$\begin{aligned} \frac{1}{n}\sum_i Y_i^* &= \frac{1}{n}\left[\sum_{i\in\mathcal{S}_T} \frac{Y_i}{p} + \sum_{i\in\mathcal{S}_C} \frac{-Y_i}{1-p}\right] \\ &= \frac{1}{n}\left[\sum_{i\in\mathcal{S}_T} \frac{Y_i}{n_T/n} + \sum_{i\in\mathcal{S}_C} \frac{-Y_i}{n_C/n}\right] \\ &= \frac{1}{n_T}\sum_{i\in\mathcal{S}_T} Y_i - \frac{1}{n_C}\sum_{i\in\mathcal{S}_C} Y_i \\ &= \hat{\tau} \end{aligned} \tag{8}$$

In this way, the average of the transformed variable captures the average treatment effect, without any explicit need to further distinguish between treatment and control groups. This equality holds exactly for the full sample, and it also holds in expectation for subsets of the data after conditioning on observables.[4] Therefore, one can then simply use standard tree-based regression methods and cross-validation approaches to predict the conditional mean of this transformed variable Y_i^*, in place of the actual outcome variable.[5]

[4]It holds in expectation for each conditional subset of the data, but may not hold exactly for any particular subset. This is because the share of observations treated in that subset of the data may not be exactly the same as the share treated in the full sample due to sampling variability. However, in a randomized experiment, the expected probability of treatment for a subset equals the overall treatment probability, which is why the equality holds in expectation.

[5]While Athey and Imbens (2016) note a number of problems with this simple approach, their algorithm is somewhat more sophisticated than this example presents in order to correct for these. For example, to the extent that treatment probability for a particular leaf differs from the overall probability p, the average of Y_i^* will differ somewhat from the actual CATE among observations in that leaf. To solve that, one simply computes the actual CATE for those observations. Another problem is that a tree grown using Y_i^* could potentially form leaves with no control observations. That is solved by precluding splits that would result in branches with fewer than a pre-defined number of treatment and control observations. See Athey and Imbens (2016) for a full description of

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While the Athey and Imbens (2016) algorithm is somewhat more sophisticated than this, it illustrates how one can translate a difficult treatment effect heterogeneity problem into a somewhat more manageable non-parametric regression problem. I use the more sophisticated version of the algorithm and add two extensions, described in the next subsections.[6]

3.3 Extensions to the Athey-Imbens Causal Tree Algorithm

The CT algorithm is designed for standard experiments with a single treatment group and a single observation per treated unit. I extend it to suit the circumstances of this data, which features multiple treatment groups and calls for a difference-in-differences strategy. These circumstances are very common, and so my extensions are generally applicable.

First, I extend this algorithm to a difference-in-differences framework where households are observed both before and after treatment. This extension is straightforward, as one need only replace the outcome variable Y_i in the CT algorithm with its change from pre- to post-treatment means, $\Delta Y_i \equiv \overline{Y}_{i,t'} - \overline{Y}_{i,t}$, where the mean is taken over time for each period, t' (the treatment period) and t (baseline period). This captures the first difference, and indeed has better statistical properties than panel data methods, as documented in Bertrand, Duflo and Mullainathan (2004). The CT algorithm then computes the difference in these differences.[7]

Second, I extend the CT algorithm to estimate heterogeneity in treatment effects across multiple treatment groups. This extension is more substantial. The natural way to implement multiple treatment effects in the CT algorithm would be to include an indicator variable for each of the $m \in \{1, ..., M\}$ treatment groups, denoted W_i^m, allowing the algorithm to find heterogeneity in treatment effects across those indicators. For example, if treatment group m resulted in larger treatment effects than others, the tree could form a new branch for all observations with $W_i^m = 1$. The practical problem with this approach is that such a split would result in no control observations in that side of the branch, making it impossible to estimate treatment effects.

The solution to this is to replace each control observation with M copies of it, each pseudo-assigned to a different treatment group. That is, for each control observation i and treatment $m \in \{1, ..., M\}$, generate a new pseudo-observation i_m with

$$\begin{aligned} Y_{i_m} &\equiv Y_i \\ X_{i_m} &\equiv X_i \\ W_{i_m} &\equiv W_i \ (=0) \\ \text{For } m' \in \{1, ..., M\} \quad W_{i_m}^{m'} &\equiv \begin{cases} 1 & \text{for } m' = m \\ 0 & \text{for } m' \neq m, \end{cases} \end{aligned}$$

the many variations on their algorithm. I use a form of their transformed outcome tree to grow the tree, but then report the true CATEs in the nodes. I use this form because simulations suggest that the alternative forms have difficulty finding some CATEs in the presence of certain kinds of correlations in the data generating process.

[6] An even more sophisticated algorithm is causal forests, which further extends causal trees to use random forests. As a sensitivity, I also conducted this analysis. The results were very similar. I use the simpler causal tree but causal forests are much more difficult to interpret, as that method produces thousands of individual treatment effects that are not easily summarized in a table or graphic.

[7] If households are observed the same number of times before and after the treatment, the resulting estimates of the LATE and CATEs are numerically equivalent to the standard difference-in-differences estimates.

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for a total of $M \times n_C$ control observations, in place of the original n_C observations. This leads to a new pseudo-sample size of $\tilde{n} = (n_T + Mn_C)$. With this transformed dataset, the tree algorithm can split on the treatment group covariates (W_i^m) while continuing to use W_i as the treatment indicator. Meanwhile, duplicating control group observations does not alter the means of control outcomes or observables. As a result, the LATE and CATE estimates on the transformed data are numerically equivalent at every branch to the corresponding estimates on the untransformed data.

To see this formally, consider the mother node, before any splits have occurred, where there is no difference between the LATE and the CATE. Simply considering the LATE, it is easy to show that the LATE of the transformed data is numerically equivalent to the LATE of the untransformed data. The estimated LATE on the transformed dataset is (denoting the set of the transformed control observations $\tilde{\mathcal{S}}_C$)

$$\begin{aligned} \frac{1}{n_T}\sum_{i\in\mathcal{S}_T} Y_i - \frac{1}{M\times n_C}\sum_{i\in\tilde{\mathcal{S}}_C} Y_i &= \frac{1}{n_T}\sum_{i\in\mathcal{S}_T} Y_i - \frac{1}{M\times n_C} M\sum_{i\in\mathcal{S}_C} Y_i \\ &= \frac{1}{n_T}\sum_{i\in\mathcal{S}_T} Y_i - \frac{1}{n_C}\sum_{i\in\mathcal{S}_C} Y_i \\ &= \hat{\tau}, \end{aligned} \tag{9}$$

which is the same as the LATE estimate of the untransformed data.

Moreover, the CATE of the transformed data at any node of the tree is also equal to the CATE for the original dataset, even after splitting on any combination of observables and/or treatment groups. Showing this involves only two differences compared to the above logic. First, the means are conditional on X_i and W_i^m. And second, the values of M in the second term of equation (9) are decremented by one for each treatment group that has previously been split upon, as those groups have been previously diverted into another branch. This latter difference does not affect the estimated CATE, because the decrement appears in both the numerator and the denominator of the second term, and therefore cancels.

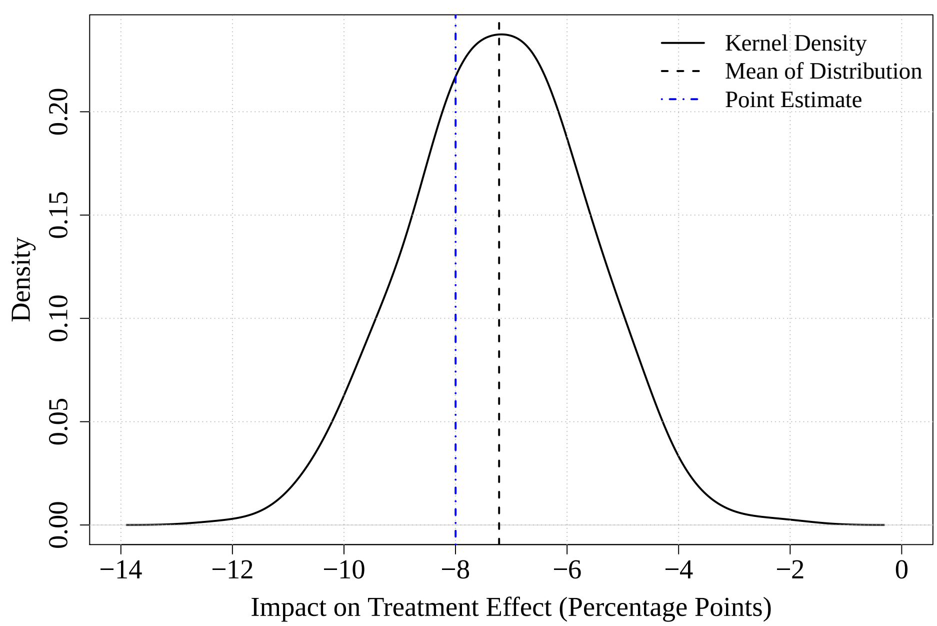
When there are overlapping dimensions of treatment, the process can be simplified somewhat to reduce computation time. One need not create an additional observation and indicator variable for each permutation of the different treatment dimensions. For example, in my data, households received one of four information treatments and one of four pricing schedules, resulting in 16 distinct treatment groups. In this case, the process outlined above can be executed twice, with $M = 4$ each time, leading to 8 $(= 4 + 4)$ copies of the control observations, rather than 16 $(= 4 \times 4)$, resulting in a pseudo-sample size of $\tilde{n} = (n_T + (4 + 4)n_c)$.

While the point estimates of the transformed data are equivalent to those of the original data, the duplicated observations are perfectly correlated (i.e., the observations are not independent) and threaten to bias standard errors. To correct for this, one can either cluster standard errors at the household level or compute standard errors using the original dataset with standard methods. A related issue arises in cross validation because now households appear in the data multiple times, and standard resampling methods are likely to assign a single household's multiple pseudo-observations to separate CV groups. To solve this, households should be block-assigned to CV groups.

A.4 Robustness Checks

4.1 Sensitivity Analysis: Random Assignment of "Awareness" in Control Group

As mentioned in section 6.2.1 and in footnote 35, the awareness variable is randomly assigned for control households. This is because it is not possible to know for certain which control households would have been "aware" had they been treated. As previously mentioned in footnote 35, I run a sensitivity analysis in which I re-estimate the awareness-treatment interaction term using 1,000 alternative random assignments of "awareness" for control households.[8] The distribution (kernel density) of those 1,000 coefficients is shown in Figure A.5. All 1,000 coefficients are negative, and 98% of them are statistically significant at the 5% level. The distribution of the effect is centered around a mean of -7.2 percentage points (i.e., awareness amplifies the treatment effect by 7.2 percentage points). This is slightly smaller than the point estimate of -8 percentage points in the main results (e.g., comparing nodes [2] and [11] near the top of Figure 5), but this is well within the range of normal sampling variability given that the point estimate has a standard error of 2 percentage points.



Notes: Distribution represents the kernel density of the coefficient on the awareness-treatment interaction. A Gaussian kernel with a bandwidth of 0.5 percentage points is used.

Figure A.5: Sensitivity Analysis — Effect of Awareness on Treatment Effect under 1,000 Alternative "Awareness" Assignments for the Control Group

[8] I estimate this treatment effect using the standard linear model in equation (2), plus an awareness indicator and interaction term.

4.2 Placebo Test: Holidays and Weekends

Figure A.6 shows the results of a placebo test, in which I re-estimate (1) for periods when time-of-use pricing was not active: holidays and weekends. It shows little average treatment effect. The is a small but significant reduction during peak hours (when prices are normally high on a weekday) and a small but significant increase between 12:00am and 12:30am (when prices are normally be low on weekdays). This is likely explained by some combination of consumer confusion about holiday/weekend pricing, habit formation, and scheduled automation of home appliances.[9] However, the fact that the treatment effect mostly disappears relative to Figure 2 confirms that the key results are indeed real.

These results also suggest that the information treatments did not on their own serve to significantly reduce consumption. If they were effective, we should see treatment effects when time-of-use pricing is not applied, regardless of the time of day. No such pattern appears in Figure A.6, with a few small but marginally significant exceptions. This suggests that the treatment effect is driven by reaction to time-of-use pricing, not simply information provision. Of course, this does not mean that information provision is irrelevant. As the results in section 6 show, information provision can amplify the effects of time-of-use pricing, even if it is not particularly effective on its own.

[9] Fowlie et al. (2017) found an analogous effect in an experiment from California.

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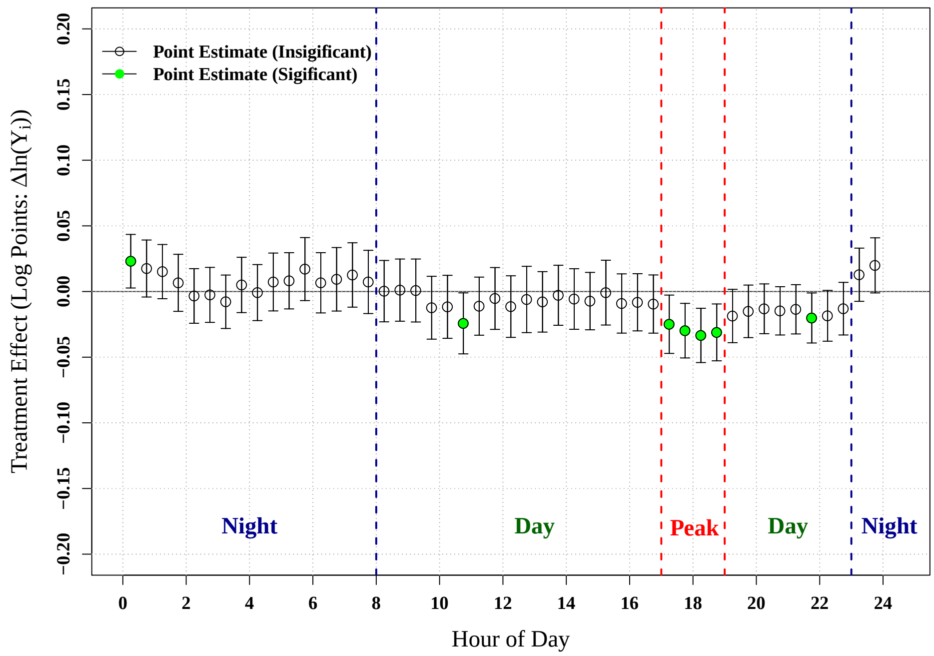

Notes: Error bars represent 95% confidence intervals. Standard errors are two-way clustered at the household and week-of-sample levels. Solid dots are statistically significant at the 95% level; empty dots are not. The horizontal placement of points indicates the middle of time period (e.g., the first point, located at the x-coordinate of approximately 0.25, is the half hour spanning 12:00am – 12:30am, and the last point laying between the two red vertical lines spanning 18:30 – 19:00 (6:30pm – 7:00pm), which is the last half-hour of the peak period). Peak periods are shown for comparison with Figure 2, but in fact rates during holidays and weekends were actually fixed at the day rate value. Only days without active time-of-use pricing (weekends and holidays) are included. For each point estimate, N = 492,850, except for 12:30am – 1:00am and 1:00am – 1:30am where some hours are missing due to daylight savings time, for which N = 489,844.

Figure A.6: Placebo Test — Estimated Treatment Effects on Holidays and Weekends

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4.3 Time-Varying in Responses to TOU Pricing

In this section, I assess whether there are time-varying differences in how households respond to TOU pricing. This is helpful for two reasons. First, it suggests whether the average response over the year is masking varying short-run versus long-run effects. Novelty could lead to decaying responses over time, whereas learning would suggest growing responses. Second, we can use time-varying responses to assess whether the lump-sum payments (which were given to participants at the beginning and end of the program) might have dampened the perceived price incentive.

To estimate time-varying responses, I estimate the following variant of equation (1):

$$\ln(Y_{i,t}) = \sum_{w=1}^{53} \eta_w W_{i,w,t} + \alpha_i + \lambda_w + \epsilon_{i,t}. \tag{10}$$

where only peak periods are included in the regression (5:00pm to 7:00pm). This specification allows for different treatment effects for each week of the year. Figure A.7 shows the estimated η_w values along with 95 percent confidence intervals. The estimated treatment effects do not show strong seasonality over time. The only notable differences are that the effect appears to be a slightly bigger in the initial weeks of the trial, and perhaps slightly smaller during the final weeks. The latter effect could be due to changes in responsiveness near the end of the trial and/or a lower likelihood of responding during the December holiday season when outdoor demand for lighting increases.

As suggested by a reviewer, the lump-sum payments to households in December 2009 and January 2011 might dampen the price incentive. This would suggest smaller price responses when those payments were more salient: i.e., the beginning and end of the trial. While we do see smaller responses in December 2010, this also coincides with the holiday season making it difficult to disentangle the two effects. We do not see smaller responses in January 2010, but this is also potentially confounded by a novelty effect at the start of the program. While the test far from perfect, there do not appear to be clear indicators of a dampening of the responsiveness coinciding with the timing of the payments.

One caveat must be made about the weekly estimates. The baseline period begins July of 2009 (during week 28). Therefore, the estimated weekly effects during weeks 1 to 27 (January-June) primarily rely on comparisons against the July-December baseline period. Hence, the effects estimated for the first half of the year (weeks 1 to 28) depend on somewhat different assumptions than the estimates for the second half of the year (weeks 29 to 53). In each case, the identifying assumption is the parallel trends assumption. The caveat is that the parallel trends assumption is more plausible in the second half of the year. For example the estimates for second half of the year compare, e.g., week 52 of 2010 to week 52 of 2009. The parallel trends assumption is more plausible in that case than it is for the first half of the year, when the identification comes from, e.g., comparing week 1 of 2010 to the average of weeks 28-53 of 2009. The identifying assumption of parallel trends would be satisfied if the mean change from weeks 28-53 of 2010 to week 1 of 2009 would be the same across the control and treatment groups, had it been untreated. While this is a stronger assumption, it is nonetheless plausible, lending credence to the estimates in Figure A.7.

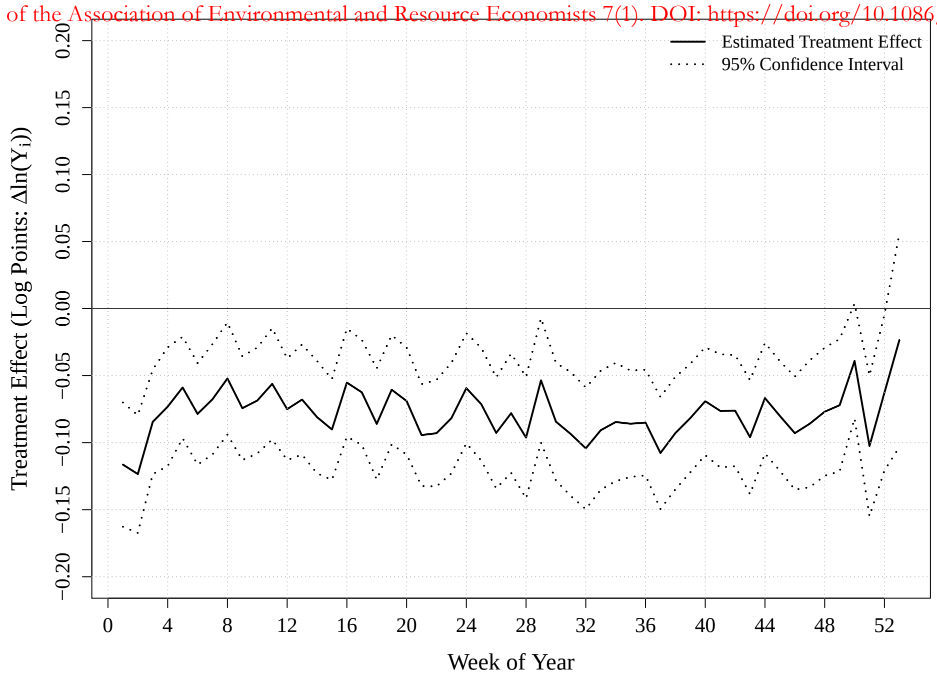


Figure A.7: Estimated Peak-Period Treatment Effects by Week of Year

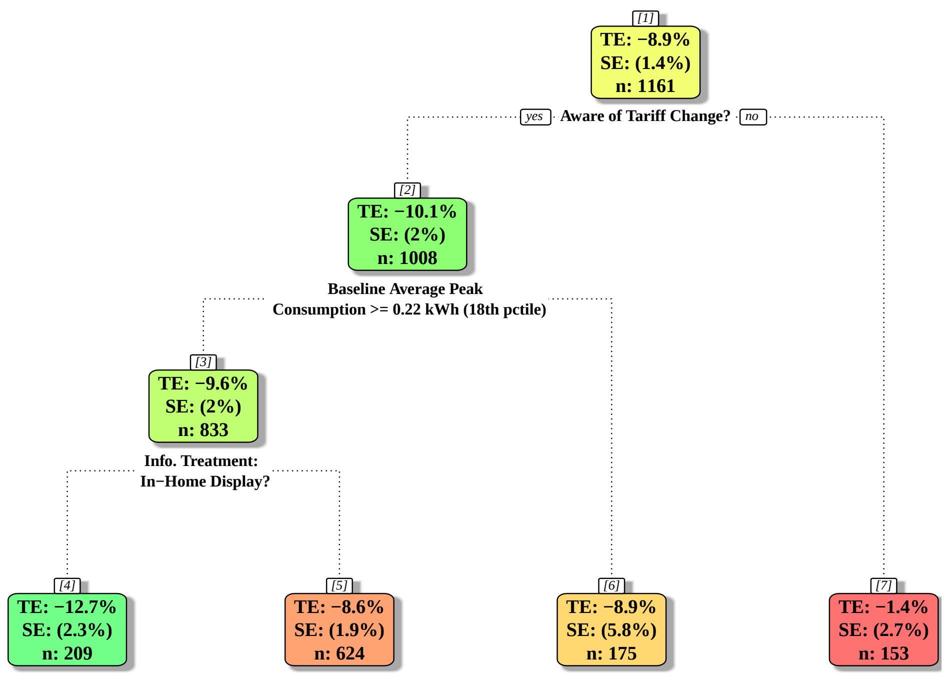
4.4 Honest Tree

The causal tree results in the body of the paper (shown in Figure 5) were estimated using the full dataset. While the size of the tree was determined through cross-validation, the same dataset was used to determine the nodes of the tree (i.e., split the data into subgroups) and estimate the treatment effects in those nodes. Athey and Imbens (2016) caution that building one's model and estimating parameters on the same data can possibly overstate the magnitude of the estimates, even if one regularizes with cross-validation. This is true of any flexible non-parametric estimation process, and it is distinct from standard overfitting (which is addressed through regularization and cross-validation).

To solve this problem, Athey and Imbens (2016) suggest a further extension in which one subsample of the data is used to build the tree and another one is used to estimate the treatment effects in each subgroup. In particular, the researcher first divides the dataset in two samples. Then, she uses one sample to estimate the structure of the tree (the nodes, or division into subgroups). The researcher uses cross-validation within this sample to determine the optimal size of the tree. So far, this process is identical to what was described in the body of the paper, but using only a subsample of the full dataset. The difference is that the researcher then ignores the estimated treatment effects in this half of the data. Instead, the researcher uses the structure of the tree determined on the first half of the data, but estimates the treatment effects in the corresponding nodes of the tree using only the yet-unused second half of the data. This provides a further safeguard that any resulting heterogeneity in estimated treatment effects is real, not an artifact of the data that survives the cross-validation process. Athey and Imbens (2016) call this an "honest" causal tree, finding it has superior confidence interval coverage in simulations.

As a robustness check, I estimate an honest causal tree. I divide the data in half and estimate the structure of the tree using cross-validation in that half. Holding the tree's structure fixed, I then use the other half of the dataset to estimate the treatment effects in each subgroup determined in the first step. The results for peak periods are shown in Figure A.8. The results from the honest tree confirm the main results: aware households are exhibit much larger reductions in peak consumption (-10% versus 1.4%).[10] After this, baseline consumption and information treatment matter, with larger households and households with the in-home display responding more.

This tree has two differences compared to the estimates on full dataset. First, the splits by baseline consumption are somewhat different: the honest tree chooses a somewhat higher cutoff for dividing aware households (0.22 versus 0.12 kWh), and it does not split unaware households at all. This difference is minor. Second, of the information treatments, only the in-home display split survives cross validation on this smaller sample. Nonetheless, the branches of the honest tree pruned by cross validation are the same as in the full-data tree. These branches (not shown) include a split on baseline consumption for unaware households and two splits below the in-home display node for the monthly bill treatment followed by the overall load reduction incentive treatment.



Notes: TE = Treatment effect (percent change in energy consumption); SE = standard error; n = number of treated observations in node. If the condition is satisfied, proceed down left branch. This figure was generated using the `rpart.plot` package (Milborrow 2016) in R.

Figure A.8: Honest Tree Treatment Effects During Peak Periods

[10] I also estimated honest trees for nighttime and daytime off-peak periods. Consistent with the results in the body, I find little-to-no heterogeneity in treatment effects during those periods.

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4.5 Checking Differential Responses by Tariff Group

Table A.2 shows the treatment effect by tariff group and pricing period (peak, day, and night). This confirms the implicit result from the causal tree that we similar responses across the four tariff groups. While the point estimates are somewhat larger for the at the higher tariffs, they are not significantly larger and the incremental reductions are small relative to the change in the prices.

Recall that the program intentionally assigned fewer households to tariffs B and D (compared to A and C; see Table 1). This leads to somewhat larger standard errors for those groups, as seen in the table. This is not simply driving the insignificant differences across tariffs: if the treatment effect for the smaller tariff groups were measured with the same precision as the larger ones, the confidence intervals would still overlap.

Table A.2: Estimated Treatment Effects by Tariff Structure

	Period of Day		
Dependent variable: ln(kWh)	Peak (1)	Day (2)	Night (3)
Tariff A (20/14/12 € cents/kWh)	−0.067***	−0.020**	−0.001
	(0.011)	(0.010)	(0.011)
Tariff B (26/13.5/11 € cents/kWh)	−0.084***	−0.014	0.009
	(0.015)	(0.013)	(0.015)
Tariff C (32/13/10 € cents/kWh)	−0.078***	−0.006	0.019*
	(0.012)	(0.010)	(0.011)
Tariff D (38/12.5/9 € cents/kWh)	−0.106***	−0.023*	0.023*
	(0.016)	(0.012)	(0.014)
F-test for equal treatment effects (p-value)	0.08	0.37	0.12
Observations (Household-Halfhours)	4,471,644	29,065,686	20,122,398
Observations (Households)	3,006	3,006	3,006
R^2	0.408	0.306	0.401

*p<0.1; **p<0.05; ***p<0.01

Note: All regressions include household and week-of-sample fixed effects. Prices listed in variable names are peak/daytime/nighttime tariffs. For example, Tariff A features a 20 € cents per kWh price during peak periods, 14 cents/kWh during off-peak daytime, and 12 cents/kWh at night. The control group (the omitted group) faced a constant price of 14.1 cents/kWh.

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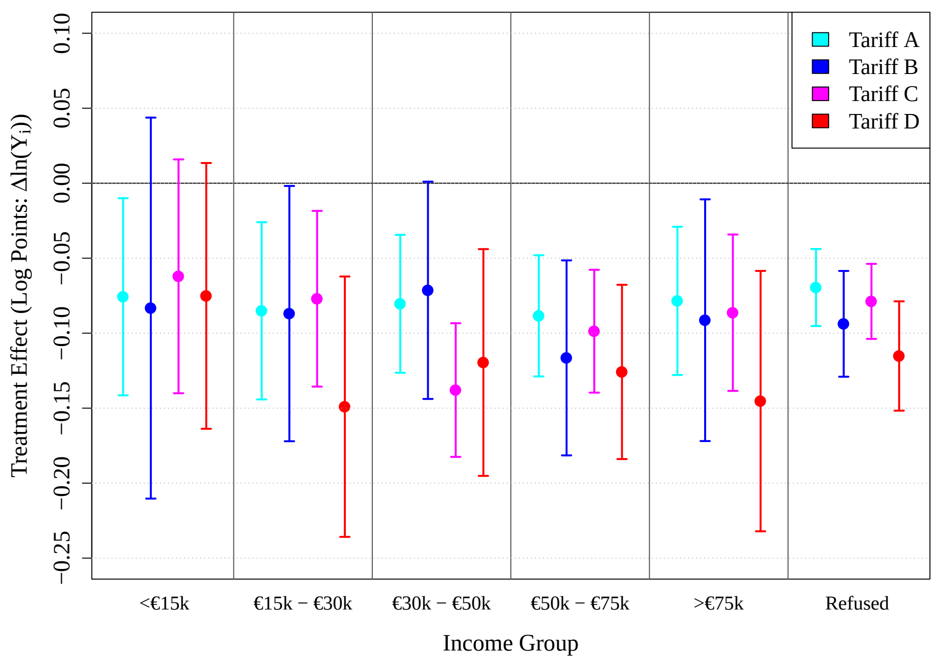
4.6 Checking Differential Price Sensitivity by Income

As suggested by a reviewer, it is possible that low-income households are more price sensitive than high-income ones. The survey data do include income groups in six bins, including "refused". About half (54%) of households refused to provide income data, followed by the 15% in the "€50k - €75k" range, 11% in "€30k - €50k" range, 8% in ">€75k", 7% in "€15k-€30k", and the remaining 5% in "<€15k". These shares are balanced across treatment and control. I have estimated the treatment effect separately for each of these groups and by tariff level, showing the results in Figure A.9 below (where the error bars represent 95% confidence intervals). The estimates are statistically noisy, but informative nonetheless. While for some income groups the point estimates appear to be larger for the highest tariff, these differences are not statistically significant. Further, for most groups the second highest tariff yields smaller responses than the immediately lower one. Together, these results do not provide strong evidence that price sensitivity varies strongly by income group.



Notes: Error bars represent 95% confidence intervals.

Figure A.9: Treatment Effects by Tariff Group and Income Bin

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4.7 Confirming No Heterogeneity on Appliances and Temperature

The causal trees find no robust sources of heterogeneity in average treatment effects by household appliance usage (or other household demographics). In this section, I confirm this by estimating effects separately by appliance ownership. I also estimate differences by temperature bins to consider the possibility that the lack of any clear differences by appliance ownership is masking differences that arise only at extreme temperatures. These effects are estimated using the same panel regression method, separately by appliance ownership and temperature bin. Temperature is measured as the wet bulb temperature in Phoenix Park, Dublin. I do not have location data at the household level, but the households in the sample are disproportionately located in Dublin (according to Commission for Energy Regulation (2011), more than 25% of households in the experiment are in Dublin). Temperature is binned into eight 3°C-wide intervals ranging from -4°C to 20°C, a histogram of which is shown in Figure A.10.

The temperature-binned treatment effects are shown in Figures A.11 and A.12. Figure A.11 shows no clear difference in the average effect across temperature bins. Figure A.12 shows this separately for homes with and without a variety of common appliances: electric home heating, electric water heating, electric cook stoves, dishwashers, and tumble dryers. There are generally no major differences in responses across these groups. The one exception is in the top right panel of Figure A.12, which shows a marginally significant difference of water heating, but only at very low temperatures. Nonetheless, this is driven by a smaller response (relative to warmer temperatures) among households without electric water heat, and not by a larger response (relative to warmer temperatures) among households with electric water heat. Indeed, among households with electric water heat, the response is no larger at cold temperatures than at warm ones. This is the opposite of what one might expect if the explanation was that those with electric water heaters respond more at cold temperatures. Altogether, these results suggest that the average response cannot be attributed to solely one margin. Instead, households appear to be responding in many ways. The evidence points towards water heating as perhaps a somewhat more important margin than others, but the statistical significance of this effect is marginal.

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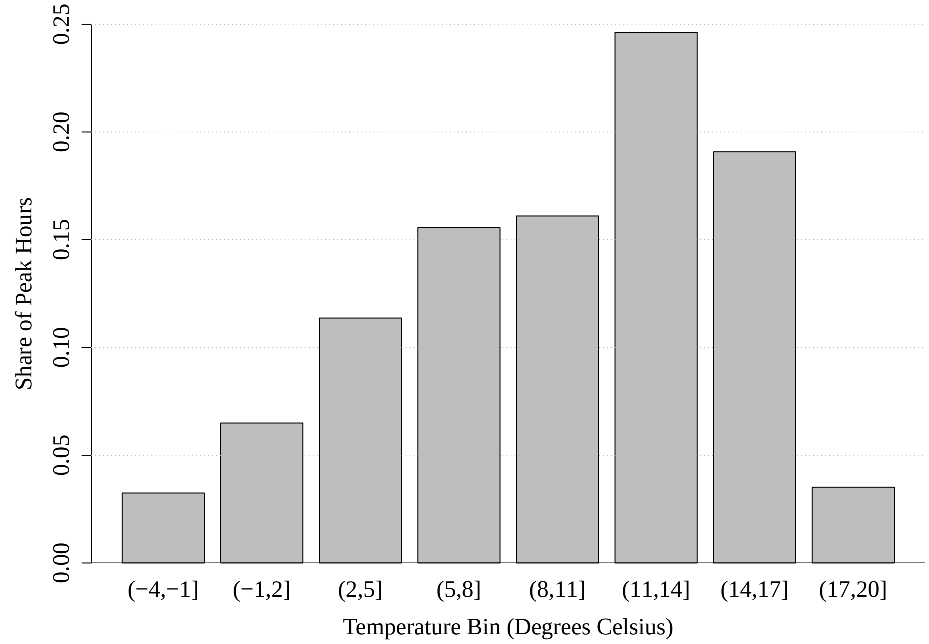

Figure A.10: Histogram of Temperature Bins

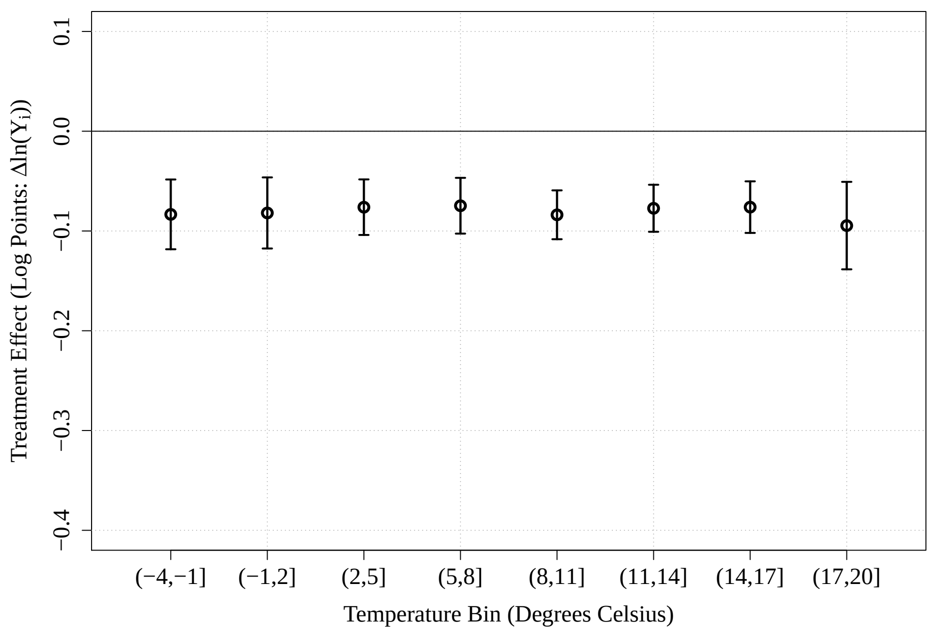

Figure A.11: Temperature-Binned Treatment Effects

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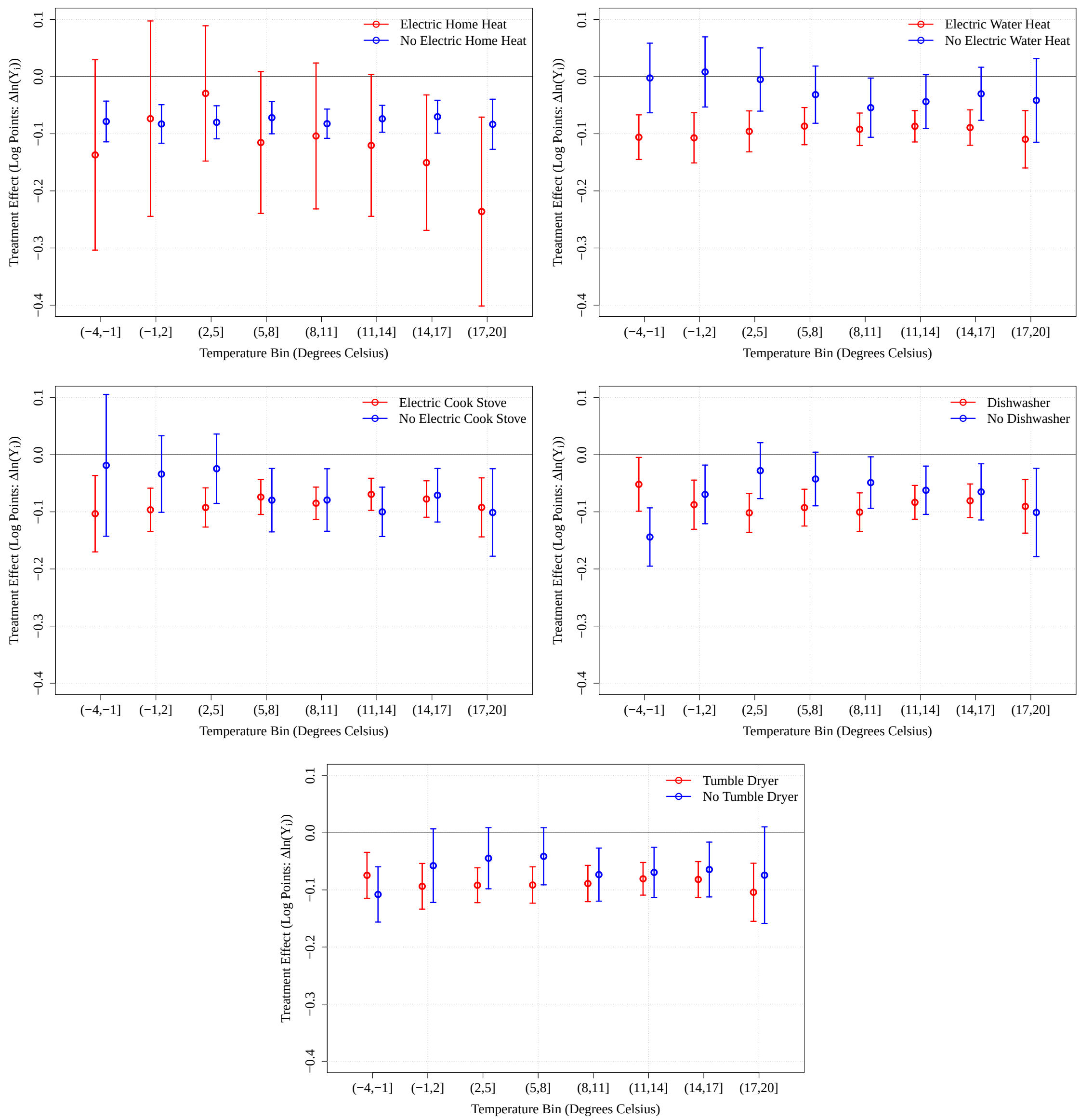

Figure A.12: Temperature-Binned Treatment Effects, by Appliance Ownership

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A.5 Real-Time Pricing Simulation

The body noted that real-time pricing can be inferior to TOU pricing (with respect to the goal of reducing peak load) in certain circumstances. To illustrate this point, I run a simulation of the effects of real-time pricing on consumption. I first estimate demand curves shown in Figure 6 separately for each "leaf" sub-group identified by the causal tree (Figure 5). These heterogeneous demand curves generally show similarly small and declining elasticities.[11] Using these demand curves, I simulate individual household consumption under three different kinds of pricing: flat pricing (14.1 cents per kWh), TOU pricing as implemented in this experiment, and real-time wholesale spot electricity prices in Ireland.[12]

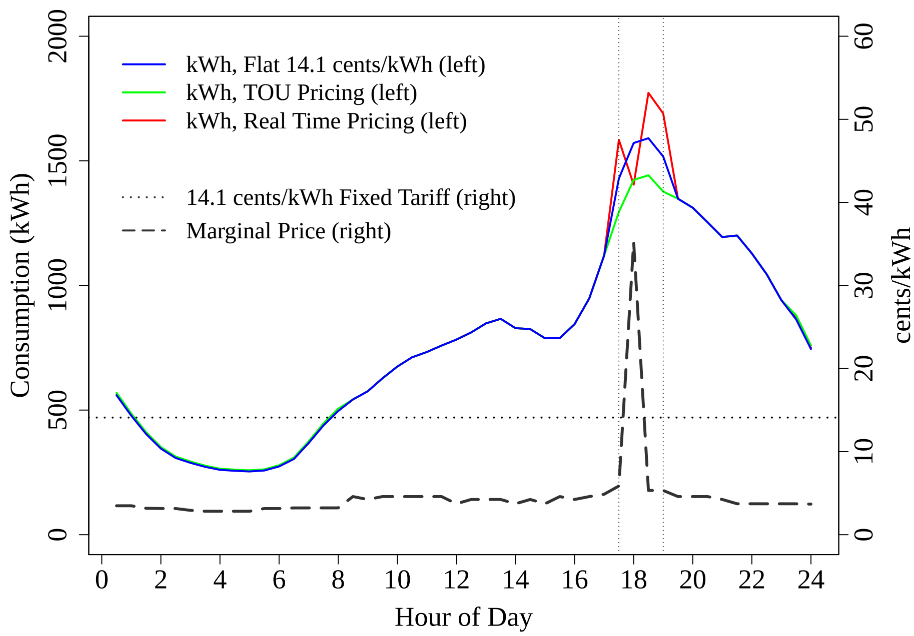

Figure A.13: Simulated Consumption under Different Pricing Regimes: January 4, 2010

Figure A.13 shows total consumption across all simulated households for a day with particularly high demand and spot prices: January 4, 2010. Focusing on the peak periods, we see that TOU pricing generally reduces consumption relative to flat pricing (green versus

[11]The exception is the group in node [10] which showed a statistically insignificant increase in consumption in response to higher prices, suggesting an upward-sloping demand curve. Because of the counterintuitive sign for this small subgroup, I treat this group as unresponsive for this simulation. I also treat daytime consumption as unresponsive, due to its similarly counterintuitive sign. Because the focus of this simulation is peak consumption, this choice is largely immaterial.

[12]Spot prices represent "EP2" final prices collected from `http://www.sem-o.com/marketdata/Pages/dynamicreports.aspx`. I use the shadow prices (without capacity uplifts) because that represents true marginal generation costs, but including the uplift has very little effect on this simulation. Average spot prices are lower than average retail prices because they do not include transmission and distribution charges. I effectively assume that those costs would be recovered through a fixed charge on households' bills and assume that households respond to marginal prices (as opposed to average prices).

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blue lines) across the entire pricing window, including the hour of highest consumption. This indicates that TOU pricing successfully reduces peak load.

By contrast, real-time pricing does not reduce peak load and may even increase it. Wholesale electricity prices spiked that day, and the simulation suggests that this would have reduced consumption during the high-priced period. However, the declining elasticity implies that the response to this price spike would be very similar to the response to standard TOU pricing. Further, while real-time prices did indeed spike on this peak-load day, they did not spike during the peak hour. Because of adjustments on the supply side, the period of peak demand is not always the same as the period of peak prices. In this example, during the period of peak demand, the market price was *lower* than the flat retail price. As a result, households would be charged lower prices during this period under real time pricing than they would under a flat tariff, resulting in even higher peak load. As a result, real-time pricing can actually be strictly worse than flat-rate pricing from the perspective of peak load.

Of course, Figure A.13 assumes that households would respond instantaneously to rapidly changing prices under a real-time pricing regime. This is clearly a strong assumption, but the alternative assumption of a sluggish response would make real-time pricing appear even less effective. Further, a more realistic assumption is that households respond less to real-time pricing than to predictable TOU pricing, since TOU prices are set and known in advance. In summary, the demonstrated insensitivity to the price level casts doubt on the benefits of real time pricing.

A.6 Welfare Impacts

In this section, I consider the costs to consumers and capacity-related benefits of switching from flat-rate pricing to time-of-use. The capacity benefit is the reduced need for utilities to build expensive peaking capacity, and it represents the primary benefit of TOU pricing.[13] The primary costs of the policy are the forgone consumption by households who reduce their peak consumption in the face of higher prices (as well as any adjustment costs), although this is partially offset by households enjoying somewhat more consumption at lower prices during off-peak hours.

The lost consumer surplus (CS) from TOU pricing is estimated to be fairly small when estimated as the "triangle under the demand curve": €3 on average per household per year on average, which ranges from €2 without an IHD to €4 with an IHD (approximately $2 to $5 per household per year, converted at the 2010 average exchange rate of $1.33 per € from Bloomberg L.P.). These losses are relatively small, but they also ignore any program implementation costs, including the purchase cost of an IHD.[14]

These estimated losses of €3 to €4 represent the "triangle cost", which is the sum of the hour-by-hour products of price changes and consumption changes, divided by two. The

[13] Another benefit is the reduced generation cost during peak periods, of course offset by increased generation costs resulting from increased consumption off-peak. I do not estimate energy savings because they are generally much smaller than the capacity benefit. For example, Fowlie et al. (2017) estimates that the energy-related benefits of TOU pricing in California are less than one-fifth the size of the capacity benefits.

[14] Unfortunately I do not have access to the cost of the IHDs used in this experiment. For reference, more advanced IHDs sold in the United States typically cost about $250 (see e.g., Bollinger and Hartmann (2016) and www.nest.com, which as of March 29, 2017 priced Nest thermostats at $249).

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triangle cost excludes the transfer from households to suppliers, in which households pay higher prices on inframarginal units of electricity consumption. Excluding such a transfer is typical because it is does not represent a true social cost. However, to gain a more nuanced view of the effect on consumers, I also estimate the full change in consumer surplus, which includes both the "triangle cost" as well as the transfer (the change in price multiplied by the *ex post* consumption). The effects on consumer surplus are estimated to be €18 to €19 per household per year. These effects are larger than the triangle cost, but they are nonetheless represent only a couple percentage points of the approximately €900 average annual Irish expenditure on electricity.[15].

Capacity benefits are estimated to be a one-time benefit of €100 per household in Ireland on average, ranging from €50 without an IHD to €130 with an IHD. The estimate of €50 assumes a weak information treatment (the bi-monthly bill), whereas the estimate of €130 assumes a strong one (an in-home electricity display, or IHD). The difference of €80 can be interpreted as the average capacity benefit of the IHD.[16]

More generally, capacity benefits are likely to be larger in regions where households consume more electricity such as the United States. This is because a fixed percentage reduction in peak demand will reduce more capacity (in kW) in high-consumption regions. Applying the same percentage reductions found in this experiment to peak demand levels consistent with the United States results in estimated benefits on the order of \$170 to \$430 per household (without and with an IHD, respectively, converted at the average 2010 exchange rate of \$1.33). In this case, the value of the IHD (the \$270 difference) is estimated to be larger because of the larger number of kilowatts reduced (even though the percentage difference is constant).

6.1 Costs: Effect on Consumers

Table A.3 shows estimates of the effect of TOU pricing on consumers. Three metrics are reported: the change in surplus excluding this transfer (reflecting true social cost), the change in consumer bills, and the change in consumer surplus including the transfer to suppliers.[17] The first value is most appropriate for a cost-benefit analysis, but the others are informative for consumer impacts. These different effects are illustrated conceptually by Figure A.14. The figure illustrates a price increase ($p_0 \rightarrow p_1$), but the computations are analogous for a price decrease ($p_1 \rightarrow p_0$), albeit with impacts of the opposite sign.

All effects were simulated using the heterogeneous impacts on peak pricing presented in Figure 5. For each half-hour in the treatment period, I simulate each treated household's counterfactual consumption (i.e., how much they would have consumed if they were not treated, using the relevant treatment effect given the household's characteristics, treatment

[15] See *The Irish Examiner*, "Electricity and gas costs up 200 in past year", June 2013.

[16] Annualizing these one-time benefits at a discount rate of 8% (derived below) suggests annualized benefits of €4 to €10 per household per year, which are larger than the estimated costs of €2 and €4, respectively.

[17] All estimates I present ignore the lump-sum payments given to households for their participation in the experiment. I ignore these payments because I want to isolate the impact of TOU pricing itself. These payments ranged from €55 to €140 in total: €50 compensation for participating in the surveys plus a payment of either €30, €50, €70, or €90 to households in the A, B, C, or D tariff groups. Accounting for these payments, the vast majority of households benefited from their participation in the experiment.

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group, and time of day). I then compute the change in the consumer's half-hourly electricity costs and consumer surplus (both with and without the transfer). The policy generally raises costs during peak hours and reduces them during off-peak hours.[18] These changes are summed to the annual level for each household. I then present the average of these values across households. The three columns in Table A.3 show the average across all treated households (first column) as well as averages for households in the weakest and strongest information treatments (bi-monthly bill and IHD, the final two columns).

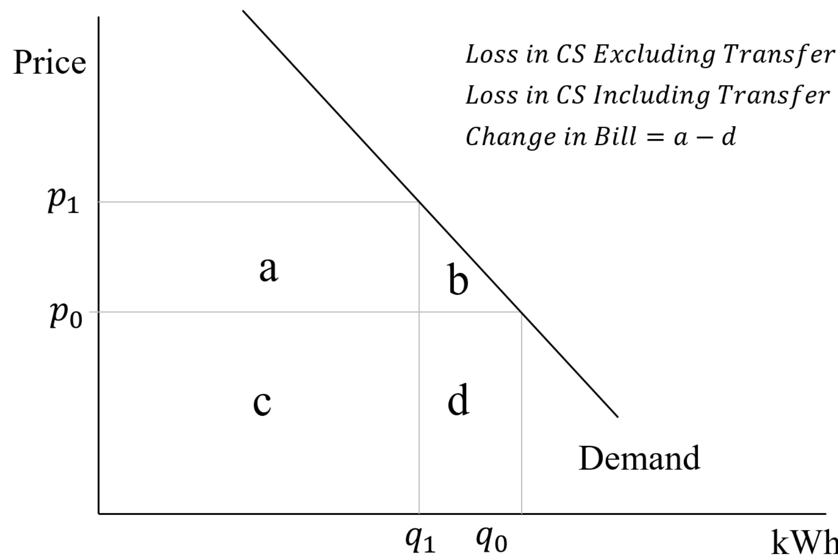

Figure A.14: Illustrative Diagram of Consumer Impacts of Price Change from p_0 to p_1

Table A.3: Effect on Treated Consumers during Treatment Period (Jan-Dec 2010)

	Average Household	Bi-Monthly Bill	IHD
Loss in CS Excluding Transfer (b)	**€2.68**	**€1.78**	**€3.61**
Loss in CS Including Transfer ($a + b$)	€18.64	€19.18	€17.66
Change in Bill ($a - d$)	€5.22	€8.61	€1.38

Notes: Letters in row labels refer to labeled areas in Figure A.14. For example, loss in CS including the transfer ($a + b$) is much larger than the increase in consumer bills ($a - d$) primarily because it also includes the value of the forgone electricity consumption (d).

The averages suggest relatively small net costs for consumers from this policy. The fact that there are net costs reflects the fact that added costs during peak hours exceed the benefits from off-peak hours on net. However, there is still substantial heterogeneity in households, with many households experiencing reduced bills due to the policy. The distributions of the

[18]For this reason, it is possible for the net cost to be negative, with the lower prices off-peak more than offsetting the higher on-peak prices.

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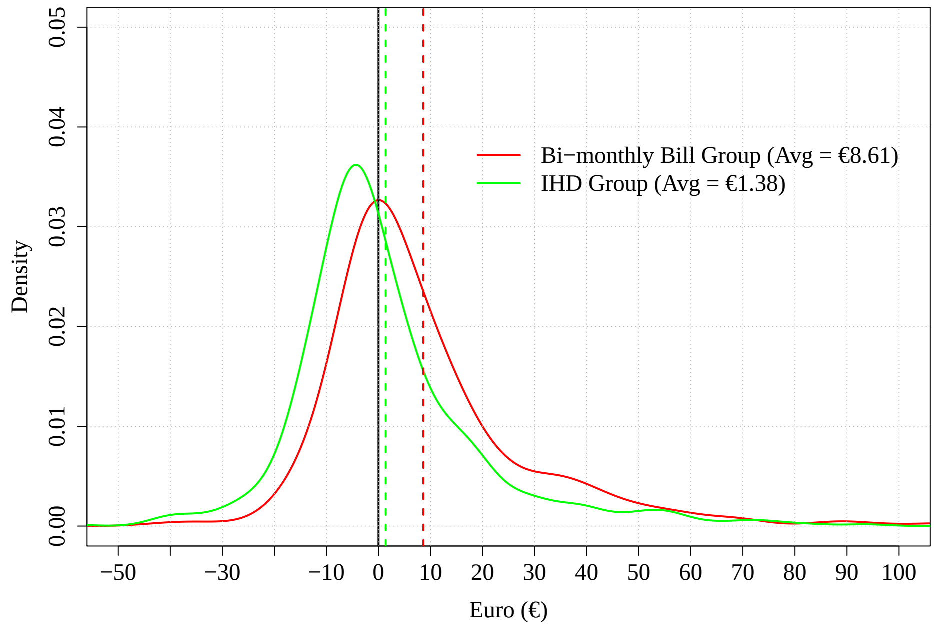

Figure A.15: Distribution of Effect of TOU Pricing on Household Bills in 2010, for Bi-Monthly Bill and IHD Treatment Groups

annual impacts on bills are shown in Figure A.15, separately for the two chosen information treatments. This reveals substantial heterogeneity, showing that TOU pricing could reduce bills for some households while increasing them for others (as much as 50%). Whether bills increase or decrease depends on the time profile of their consumption and their price responsiveness.

For a household to reduce its bill, it must reduce its electricity use during peak hours, forgoing consumption that has value to households. This explains why IHD households exhibit larger losses of consumer surplus (including the transfer) despite having lower bills: they are forgoing consumption, which is costly but shows up as a savings on their bills. It also explains the lower loss in consumer surplus (excluding transfer) for the bi-monthly bill group: they are not reducing as much, implying smaller lost value of foregone consumption. Their consumer surplus losses are mostly transfers to the utility.

6.2 Benefits: Capacity Savings from Reduced Peak Load

In this section, I estimate the avoided construction costs ("capacity benefit") associated with the effects from the treatments tested in this experiment. It should be noted that the following estimates are only approximate and should be considered to be "back of the envelope" due to the uncertainties in the various inputs and the rather strong assumptions required.

For purposes of external application, I estimate the per-household capacity benefit. I construct estimates based on three alternative estimated peak-period treatment effects: the average treatment effect (-8.9%), the treatment effect for the bi-monthly bill group (-4.8%),

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Table A.4: Estimated Benefits from Reduced Required Capacity

	Average Household	Bi-Monthly Bill	IHD
Ireland	€100	€50	€130
United States	$325	$170	$430

Notes: The values are estimated assuming the estimated treatment effect for each group: all households (-8.9%), the bi-monthly bill group (-4.8%), and the IHD group (-12.5%).

and the treatment effect for the IHD group (-12.5%).[19] This distinction allows for an estimation of the value added by the IHD. I also construct each estimate for Ireland as well as for the United States, where households have higher energy consumption and hence more potential for peak reductions. However, one should be cautious in interpreting the estimated U.S. benefits because it involves extrapolating my results from Ireland to a very different country, where electricity demand is driven by different factors. Nevertheless, some literature suggests comparable effects of dynamic pricing in the United States in a variety of settings, not only residential but also commercial and industrial (e.g., Jessoe and Rapson 2014; Bollinger and Hartmann 2016; Blonz 2016), so these estimates are likely of the correct order of magnitude.

The results are shown in Table A.4. All values have been rounded to the nearest €5 (or nearest $5 in the case of the United States). They suggest substantial benefits on the order of €50 to €130 in Ireland, depending whether an IHD is included. The corresponding figures for the United States are $170 to $430 per household, and on the region (Ireland versus United States). The IHD more than doubles the benefits because the estimated reduction on peak load is much larger with an IHD. The effects are larger in the United States—even on a per-household basis—because American households use more than twice as much electricity as Irish ones do, meaning equivalent percentage changes have larger effects in watts.

The additional capacity savings an IHD generates (€80 in Ireland and $270 in the United States) do not appear to justify the IHD's cost in Ireland but may narrowly do so in the United States. While I do not have access to the cost of the particular IHDs used in this experiment, similar smart thermostats and electricity monitors cost about $250 in the United States.[20] However, those devices generally have more functionality than the more simple IHDs used in this experiment, likely leading to larger benefits. This suggests the added value I estimate is a lower bound for the value of more advanced devices like smart thermostats.

Estimating Avoided Costs

The benefits in Table A.4 are computed as the construction costs avoided due to lower peak demand. TOU pricing reduces peak load, mitigating the need to build expensive peaking plants that are only activated a few times each year and otherwise sit idle. For each watt of

[19]These treatment effects differ from those shown in Figure 5 because those also condition on awareness and baseline energy consumption.

[20]See e.g., Bollinger and Hartmann (2016) and www.nest.com, which as of March 29, 2017 priced Nest thermostats at $249.

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peak load that is reduced, utilities can reduce investment in an additional watt of peaking capacity. This implies that the costs avoided due to a successful TOU pricing program are simply the product of the reduction in peak load (in watts) due to the program and the per-watt cost of building and maintaining a peaker plant.

For my estimate of the cost of capacity, I use the average of estimates from reports by the EIA and Brattle Group, resulting in an estimated cost of \$1,057,000 per MW, or \$1.057 per watt.[21] In addition, there are fixed operation and maintenance (O&M) costs required to keep the plant in operating condition, which I estimate to be \$8.76 per kW-year,[22] which is approximately \$86 per kW (or about \$0.086 per watt) in present value assuming a discount rate of 8% and 20 year plant life,[23]. Adding this to the construction costs yields a total NPV avoided cost of \$1.14 per watt of peak load reduced.[24] In order to compare the benefits to the costs to Irish households (which are computed in Euro), I convert this value to Euros at the average 2010 exchange rate of €0.75 per dollar, yielding a cost per watt of €0.86. For the United States calculation, I use the value in dollars.

The reduction in peak consumption from the program is also straightforward to calculate. Peak load for the entire Ireland electricity grid during the 2009-10 winter occurred at 5:30-6:00pm on Thursday, January 7, 2010. During that half hour, average household load among treated households treatment group was approximately 1.2 kW. As previously shown, the average effect on peak load is approximately -8.9%. For those with the weakest information treatment—the bi-monthly bill—the effect was -4.8%, compared to -12.5% for those who received an IHD.[25]

Using these three estimated treatment effects, the estimated reductions in peak load are 0.11 kW (average), 0.06 kW (without an IHD), and 0.15 kW (with an IHD).[26,27] At a value of €0.86 (derived above, and equivalent to \$1.14 per watt), this implies a benefit of approximately

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[21] Capital costs are the average of estimated costs by EIA (Table 1, Conventional CT) and Brattle Group (Table 1, "Installed" cost). EIA costs are in 2012\$ whereas Brattle estimates are in 2018\$, assuming a 3% inflation rate. Therefore, I escalated EIA figures at 3% to 2018 to be comparable with Brattle's estimates.

[22] Based on EIA (ibid.), Table 1, Conventional CT, Fixed O&M, adjusted to 2018\$ at 3% annually, as described in the previous footnote.

[23] I use the 8% discount rate suggested by the Brattle Group (same source as above, Table 25). I assume a 20 year economic life, also from Brattle (page iv).

[24] This estimate is comparable to the estimate of \$1.19 per watt used in Blonz (2016).

[25] This effect on peak consumption appears to be even larger during the peak day of January 7, 2010 (-22% on average that day, compared to -8.9% for the full year). To be conservative, I use the annual average treatment effect. The very large estimated treatment effect during the first few weeks of the experiment may owe to the fact that the treatment had just begun and was fresh in participants' minds. Such large treatment effects are unlikely to be sustainable in the long run, and indeed the effect stabilized soon after around its average level, as seen in Figure A.7.

[26] This is computed as the observed treated peak consumption in the sample, scaled up to the untreated counterfactual level (e.g., $\frac{1.2\text{ kW}}{1-0.089}$ for the average effect), multiplied by the treatment effect (e.g., -0.089).

[27] One may be concerned that this may overstate the reduction in peak demand. In particular, large reductions during peak periods may be offset by shifting to off-peak times. If either effect is substantial enough, TOU pricing could simply change the timing of the peak, rather than its overall level. The data suggests this is not the case, as shown by Figures 2 and 3. Figure 2 shows no statistically significant increase in consumption except for a small increase during 11:00pm-12:30am, which generally features the lowest load and is not at risk of becoming the new peak. Figure 3 shows no shifting of the peak hour on average; instead it suggests a general reduction in load in the key 5pm-10pm window. This suggests that shifting of the peak to other times is not a problem.

€100 per household (≈€0.86 per watt $\times$ 110 watts) on average. For households without IHD, the benefit is €50 per household (€0.86 $\times$ 60 watts), or €130 with an IHD (€0.86 $\times$ 150 watts).

Extrapolating these figures to the United States raises several issues. First, American households may respond differently than Irish households for a variety of reasons, including differences in the drivers of peak demand and differences in income. Evidence from the United States (e.g., Blonz 2016; Jessoe and Rapson 2014) suggests effects of similar magnitude, implying this approximation is reasonable. A second difference is that Irish households use much less electricity than U.S. households do (approximately 60% less). This means that for a given percentage change in consumption, the capacity savings (in watts) would be larger for American households—perhaps more than twice as large. I account for this in my estimates through an adjustment based on U.S. electricity demand.

With these issues in mind, I present a rough order of magnitude of the potential benefits of TOU pricing to American utilities. Assuming American households respond at similar percentages as the Irish households do, the capacity savings are estimated to be \$325 on average, \$170 per household without an IHD, and \$430 per household with an IHD. These figures are simply the Irish per-household benefits (€100, €50, and €130 respectively) converted to Euro at the 2010 exchange rate (\$1.33 per Euro) and scaled by the ratio of U.S. and Irish electricity consumption, which is approximately 2.5.[28]

[28] In the United States in 2015, average annual residential electricity consumption is approximately 11,270 kWh per household (based on EIA and Census data). According to the Sustainable Energy Authority of Ireland, the corresponding value for Ireland is 4,470 kWh per household. The ratio of these values is 2.5.

A.7 List of Survey Questions Used

Table A.5 displays a summary of the survey variables used in the analysis. This list shows 85 survey questions. The total number of variables used in the analysis is larger than this for several reasons. First, many of the variables are factor variables, which were converted to sets of indicator variables for each level (for example, employment status is a factor variable with seven levels). Second, questions asked in both surveys appear twice, once for each survey, to capture potential changes in status. Third, I also use variables representing treatment groups, which do not appear in the survey. Fourth, the smart meter data also provides other non-survey data. I only have listed the survey questions I use (others were incomplete).[29] Variables measured on Likert scales (i.e., agree/disagree or satisfied/dissatisfied on a scale of 1-5) are treated as numeric in the analysis.

Table A.5: List of Survey Questions Used

	Question	Variable Type	Data Source
	Socio-demographics		
1	Gender (recorded from voice)	Binary	Both surveys
2	May I ask what age you were on your last birthday? 18-25; 26-35; 36-45; 46-55; 65+.	Ordered factor	Both surveys
3	Moving on to education, which of the following best describes the level of education of the chief income earner? None; Primary; Secondary to Cert; Secondary without Cert; Third.	Factor	Initial survey
4	How many people in your household work for pay?	Integer	Follow-up survey
5	What is the employment status of the chief income earner in your household, is he/she: An employee; Self-employed (with employees); Self-employed (with no employees); Unemployed (actively seeking work); Unemployed (not actively seeking work); Retired; Carer (Looking after relative or family).	Factor	Both surveys
6	What is the occupation of the chief income earner in your household? [coded to Irish social class: AB; C1; C2; DE; Farmer]	Factor	Both surveys
7	How many people over 15 years of age live in your home?	Integer	Both surveys
8	How many people under 15 years of age live in your home?	Integer	Both surveys
9	How many total people live in your home?	Integer	Computed
10	What best describes the people you live with? Live alone; Multiple adults; Both adults and children	Factor	Both surveys
11	Have you had to go without heating during the last 12 months through lack of money?	Binary	Both surveys
	Housing Characteristics		
12	I would now like to ask some questions about your home. Which best describes your home? Apartment; Bungalow; Detached; Semi-detached; Terraced.	Factor	Initial survey
13	Approximately how old is your home? 0-5 years; 10-30 years; 30-75 years; 75+ years.	Factor	Initial survey
14	Do you own or rent your home? Own outright; Own with mortgage; Rent (public); Rent (private); Other.	Factor	Both surveys
15	And now considering energy reduction in your home please indicate the approximate proportion of light bulbs which are energy saving (or CFL)? None; about a quarter; about half; about three quarters; all.	Ordered factor	Initial survey
16	Please indicate the approximate proportion of windows in your home which are double glazed? None; about a quarter; about half; about three quarters; all.	Ordered factor	Initial survey
17	Is your attic insulated and if so when was the insulation fitted?	Ordered factor	Initial survey
18	Returning to heating your home, in your opinion, is your home kept adequately warm?	Binary	Both surveys
19-28	Over the last twelve months have you done any of the following?		
19	Added double glazing to some or all of your windows	Binary	Follow-up survey
20	Installed insulation to your home (attic or walls)	Binary	Follow-up survey

[29] Full lists of all questions asked are available from ISSDA, here and here.

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21	Replaced appliances with A rated ones	Binary	Follow-up survey
22	Fitted a new lagging jacket on your hot water tank	Binary	Follow-up survey
23	Fitted other energy saving devices - such as usage monitors	Binary	Follow-up survey
24	Added solar panels	Binary	Follow-up survey
25	Added draught-proofing to your doors or windows	Binary	Follow-up survey
26	Replaced a central heating boiler with a more efficient one	Binary	Follow-up survey
27	Added thermostatic controls to radiators so that their temperature could be turned down when the room is not in use	Binary	Follow-up survey
28	None of these	Binary	Follow-up survey
	Appliances & Electronics		
29	Do you have internet access in your home?	Binary	Initial survey
30	How many electric showers are in your home?	Integer	Computed
31	Do you have a timer to control when your hot water/immersion heater comes on and goes off?	Binary	Initial survey
32	Does your hot water tank have a lagging jacket?	Binary	Initial survey
33	Which of the following best describes how you cook in your home? Electric cooker; Gas cooker; Oil fired cooker; Solid fuel cooker; Microwave; Don't cook.	Factor	Both surveys
35-40	Which of the following best describes how you heat your home?		Initial survey
34	Electricity (electric central heating/storage heating)	Binary	Both surveys
35	Electricity (plug in heaters)	Binary	Both surveys
36	Gas	Binary	Both surveys
37	Oil	Binary	Both surveys
38	Solid fuel	Binary	Both surveys
39	Renewable (e.g. solar)	Binary	Both surveys
40	Other	Binary	Both surveys
41	Do you have a timer to control when your heating comes on and goes off?	Binary	Initial survey
42	How many appliances do you own?	Integer	Computed
43-52	Please indicate how many of the following appliances you have in your home?		
43	Washing machine	Integer	Initial survey
44	Tumble dryer	Integer	Initial survey
45	Dishwasher	Integer	Initial survey
46	Electric shower (instant)	Integer	Initial survey
47	Electric shower (electric pumped from hot tank)	Integer	Initial survey
48	Electric cookers	Integer	Initial survey
49	Electric heaters (plug-in convector)	Integer	Initial survey
50	Stand alone freezer	Integer	Initial survey
51	Water pump	Integer	Initial survey
52	Immersion heater	Integer	Initial survey
53	How many entertainment appliances do you own and actually use?	Integer	Computed
54-58	And how many of the following entertainment appliances do you have? Only those that are actually used should be mentioned?		
54	TVs less than 21 inches	Integer	Initial survey
55	TV's greater than 21 inches	Integer	Initial survey
56	Desk-top computers	Integer	Initial survey
57	Lap-top computers	Integer	Initial survey
58	Game consoles (such as Xbox, PlayStation, or Wii).	Integer	Initial survey
	Attitudes & Behavior		
59	Do you use the internet regularly yourself?	Binary	Initial survey
60	Are there other people in your household that use the internet regularly?	Binary	Initial survey
61-67	And now, I would like to ask you a few questions about your general attitudes towards energy, electricity use and the electricity bill. [do you agree or disagree]:		
61	I/we am/are interested in changing the way I/we use electricity if it reduces the bill	Agree-Disagree, (1-5)	Initial survey
62	I/we am/are interested in changing the way I/we use electricity if it helps the environment	Agree-Disagree, (1-5)	Initial survey
63	I/we can reduce my electricity bill by changing the way the people I/we live with use electricity	Agree-Disagree, (1-5)	Initial survey
64	I/we have already done a lot to reduce the amount of electricity I/we use	Agree-Disagree, (1-5)	Initial survey
65	I/we have already made changes to the way I/we live my life in order to reduce the amount of electricity we use.	Agree-Disagree, (1-5)	Initial survey

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66	I/we would like to do more to reduce electricity usage	Agree-Disagree, (1-5)	Initial survey
67	I/we know what I/we need to do in order to reduce electricity usage	Agree-Disagree, (1-5)	Initial survey
68	Does your home have a Building Energy Rating (BER) - a recently introduced scheme for rating the energy efficiency of your home?	Binary	Initial survey
69-72	Which of the following do you think will be benefits [of the your participation in the trial]?		
69	Learn how to reduce my energy usage	Binary	Initial survey
70	Learn how to reduce my electricity bill	Binary	Initial survey
71	Do my part to help the environment by my participation	Binary	Initial survey
72	Do my part to make Ireland become more up to date	Binary	Initial survey
73-76	Thinking of what will be the main consequences of your participation in the trial, for each of the following statements, [do you agree or disagree]:		
73	My household may decide to make minor changes to the way we use electricity	Agree-Disagree, (1-5)	Initial survey
74	My household may decide to make major changes to the way we use electricity	Agree-Disagree, (1-5)	Initial survey
75	My household may decide to be more aware of the amount of electricity used by appliances we own or buy	Agree-Disagree, (1-5)	Initial survey
76	In future, when replacing an appliance, my household may decide to choose one with a better energy rating	Agree-Disagree, (1-5)	Initial survey
77	How do you think that your electricity bills will change as part of the trial? Increase; No Change; Decrease.	Ordered factor	Initial survey
78-83	Thinking about electricity and its use, generation and sale in the Irish context, please indicate your level of satisfaction with each of the following were 1 is very satisfied and 5 is very dissatisfied:		
78	The number of suppliers competing in the market	Satisfied-Dissatisfied (1-5)	Initial survey
79	The percentage of electricity being generated from renewable sources	Satisfied-Dissatisfied (1-5)	Initial survey
80	The overall cost of electricity	Satisfied-Dissatisfied (1-5)	Initial survey
81	The number of estimated bills received by customers	Satisfied-Dissatisfied (1-5)	Initial survey
82	The opportunity to sell back extra electricity you may generate (from solar panels etc) to your electricity supplier	Satisfied-Dissatisfied (1-5)	Initial survey
83	The environmental damage associated with the amount of electricity used	Satisfied-Dissatisfied (1-5)	Initial survey
84	Our society needs to reduce the amount of energy we use	Agree-Disagree, (1-5)	Follow-up survey
85	As part of the trial, the way you were charged for the electricity you used was changed from a single rate for all electricity to one that varies by time of day. Were you aware of this?	Binary	Follow-up survey

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