The Short-Run and Long-Run Effects of Behavioral Interventions: Experimental Evidence from Energy Conservation

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Abstract

Interventions to affect repeated behaviors, such as smoking, exercise, or workplace effort, can often have large short-run impacts but uncertain or disappointing long-term effects. We study one part of a massive set of randomized control trials in which home energy reports containing personalized feedback, social comparisons, and energy conservation information are being repeatedly mailed to more than five million households across the United States. We show that treatment group households reduce energy use within days of receiving each of their first few reports, but these initial responses decay rapidly in the months between reports. This cyclical pattern of stimulus and response attenuates as reports are repeatedly delivered and households form a new "capital stock" of physical capital or consumption habits. When a randomly-selected group of households has reports discontinued after two years, the treatment effects decay much more slowly than they had between the initial reports. We show how assumptions about long-run persistence can significantly impact program adoption decisions, and we illustrate how program design that accounts for this capital stock formation process can significantly improve cost effectiveness.

JEL Codes: D03, D11, L97, Q41.

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1 Introduction

While some of the decisions people make are occasional, such as buying a car or enrolling in a retirement savings plan, many of our choices are constantly repeated, such as whether or not to smoke, exercise, eat healthfully, work or study hard, and pay bills on time. Sometimes, our choices differ from those that would maximize social welfare, or perhaps even our own long-run welfare. In an attempt to "improve" behaviors, individuals, employers, government agencies, and parents have experimented with many different kinds of interventions, such as financial incentives, information provision, commitment contracts, appeals to the public good, and social comparisons. In an effort to produce useful and timely insights, evaluations often only examine short-run effects. The studies that do examine long-run effects often find that it is very difficult to sustainably change behaviors.[1]

Given this, several related questions are crucial to designing and evaluating programs to affect repeated behaviors. First, is it helpful to repeat an intervention, or do responses eventually attenuate? Second, how persistent are effects after the intervention ends? Third, do longer interventions cause more persistent post-intervention effects? These questions often determine whether an intervention is cost-effective, and understanding the answers can help optimize program design. They also provide deeper insight into the mechanisms through which an intervention affects our behaviors.

In this paper, we study the short-run and long-run effects of a program that provides "Home Energy Reports" featuring personalized feedback, social comparisons, and energy conservation information. The reports are mailed to households monthly or every few months for an indefinite period of time. The intervention, which is managed by a company called Opower, is typically implemented as a randomized control trial, giving particularly credible estimates of its effects on energy use. Opower's programs have been implemented at 70 utilities across the United States, and there now 8.4 million households in treatment and control groups - one out of every twelve households in the country. Utilities hire Opower to implement the intervention primarily because the resulting energy savings help to comply with state regulations requiring energy conservation.

We analyze one Opower program that uniquely combines three features. First, the program

[1] For example, see Cahill and Perera (2009) for a review of the long-run effects of interventions to encourage smoking cessation, as well as a number of studies of exercise, weight loss, school performance, and other behaviors that we discuss later in the introduction.

has been running continuously since October 2008, allowing us to assess the durability of effects over a relatively long period. Second, a subset of treatment group households were randomly selected to be dropped from treatment after two years, allowing us to measure the persistence of effects for an additional two years after the intervention stops. Third, while most utilities manually record household electricity use on a monthly basis, this utility uses advanced meters that record consumption each day. Although in recent years, millions of households have been outfitted with similar "smart meters" (Joskow 2012, Joskow and Wolfram 2012), the granularity of these data has generated privacy concerns that make it difficult to acquire them for research. In total, we have just over 200 million observations of daily energy use over six years at 122,000 households. With these data in hand, we can analyze the intervention's effects in remarkable detail.

Our analysis shows that the treatment effects wax and wane with the stimulus of initial reports: people reduce energy use markedly within days of when the reports arrive in their mailboxes, but these initial responses decay away relatively quickly. The decreases in the daily flows of electricity use within ten days of receiving the initial four reports add up to well over 100 percent of the average daily flow of savings over the first year. This is mathematically possible only because consumers "backslide": their conservation efforts decay at a rate that, if continued, might cause the treatment effects to disappear in well under a year. We also show that the autocorrelation of consumption decreases immediately after report arrivals is significantly higher in treatment than in control. This is important because it means that the repeated jumps in the average treatment effect upon arrival of the initial few reports are not simply due to new households opening reports for the first time: instead, some of the same households are repeatedly having their attention re-directed to energy conservation.

Interestingly, this cyclical response to the stimulus of receiving reports attenuates after the first few reports: the immediate consumption decreases become much smaller, and the decay rate between reports becomes statistically indistinguishable from zero. What remains is a highly durable treatment effect: as long as the program is continued over our four-year sample, the effects continue to increase. The effects are 28 percent larger in the third and fourth years of treatment than in the second year. Furthermore, when the reports are discontinued for some households after two years, the treatment effects are quite persistent. In fact, the long-run decay rate after two years of treatment is about six to twelve times slower than the decay between the initial reports. These results imply that as the intervention is repeated, people gradually develop a new "capital stock" that causes persistently lower energy use. This capital stock might be physical capital, such as new lightbulbs or automatic thermostats, or "consumption capital" - a stock of energy use habits in the sense of Becker and Murphy (1988).

Tangibly, what are consumers doing in response to the intervention? We show that the changes involve a combination of heating, cooling, and other household energy uses: the treatment effects on temperate days, as well as the slope of the relationship between weather and the treatment effects, both grow continually over the four years of the program. We show that the intervention does not induce many large-scale changes to physical capital stock: the utility subsidizes and tracks major household energy efficiency investments such as purchase of energy efficient washing machines and refrigerators, and the differences between treatment and control are not statistically or economically significant. Interestingly, however, treatment and control households also have the same probability of reporting that they have engaged in a broad swathe of energy conservation behaviors over the past year. Although these self-reports should be interpreted cautiously, they suggest that some of the behavior changes are on the "intensive margin," by which we mean that the program motivates households to do more of the same things that they already were doing. Taken together, the evidence suggests that households respond to the reports in a variety of different ways, perhaps including small capital stock changes and consistently more conservative utilization of various elements of that capital stock.

After presenting the empirical results, we turn to the economic implications. First, we carry out a simple cost effectiveness analysis covering only the sample period. In this site, the observed cost effectiveness of two years of treatment followed by two years of gradually-decaying effects is 2.15 cents per kilowatt-hour (kWh) of electricity conserved. This compares favorably to other energy conservation programs, which have cost effectiveness estimated at 5.0 cents/kWh (Arimura *et al.* 2011) and between 1.6 to 3.3 cents/kWh (Friedrich *et al.* 2009). However, in the absence of perfect foresight, a policymaker deciding whether or not to implement the Opower program could have come to very different conclusions depending on what she assumed about persistence. A conservative policymaker assuming zero persistence would have expected cost effectiveness to be

4.42 cents/kWh, which is above Friedrich *et al.*'s upper bound. By contrast, full persistence would have implied 2.07 cents/kWh.

Second, we impose additional out-of-sample assumptions in order to demonstrate the importance of persistence for program design. We predict that a one-shot intervention would have cost effectiveness of 4.61 cents/kWh, and the effects would decay to zero within less than six months. The incremental effects of repeated interventions can be conceptually decomposed into two parts: an *intensity effect*, through which repeated intervention may induce households to undertake more of the same behaviors, and a *composition effect*, through which repeated intervention may induce households to respond in more persistent ways, for example by changing physical capital or consumption habits. In this context, exploiting the composition effect - repeating treatment until the effects become more persistent - dramatically improves predicted cost effectiveness, to better than 1.9 cents/kWh. Once the composition effect is exhausted, incremental treatment is still predicted to be highly cost-effective, but this is because of the intensity effect: treatment effects continue to increase as treatment is continued.

Of course, the treatment effects and cost effectiveness estimates are specific to this program in this location. We believe that two basic implications generalize outside of this context. First, short-run and long-run effects can differ substantially, and persistence can significantly influence cost effectiveness - and thus program adoption decisions. Thus, it can be important to measure long-run effects when evaluating programs that might be replicated or scaled up, even if this delays the decision-making process. Second, understanding the dynamics of "capital stock formation" in each context can be very important for program design. In general, it may be optimal to repeat an intervention until participants have solidified a new stock of habits or other technologies that make the behavior changes persistent. After that point, it may be optimal to reduce the frequency of interventions, unless incremental treatment continues to induce additional capital stock changes.

Our results are related to several different literatures. The cyclical response to the stimulus of home energy reports is reminiscent of evidence that consumers "learn" about late fees and other charges as we incur them, but we act as if we forget that knowledge over time (Agarwal *et al.* (2011), Haselhuhn *et al.* (2012)). For some consumers, the home energy report acts simply as a reminder to conserve energy, making this related to studies of reminders to save money (Karlan, McConnell, Mullainathan, and Zinman 2010) or take medicine (Macharia *et al.* 1992). There are also studies of the long-run effects of other interventions to affect exercise (Charness and Gneezy 2009), smoking (Gine, Karlan, and Zinman 2010, Volpp *et al.* 2009), weight loss (John *et al.* 2011), water conservation (Ferraro and Price 2011), academic performance (Jackson 2010, Levitt, List, and Sadoff 2010), voting (Gerber, Green, and Shachar 2003), charitable donations (Landry *et al.* 2010), labor effort (Gneezy and List 2006), and other repeated choices. Two key distinguishing features of our study are our exceptionally long time horizon and our high-frequency outcome data. These allow unusual insight into how our behaviors wax and wane with repeated stimuli and how effects can become more persistent over time.

Aside from being of scientific interest, these results have very concrete practical importance. Each year, electric and natural gas utilities spend several billion dollars on energy conservation programs in an effort to both reduce energy use externalities and ameliorate other market failures that affect investment decisions for energy-using durable goods (Allcott and Greenstone 2012). Traditionally, one significant disposition of these funds has been to subsidize energy efficient investments, such as Energy Star appliances or home energy weatherization. Recently, there has been significant interest in "behavioral" energy conservation programs, by which is meant information, persuasion, and other non-price interventions.[2] The Opower program is perhaps the most salient example of this approach. One of the foremost questions on practitioners' minds has been the extent to which behavioral interventions can reduce energy use over the long run: while capital stock changes like new insulation are often believed to have persistent long-run effects, it was not obvious what would happen after several years of home energy reports. Our results give some initial evidence on this issue.

The paper proceeds as follows. Section 2 gives additional background on the program and describes the data. Sections 3 presents short-run analysis using high-frequency data, while Section 4 presents the long-run analysis. Section 5 discusses some additional evidence on the channels through which the intervention acts. Section 6 carries out the cost effectiveness analysis, and Section 7 concludes.

[2] Abrahamse *et al.* (2005) is a useful literature review of behavioral interventions centered around energy conservation, and Allcott and Mullainathan (2010b) cite some of the more recent work.

2 Experiment Overview

2.1 Background

Aside from selling energy, most electric and natural gas utilities in the U.S. also run energy conservation programs, such as home energy audit and weatherization programs and rebates for energy efficient light bulbs and appliances. While energy conservation can reduce revenues for private investor-owned utilities, many states require utilities to fund conservation programs out of small surcharges called System Benefits Charges, and in recent years many states have passed Energy Efficiency Resource Standards that require utilities to cause consumers to reduce energy use by some amount relative to counterfactual, often 0.5 to 1 percent per year.

Opower is a firm that contracts with utilities to help achieve these energy conservation requirements. Their "technology" is unusual: instead of renovating houses or subsidizing energy efficiency, they send two-page Home Energy Report letters to residential consumers every month or every several months. Figure 1 reproduces a home energy report for an example utility. The first page features a "Social Comparison Module," which compares the household's energy use to that of 100 neighbors with similar house characteristics. The second page includes more personalized energy consumption feedback and an "Action Steps Module," which provides energy conservation tips. The exact content of the reports varies over time.

The initial proof of concept that social comparisons could affect energy use was developed in pair of papers by Nolan *et al.* (2008) and Schultz *et al.* (2007). There is also a body of evidence that social comparisons affect choices in a variety of domains, such as voting (Gerber and Rogers 2009), retirement savings (Beshears *et al.* 2012), and charitable giving (Frey and Meier 2004).

Building on these initial studies, nearly all of Opower's programs have been implemented as randomized control trials (RCTs), with report recipients randomly selected from a population of residential consumers. This means that it is straightforward to evaluate the effects on energy use. Allcott and Mullainathan (2012) show that the average treatment effects across the first 14 Opower sites range from 1.4 to 2.8 percent. Opower programs have also been studied by Allcott (2011), Ayres, Raseman, and Shih (2009), Costa and Kahn (2010), Davis (2011), and a number of consulting reports such as Violette, Provencher, and Klos (2009) and KEMA (2012). Allcott and Rogers (2012) study the long-run effects at two other Opower sites, finding somewhat less persistence that at the site analyzed here but drawing qualitatively similar conclusions. This paper is significantly different than Allcott and Rogers (2012), as it includes granular analysis of high-frequency outcome data, more evidence on the mechanisms that underlie the treatment effects, and detailed discussions of cost effectiveness.

2.2 Experimental Design

As of summer 2012, OPOWER had partnered with 70 utilities and was delivering HERs to more than five million households in its treatment groups. We focus on the only site which is one of Opower's longest-running programs, has a randomly-selected group of households dropped from treatment, and has high-frequency energy use data. We have been asked not to directly identify the partner utility, although some readers may recognize the details of the experimental design.

The experiment takes place at a large utility on the West coast. The experimental population comprises 78,887 households that use both natural gas and electricity, are relatively heavy energy users (more than 80 million British thermal units per year), live in single-family homes, have daily energy use data since the beginning of 2007, have at least 100 neighbors in similar-sized houses within a two-mile radius, have valid addresses, and do not have a solar photovoltaic system. The population was randomly assigned to treatment (34,942 households) and control (43,945 households). Two thirds of treatment group households were randomly assigned to receive monthly reports, while one third receive reports each quarter.

The first home energy reports were mailed in October 2008. Approximately 11,600 households were randomly selected to stop receiving reports after September 2010. We call this group the "dropped group." The remainder of the treatment group, which we call the "continued group," is still receiving reports at their original frequency. In February 2011, a "second wave" of 44,000 households from two nearby suburbs was added to the program, with half assigned to bimonthly treatment and half to control.

We carry out two analyses in this paper. In the "short-run analysis," we analyze daily energy use data, testing for high frequency variation in the ATEs. In the "long-run analysis," we collapse the data to the monthly level and measure the treatment effects over the past four years.

2.3 Data for Long-Run Analysis

Table 1 presents descriptive statistics. Baseline electricity usage is the household's average daily consumption during calendar year 2007. The average in the experimental population is 30.3 kilowatt-hours (kWh) per day, or 11,059 kWh per year. This is close to the national average of 11,280 (U.S. Energy Information Administration 2011). For context, one kilowatt-hour is enough energy to run an air conditioner for one hour or one standard 60-watt lightbulb for about 17 hours. This utility has an increasing block price schedule, with marginal prices of 8 to 11 cents/kWh. While there is no centralized data on marginal prices, the average price per kilowatt-hour consumed by all US residential consumers is 11.7 cents (U.S. Energy Information Administration 2011).

The dataset is extremely clean, but there are a small number of very high meter reads that may be inaccurate. We exclude the 0.00035 percent of observations with more than 1500 kilowatt-hours per day. Baseline energy usage is balanced between treatment and control groups, as well as between the dropped and continued groups within the treatment group. Natural gas usage follows very different patterns than electricity, so for simplicity, we analyze only the latter.

We also observe temperature data from the National Climatic Data Center, which are used to construct heating degree-days (HDDs) and cooling degree-days (CDDs). The heating degrees for a particular day is the difference between 65 degrees and the mean temperature, or zero, whichever is greater. Similarly, the cooling degree days (CDDs) for a particular day is the difference between the mean temperature and 65 degrees, or zero, whichever is greater. For example, a day with average temperature 95 has 30 CDDs and zero HDDs, and a day with average temperature 60 has zero CDDs and 5 HDDs. HDDs and CDDs vary at the household level, as households are mapped to different nearby weather stations.

Heating and cooling are the two largest uses of electricity in homes, and thus heating and cooling degree days are important correlates of electricity demand. In turn, the higher electricity demand magnifies the level of potential energy conservation. The Opower program's effects are therefore highly seasonal. This experiment takes place in a moderate climate, with 25 heating degrees on an average day in January and 2.2 cooling degrees on an average day in July.

There is one source of attrition from the data: households that become "inactive," typically when they move houses. In some cases we observe an account-holder's electricity use at a different location after he or she moves, but we drop these observations, and these people no longer receive reports from the program. As Table 1 shows, 20 percent of households move in the four years after treatment begins, or about four to five percent per year. This is balanced between treatment and control groups, as well as between dropped and continued groups.

There is also a source of attrition from the program: people in the treatment group can contact the utility and opt out of treatment. In this site, 1.8 percent of the treatment group has opted out since the beginning of the program. The majority of this happens within the first two years of treatment: only 0.55 percent of the continued group opts out after October 2010, the beginning of the period when the dropped group has reports discontinued. We continue to observe electricity bills for households that opt out, and we of course cannot drop them from our analysis because this would generate imbalance between treatment and control. We estimate an average treatment effect (ATE) of the program, where by "treatment" we more precisely mean "receiving reports or opting out." Our treatment effects could also be viewed as an intent-to-treat estimate, where by the end of the sample, the Local Average Treatment Effect on the compliers who do not opt out is about 1/0.982 larger than our reported ATE. Because the opt-out rate is so low, we do not make any more of this distinction in our analysis. However, when calculating cost effectiveness, we make sure to include costs only for letters actually sent, not letters that would have been sent to households that opted out or moved.

2.4 Data for Short-Run Analysis

Table 2 presents the daily electricity use data for the short-run analysis. We separate the households into three different groups: the monthly and quarterly groups that begin treatment in October 2008, and the bimonthly group from the second wave that begins in February 2011. Within each group, each household was scheduled to receive reports on the same set of days. For this part of the analysis, we exclude the dropped group households after their reports are discontinued. This reduces the sample size somewhat after September 2010 but does not generate imbalance because the households were randomly selected.

While pre-treatment usage is balanced between treatment and control for the monthly and quarterly groups, this is not the case for the bimonthly group that begins in February 2011: the treatment group's average pre-treatment usage is lower than its control group by 0.69 kWh/day, with a robust standard error of 0.20 kWh/day. The reason is that the partner utility asked Opower to allocate these households to treatment and control based on odd vs. even street address numbers. Thus, it is especially important that we d control for baseline usage when analyzing this group of households. After controlling appropriately, the imbalance does not appear to significantly bias the results, although readers may feel free to focus on the results from the monthly and quarterly groups in the first wave.

In order to analyze how daily average treatment effects respond to the receipt of home energy reports, we must predict when the reports actually arrive. In this experiment, all of the reports to be delivered in a given month for any of the three frequency groups are generated and mailed at the same time. Opower's computer systems generate the reports between Tuesday and Thursday of the first or second week of the month. The computer file of reports for all households in each utility is sent to a printing company in Ohio, which prints and mails them on the Tuesday or Wednesday of the following week. According to the U.S. Postal Service "Modern Service Standards," the monthly and quarterly groups are in a location where expected transit time is eight USPS "business days," which include Saturdays but not Sundays or holidays. The bimonthly group is in a nearby suburb where the expected transit time is nine business days. Of course, reports may arrive before or after the predicted day, and people may not open the letters immediately.

3 Short-Run Analysis

3.1 Graphical

We begin by plotting the average treatment effects for each day of the first year of the experiment for the monthly and quarterly groups, using a seven-day moving window to smooth over idiosyncratic variation. These ATEs are calculated simply by regressing Y_{it}, household i's electricity use on day t, on treatment indicator T_i, for all days t within a seven-day window around day d. We control for a vector of three baseline usage variables $\mathbf{Y}_i^b$: average baseline usage (January-December 2007), average summer baseline usage (June-September 2007), and average winter baseline usage (January-March and December 2007). We also include a set of day-specific constants π_t. For each day d, the regression is:

$$Y_{it} = \tau^d T_i + \boldsymbol{\theta}\mathbf{Y}_i^b + \pi_t + \varepsilon_{it}, \qquad \forall t \in [d-3, d+3] \tag{1}$$

Figure 2 plots the ATEs τ^d with 90 percent confidence intervals. In this regression and all others in the paper, standard errors are robust and clustered at the household level to control for arbitrary serial correlation in ε_{it}, per Bertrand, Duflo, and Mullainathan (2004). Here and everywhere else in the paper, superscripts index time periods; we never use exponents.

Figure 2 shows that the τ^d coefficients increase rapidly around October 24th, 2008, the date when the first report is predicted to arrive. Four days before the predicted arrival date, the ATE for the monthly group is -0.02 kWh/day, with a 90 percent confidence interval of (-0.11,0.07). By November 3rd, 10 days after the predicted arrival date, the ATE is -0.30 kWh/day, with a confidence interval of (-0.21, -0.40). This is equivalent to each treatment group household turning off five standard 60-watt lightbulbs for an hour, every day. The point estimates decay slightly in absolute value over the next two weeks, but this decay is small relative to the confidence intervals.

The monthly group's second report is predicted to arrive on November 21, 2008. From four days before that date until 10 days after, the treatment effect doubles: it increases in absolute value from -0.28 to -0.61 kWh/day. There are also jumps in the absolute value of the treatment effect - i.e. sudden decreases in treatment group consumption - after the third and fourth reports, but they are not nearly as noticeable as the first two.

The blue line on Figure 2 plots the daily ATEs for the quarterly group, which was randomly selected from the same population as the monthly group. Their first report also should have arrived on October 24th. Between four days before and 10 days after that date, the quarterly group's electricity use similarly decreases by a point estimate of 0.30 kWh/day. Between early November and early January, the treatment effect weakens by 0.1 to 0.2 kWh/day. In practical terms, perhaps half of the lightbulbs that were initially turned off are now back on. The quarterly group's second report arrives on the same day as the monthly group's fourth report: January 23rd, 2009. In the 14 days between January 19th and February 2nd, the point estimates of the quarterly group's treatment effect increase in absolute value from -0.25 to -0.58. These effects similarly decay away until mid-April, when the third report arrives. Around this and the fourth report, the effects similarly jump and decay, although these cyclical responses appear to become less pronounced.

This initial presentation of raw data makes clear the basic trends in households' responses to the treatment. However, the standard errors are wide, and the point estimates fluctuate on top of this basic potential pattern of jumps and decays. Holidays and weather are likely to influence the treatment effects. Collapsing across multiple report arrivals can reduce standard errors and smooth over idiosyncratic variations, and controlling for weather can both increase efficiency and, if correlated with report timing, remove bias.

We therefore estimate the treatment effects in "event time," meaning days before and after predicted report arrival. We index the 48-hour periods before and after report arrival by a and define an indicator variable A_t^a that takes value 1 if day t is a 48-hour periods after a report arrival date. We construct a vector $\mathbf{M}_{it}$ of functions of heating and cooling degree days that parsimoniously captures the typical relationship between these variables and the treatment effects.[3] Denote τ^a as the ATE for each period a. The event time regression is:

$$Y_{it} = \sum_a \tau^a A_t^a T_i + \boldsymbol{\beta} T_i \mathbf{M}_{it} + \boldsymbol{\theta} \mathbf{Y}_i^b + \pi_t + \varepsilon_{it} \tag{2}$$

Figure 3a plots the τ^a coefficients and 90 percent confidence intervals for the monthly, quarterly, and bimonthly groups using data around each group's first four reports. The point of this graph is to show the shape of the τ^a coefficients in event time, not the average level. Thus, the levels of all τ^a coefficients within each group have been shifted so that all three groups can be presented on the same graph. Within each group, the effects follow remarkably similar patterns in event time. The treatment group decreases consumption by about 0.2 kWh/day in the several days around the predicted arrival date. Some reports arrive and are opened before the predicted date, which causes consumption to decrease before $a = 0$. The absolute value of the treatment effect reaches its maximum eight to ten days after the report arrives. After that point, the treatment effect decays, as the treatment group's conservation efforts diminish. This decay is difficult to observe for the monthly group, because before much decay happens, the next report arrives, causing event time

[3] $\mathbf{M}_{it}$ contains six variables: $1(CDD_{it}) > 0$, CDD_{it}, $1(0 < HDD_{it} \leq 5)$, $1(5 < HDD_{it} \leq 35)$, $HDD_{it} \cdot 1(5 < HDD_{it} \leq 35)$, and $1(HDD_{it} > 35)$. This were chosen based on inspection of the non-parametric relationship between ATEs and degree-days, as illustrated by Figure 5.

to re-start. For the quarterly group, the treatment effect decays by 0.2 kWh/day between 10 days and 80 days after the report arrival.

Figure 3b is analogous to Figure 3a, except that it uses data for all reports beginning with the fifth report. The treatment effects are very close to constant in event time. Coefficients for the bimonthly group are very imprecisely estimated because there are only six reports delivered to this group, meaning that this graph is estimated off of the event windows around only two reports.

3.2 Empirical Strategy

We now carry out formal econometric estimates of the patterns suggested in the figures. First, we estimate the magnitude of the increases in the absolute value of the treatment effect around the report arrival window. Second, we estimate the rate of decay in the treatment effect between reports.

Define S_t^0 as an indicator variable for the seven-day arrival period beginning three days before and ending three days after the predicted arrival date. S_t^{-1} is an indicator for the seven-day period before that, and S_t^1 is an indicator for the seven-day period after. Define $S_t^a = S_t^{-1} + S_t^0 + S_t^1$ as an indicator for all days in that window. As before, $\mathbf{M}_{it}$ is the same function of weather, $\mathbf{Y}_i^b$ is the three baseline usage controls, and π_t are day-specific dummies. The coefficient on τ^1 in the following regression reflects the change in the treatment effect in period S^1 relative to period S^{-1}:

$$Y_{it} = \left(\tau^a S_t^a + \tau^0 S_t^0 + \tau^1 S_t^1 + \tau\right) \cdot T_i + \boldsymbol{\beta} T_i \mathbf{M}_{it} + \boldsymbol{\theta} \mathbf{Y}_i^b + \pi_t + \varepsilon_{it} \tag{3}$$

To estimate the decay rate, we define an indicator variable S_t^w to take value 1 if day t is in a window beginning eight days after a predicted arrival date and ending four days before the earliest arrival of a subsequent report. The variable d_t is an integer reflecting the number of days past the beginning of that period, divided by 365. For example, for a t that is 18 days after a predicted arrival date, d takes value (18-8)/365. Thus, the coefficient on d_t, denoted δ, measures the decay of the treatment effect over the window S^w in units of kWh/day per year.

$$Y_{it} = (\tau^w + \delta d_t) \cdot T_i S_t^w + \boldsymbol{\beta} T_i \mathbf{M}_{it} + \boldsymbol{\theta} \mathbf{Y}_i^b + \pi_t + \varepsilon_{it} \tag{4}$$

Our model predicts linear decay of the treatment effects as d_t increases. One might hypothesize that the decay process could be convex or concave, and it would seem unrealistic to extrapolate beyond the time when the predicted treatment effect reaches zero. However, our sample is not long enough the effects to return to zero, and it is not large enough to precisely estimate non-linearities. We therefore use the linear model for simplicity.

3.3 Results

Analogously to Figures 3a and 3b, we run the above regressions separately for the initial set of four reports and all reports after that initial set. Table 3a presents the estimates of Equation (3) for the windows around the first four reports. There are three pairs of columns, for the monthly, quarterly, and bimonthly groups. Within each pair, the regression on the right includes the degree-day controls $\beta T_i \mathbf{M}_{it}$, while the left regression does not. Across the six regressions, the coefficient τ^1 on the TS^1 interaction ranges from -0.162 to -0.248 kWh/day. This means that between the week before the seven-day arrival windows and the week after those windows, the average household that receives a letter reduces consumption relative to counterfactual by the equivalent of three or four 60-watt lightbulbs for one hour. The coefficients do not change substantially when controlling for weather.

What's especially remarkable about the immediate consumption decreases after the initial reports is that they add up to more than the average daily flow of savings across all days in the first year of treatment. Multiplying the above bounds on the estimated per-report effects $\widehat{\tau}^1$ for the initial four reports by four gives a total decrease of 0.65 to 0.99 kWh per day - the equivalent of turning off a standard 60-watt lightbulb for an additional 11 to 16 hours. By contrast, we will estimate (in Column 1 of the upcoming Table 6) that the first-year ATE is -0.66 kWh per day. Of course, if the effects did not decay in the intervening days after these initial reports, this would not be mathematically possible.

What makes this possible is that, as we saw in Figures 2 and 3a, the treatment effects do decay between the initial four reports. Table 4a measures this formally using Equation (4). The estimates of δ vary across the three groups, but the coefficients are positive in all regressions and statistically positive in all but one. The quarterly and bimonthly estimates are more highly robust to weather controls. For the monthly group, the point estimates differ somewhat when weather is included. This is likely because relative to the quarterly and bimonthly groups, the monthly group has shorter event windows S^w that can be used to estimate $\widehat{\delta}$, and because the sample period is limited to the first four reports, there are fewer days that can be used to estimate the weather controls $\widehat{\beta}$.

To put the magnitudes of δ in context, focus on the estimates for the quarterly group, controlling for weather. A $\widehat{\delta}$ of 0.738 means that a treatment effect of -0.738 kWh/day would decay to zero in one year, if the linear decay continued to hold. Thus, the jump in treatment effects of $\widehat{\tau}^1 = -0.248$ from Column 4 of Table 3a would decay away fully within about four months. This never happens, because the next report arrives less than three months after the window S^w begins.

Tables 3b and 4b replicate Tables 3a and 4b for the remainder of the samples beginning with the fifth report. Table 3b shows that in the monthly and quarterly groups from the first wave, the τ^1 coefficients are still statistically significant, but they are only about one-quarter the magnitude of $\widehat{\tau}^1$ for the initial four reports. The coefficient for the bimonthly group, however, is relatively large. Because this is estimated off of only the fifth and sixth reports, it is difficult to infer much of a pattern. It could be that there are unobserved moderators of the treatment effects that coincide with these reports, or that the information included in these particular reports was different in a particularly compelling way.

Table 4b shows that there is no statistically significant decay of the effects after the first four reports. Interestingly, all of the point estimates are positive, suggesting that there may still be some decay, but the event windows are not long enough for precise estimates. This highlights the importance of the next section, in which we exploit the discontinuation of reports to estimate a decay rate over a much longer period: two years instead of two to ten weeks.

Appendix Tables A1a-b and A2a-b replicate Tables 3a-b and 4a-b with two sets of additional robustness checks. The left column of each pair excludes outliers: all observations of Y_{it} greater than 300 kWh/day and all households i with average baseline usage greater than 150 kWh/day, which is five times the mean. Based on our inspection of the data, these observations do not appear to be measured with error. However, they implicitly receive significant weight in the OLS estimation, so a small number of high-usage households could in theory drive the results. Relative to Tables 3a-b and 4a-b, the coefficients change only slightly.

The right column of each pair in the appendix replicates the right column in each pair of regressions from the body of the paper, except controlling for the interaction of the treatment dummy with control group average usage on day t. Daily treatment effects are strongly correlated with control group usage, and these regressions control for any underlying patterns in electricity use that might be associated with report arrival times. While much of this correlation acts through the weather controls which are included in Tables 3 and 4, control group average usage is a slightly better predictor of the ATEs. Here again, the coefficients of interest are strikingly robust. The only coefficient that changes is $\widehat{\delta}$ for the monthly group's initial four reports, which shrinks in magnitude, making it statistically indistinguishable from the estimated $\widehat{\delta}$'s for the other five specifications in Table 4a.

All households in all three groups receive reports around the same day of the month, typically between the 19th and the 25th. One might worry that our results could somehow be spuriously driven by underlying monthly patterns in the treatment effect. Of course, these underlying patterns would have to take a very specific form: they would need to generate cycles in treatment effects that begin in October 2008 and eventually attenuate for the monthly and quarterly groups, then appear beginning in February 2011 for second wave households but do not re-appear for the monthly and quarterly groups. We can explicitly test for spurious monthly patterns by exploiting the differences in report frequencies to generate placebo report arrivals. We focus on the monthly vs. quarterly frequencies, because they are randomly assigned, and consider only the period after the first four reports, because before that, the quarterly ATE changes significantly in the time between reports. If there were spurious day-of-month effects, the quarterly group's treatment effects would jump in absolute value at the times when the monthly group receives reports but the quarterly group does not. Appendix Table A3 shows that the $\widehat{\tau}^1$ coefficient for these placebo report arrival dates is statistically zero and economically small relative to the $\widehat{\tau}^1$ estimated in Tables 3a and 3b.

3.4 Initial Effects and the Extensive Margin

There are two basic models of why the average treatment effects would jump repeatedly in absolute value upon the arrival of the first few reports. One model is that additional reports act on the extensive margin of households, affecting new and different consumers each time. This could happen because not everyone reads and pays attention to unsolicited mail, so only a fraction of households open each report. Households that open a report for the first time are spurred to reduce energy use in whatever ways are immediately possible, but this motivation gradually wanes. In this model, the repeated cycles in the treatment effect are caused by incremental households opening a report for the first time, and the cycles attenuate because eventually every household has experienced the initial "shock" of opening that first report.

An second model is that the reports repeatedly motivate the *same* households to conserve: each report draws attention to energy conservation, then that attention fades somewhat, but the next report draws attention again. This model is a bit more puzzling: it requires repeated learning and forgetting, or attention that is malleable at relatively high frequencies, repeated experimentation and failure, or something else that would generate repeated within-household cycling.

While it is often difficult to say much about individual-level treatment effects as opposed to average or conditional average treatment effects, our setting offers a unique opportunity to test between these two models. Intuitively, we want to test whether a household that decreases consumption after a report arrives is also likely to decrease consumption after the next report arrives. If the correlation between these two decreases is positive, this means that some of the same households are repeatedly conserving.

Mathematically, index the first four home energy reports by $h = \{1, 2, 3, 4\}$ and denote S_{th}^{-1} and S_{th}^{1}, respectively, as the pre-arrival and post-arrival periods for report h. Define ΔY_{ih} as the difference in household i's consumption after vs. before report h arrives: $\Delta Y_{ih} = \overline{Y}_{it}|(S_{th}^{1} = 1) - \overline{Y}_{it}|(S_{th}^{-1} = 1)$. ΔY_{ih} can also be thought of as a household-specific estimate of τ^1 for report h, including the household's idiosyncratic errors. We wish to test whether this "treatment effect" is correlated with the treatment effect for the previous report, i.e. whether ΔY_{ih} is positively correlated with ΔY_{ih-1}.

The actual test is a bit more nuanced, because there could be natural sources of positive or negative autocorrelation in ΔY. For example, mean reversion would mechanically generate negative autocorrelation: a household that goes on vacation as the first report arrives and returns as the second report arrives has a negative series of ε_{it}'s over that period. This gives a negative ΔY_{i1} and a positive ΔY_{i2} as consumption drops and then reverts to normal. Thus, our empirical specification must control for the control group's natural underlying correlation in ΔY and test whether the correlation is relatively higher or lower in the treatment group. If the correlation is relatively lower, this means that the first "extensive margin" model is more common: the jumps in the absolute value of the average treatment effects are more likely to be caused by households that did not already decrease consumption. On the other hand, if the correlation is relatively higher in treatment relative to control, this means that the jumps in the ATEs are more likely to be caused by households that had already decreased consumption.

We regress ΔY_{ih} on ΔY_{ih-1}, controlling for report-specific intercepts ϕ_h:

$$\Delta Y_{ih} = \rho T_i \Delta Y_{ih-1} + \sigma \Delta Y_{ih-1} + \tau^1 T_i + \phi_h + \varepsilon_{ih} \tag{5}$$

Table 5 presents the results of this regression. Columns 1-3 present the results separately for the monthly, quarterly, and bimonthly groups. In order to increase precision, Column 4 combines all of the data and controls from the first three columns. Column 5 adds interactions of T with ϕ, which allows for differential treatment effects for each report in each frequency group. Column 6 excludes outliers - households with baseline usage larger than 150 kWh per day and observations of ΔY larger than 100 kWh/day in absolute value.

The τ^1 coefficients on the T dummies are analogous to the τ^1 coefficients from Equation (3), which measure the treatment group's reduction in consumption between pre-arrival period S^{-1} and post-arrival period S^1. The coefficients will differ in general because the regressions are structured differently, and in particular because Equation (5) excludes the large consumption reduction associated with the first report because there is no lagged change ΔY_{ih-1} for that report. In the first three columns, the $\widehat{\tau}^1$ range from -0.053 to -0.127, slightly less than the $\widehat{\tau}^1$ from Equation (3) but consistent the basic result of immediate reductions in energy use after reports arrive.

The $\widehat{\rho}$ coefficient is positive in all six regressions, although it is statistically indistinguishable from zero for the bimonthly group when considered in isolation in Column 3. To put the magnitudes in context, the inter-quartile range of ΔY_{ih} is $[-3, 2\frac{4}{7}]$. The estimated $\widehat{\rho} \approx 0.02$ implies that a household that reduced consumption by 1 kWh/day after vs. before the first report arrives is predicted to reduce consumption by 0.02 kWh/day after vs. before the second report arrives, after controlling for underlying patterns in the control group. This magnitude would seem small if it reflected the autocorrelation in household-specific treatment effects. However, ΔY_{ih-1} reflects a true household-specific treatment effect plus a relatively large idiosyncratic error. Thus, $\widehat{\rho}$ should not be interpreted as the autocorrelation in household-specific treatment effects, as it would suffer from attenuation bias. Instead, $\rho = 0$ is a directional test of our two models introduced above. Our finding that $\widehat{\rho} > 0$ means that the initial reports repeatedly stimulate some of the same households into immediate conservation.

4 Long-Run Analysis

4.1 Graphical

For the long-run analysis, we collapse the same data to the household-by-month level to reduce computational burden and analyze the intervention's effects over four years. We analyze the monthly and quarterly groups together, and we focus on the first wave, as second wave households began only in February 2011.

We first plot the ATEs for each month of the sample for both the continued and dropped treatment groups. The variables D_i and E_i are indicator variables for whether household i was assigned to the dropped group and the continued group, respectively. Both variables take value 0 if the household was assigned to the control group which never received reports, meaning that $D_i + E_i = T_i$. In this regression, m indexes the 56 calendar months from the beginning to the end of the post-baseline sample, from January 2008 through August 2012. The sets of coefficients τ_m^D and τ_m^E are month-specific treatment effects for the dropped and continued groups, respectively. We include 56 month-specific controls for baseline usage, denoted $\theta_m Y_{im}^b$, where Y_{im}^b is household i's average usage in the same calendar month. The variables π_m are month-specific intercepts. The estimating equation is:

$$Y_{im} = \tau_m^D D_i + \tau_m^E E_i + \theta_m Y_{im}^b + \pi_m + \varepsilon_{im} \tag{6}$$

Figure 4 present the estimates of Equation (6). Other than the controls for baseline usage, which substantially improve efficiency, these graphs present unadulterated differences in means. As a result, they give a clear sense of what the data contain and what should be considered in the more formal analysis below. The y-axis is the treatment effect, which is negative because the treatment causes a reduction in energy use. The first vertical line indicates the date of first report generation for the treatment groups. The second vertical line marks the date when the last reports were generated for the dropped group.

To the left of the first vertical line, the intervention has not yet started, and the treatment effect is statistically zero. As we saw in the previous section, consumers respond immediately to the reports. The effects continue to increase in absolute value fairly rapidly over the first year, and the rate of growth in the effect slows after that. Until the second vertical line, both the continued and dropped groups receive the same treatment, and the effects are indistinguishable in the two groups, as would be expected due to random assignment. The magnitudes of the effects exhibit seasonality: due to mild summers and moderately cold winters, the treatment effects are stronger in the winter.

The second vertical line marks the beginning of the program's third year. After this point, effects continue to increase in absolute value for the group still receiving reports. By contrast, the effects in the dropped group decay slightly relative to what they had been while the intervention was ongoing. The ATEs in winter and summer 2012 are each about 0.1 kWh/day less than they had been in winter and summer 2011, respectively.

4.2 Empirical Strategy

In the long-run analysis, we ask two questions. First, how durable are the effects as long as the treatment continues? Second, how persistent are the effects after treatment is discontinued? When answering the second question, we can compare long-run decay rates to the short-run decay rates estimated in the previous section.

We define P_m^0, P_m^1, and P_m^2 as indicator variables for whether month m is in the pre-treatment period or the first or second year of treatment, respectively. P_m^3 is an indicator variable for whether month m is in the third or fourth year of treatment, which is the period after the dropped group has reports discontinued. The variable r_m is the negative of time in years until the end of the sample. In the long-run analysis, $\mathbf{M}_{im}$ is a vector of weather controls with two elements: average heating degrees and average cooling degrees for household i in month m. Our estimating equation is:

$$\begin{aligned} Y_{im} = & (\tau^0 P_m^0 + \tau^1 P_m^1 + \tau^2 P_m^2) \cdot T_i + \gamma E_i P_m^3 \\ & + \alpha D_i P_m^3 + \delta^{LR} r_m D_i P_m^3 \\ & + \psi \mathbf{M}_{im}(T_i P_m^2 + D_i P_m^3) + \lambda \mathbf{M}_{im}(P_m^2 + P_m^3) \\ & + \theta_m Y_{im}^b + \pi_m + \varepsilon_{im} \end{aligned} \tag{7}$$

In the first line of this equation, the coefficients τ^0, τ^1, and τ^2 are ATEs for the treatment groups - i.e., both the continued and dropped groups - for the pre-treatment period and the first and second year, respectively. The γ coefficient measures the continued group's treatment effect in years 3 and 4. The second line parameterizes the treatment effect for the dropped group after treatment is discontinued. The coefficient δ^{LR} is the long-run decay rate of the treatment effect. Because r_m has units in years, the units on δ^{LR} will be the change in the treatment effect per year, i.e. kWh/day per year. As in the short-run analysis, we assume linear decay rates for simplicity, because the sample is still not long enough or large enough to reject this assumption. Because r_m increases to zero at the end of the sample, α reflects the fitted treatment effect for the dropped group at the end of the sample.

Controlling for the interaction of the treatment effect with heating and cooling degrees $\mathbf{M}_{im}$ means that the τ, α, and δ coefficients reflect treatment effects when the mean temperature is 65 degrees. Thus, the δ coefficient reflects the decay in the treatment effect after controlling for weather-related fluctuations.

4.3 Statistical Results

Table 6 presents the estimates of Equation (7) and closely-related specifications. Column 1 estimates just the τ, γ, and α coefficients, omitting the decay rates and not conditioning on weather. The treatment effects closely map to the effects illustrated in Figure 4: effects increase in absolute value from statistically zero in the pre-treatment period to -0.452 and -0.660 kWh/day in the first and second years, respectively. The program's effects are highly durable: when continued in the third and fourth years, the estimated ATE is -0.842 kWh/day. When the program is discontinued, the effects are also remarkably persistent: the ATE is -0.612 kWh/day for the dropped group in the two years after treatment is discontinued.

Column 2 tests for whether the effects in the two groups increase or decrease in the post-drop period, relative to what they had been in the second year. To do this, we re-estimate Column 1 after substituting $\tau^{23}(P_m^2 + P_m^3) \cdot T_i$ for $\tau^2 P_m^2$. The α and γ coefficients now reflect each group's difference in effects for years 3 and 4 relative to year 2. Column 3 repeats this specification including the weather controls from the third line of Equation (7). In each of these two columns, we see that as long as the reports continue over the third and fourth years, treatment group households continue to incrementally reduce energy use. The effects in the dropped group are smaller in absolute value, but this difference is not statistically significant from what it had been in the second year. This means that the decay of the treatment effect is slow enough that it cannot be picked up in this specification. As Figure 4 illustrates, however, the effect does appear to decay after reports are discontinued. Combining the third and fourth years into one period makes it difficult to detect the decay between the beginning and the end of that period.

Column 4 adds the linear decay term $\delta^{LR} r_m D_i P_m^3$ to the basic specification in Column 1. Relative to control, consumption in the dropped group increases by 0.131 kWh/day per year across the third and fourth years. Column 5 includes the weather terms, meaning that this is exactly the specification in Equation (7). Although they are not statistically significant, the ψ coefficients are both negative, which implies that as temperatures deviate more from 65 degrees, the treatment effect becomes stronger. The weather controls change $\widehat{\delta}^{LR}$ only slightly, to 0.117 kWh/day per year. The $\widehat{\alpha}$ coefficients show that the predicted treatment effect at the end of the first year is -0.45 kWh/day, which closely aligns with the illustration in Figure 4.

Column 6 repeats Column 5 including only the balanced panel, and the coefficients are all essentially unchanged. This means that the changes in the effects over time are not somehow due to consumers with different treatment effects differentially attriting from the sample as they move.

Allcott (2011) documents that the program causes more conservation by heavier baseline users, and monthly treatment causes more conservation than quarterly. Appendix Table 4 confirms these results for this site but documents that the standard errors are too large to infer much about whether decay rates differ along these dimensions.

In sum, the results show that when reports are discontinued after two years of treatment, about two-thirds of the effect remains two years later. In tangible terms, a treatment effect of -0.660 kWh/day for the program's second year means that the average treatment group household took actions equivalent to turning off a standard 60-watt lightbulb for about 11 hours each day. At the end of the sample, the average dropped group household took actions equivalent to turning off that lightbulb for 7.5 hours each day. By contrast, the average continued group household was doing twice as much - the equivalent of turning off that lightbulb for about 15 hours each day. We can also compare the estimated long run decay rate $\widehat{\delta}^{LR}$ to the decay rate between each of the first four reports, the $\widehat{\delta}$ estimated in the previous section. In most specifications, this $\widehat{\delta}$ was between 0.75 and 1.5 kWh/day per year, which is six to 12 times faster than $\widehat{\delta}^{LR}$. In the next two sections, we discuss the implications of the differences between the initial decay rate δ and the long-run decay rate δ^{LR}.

5 Mechanisms

Concretely, what actions underlie the observed effects? In many behavioral interventions, this question is difficult to answer. For example, if a program incentivizes people to lose weight, it may not be clear how much of their observed weight loss comes from exercise vs. reduced calorie intake, and within these categories, what form of exercise is the most useful and what foods have been cut out of their diets. In our setting, there are three particularly important questions about the tangible underlying actions. First, how much of the conservation is associated with heating vs. cooling, and does this pattern change over time? Second, how much of the effect comes from large observed changes to physical capital stock? Third, what do treatment group households report that they are doing differently than control group households?

5.1 Heating and Cooling Effects

The dashed line on Figure 5 shows that average control group usage is lowest on days when the mean temperature is between 60 and 65 degrees Fahrenheit. At warmer temperatures, more electricity is used to power air conditioners. At colder temperatures, more electricity is used for space heating, water heating, and other heating-related activities. Of course, this relationship does not reflect the causal effect of temperature on usage, as temperature may be associated with other underlying factors that affect energy use. For example, people tend to be at home using electricity on Thanksgiving, and New Year's, and these holidays happen at cold times of year. However, because heating and cooling are the largest uses of energy in the average American household (U.S. Energy Information Administration 2005), other unobserved factors may be relatively small compared to the extent to which heating and cooling affect electricity use.

Figure 5 also plots daily ATEs as a function of mean temperature for each of the four post-treatment years, smoothed using a rectangular kernel with halfwidth of two degrees. If none of the treatment effect were associated with heating and cooling, then these lines would be horizontal as a function of temperature. For example, if the treatment effect resulted entirely from households purchasing more energy efficient microwaves, and if people don't tend to use microwaves more on hotter or colder days, then the lines on Figure 5 would be horizontal. Instead, the ATE is smallest in absolute value at 64 degrees, and households conserve more as temperatures move above or below this point. This suggests that the intervention does affect heating- and cooling-related actions, such as adjusting thermostats or purchasing energy efficient air conditioners. In addition, the graph suggests that both heating and cooling actions and non-temperature-related actions increase over the program's life: the level of the treatment effect at 64 degrees, as well as the slope of the relationship between weather and temperature, increase over the successive years of the program.

5.2 Utility Energy Efficiency Program Participation

Like many utilities across the country, the utility we study runs a series of other energy conservation programs that subsidize or directly install energy efficient capital stock. The utility maintains household-level program participation data, which is primarily used to estimate the total amount of energy that each program conserves. However, these household-level data are also useful in estimating whether the Opower intervention affects energy use through an increase in program participation.

Table 7 presents the program participation data for the experimental population for an example year, calendar year 2011. For each program, the utility has estimated the kilowatt-hours of electricity that a typical participant would save. The table lists all ten programs where any amount of electricity would be conserved and at least one household in the first wave experimental population participated in 2011. The most popular of these programs are a subsidy for new energy efficient clothes washers, installation of compact fluorescent lightbulbs, the removal of an old energy-inefficient refrigerator or freezer, and installation of low-flow showerheads.

Column 1 of Table 7 shows the estimated flow of savings per participant, translated into kilowatt-hours per day to be consistent with the units in the rest of the paper. Column 3 shows the difference, also in kWh/day per household, in estimated savings between the continued and control groups. Column 4 reports the difference in estimated savings between the entire treatment group and the control group. Only one program, a program to replace traditional incandescent lightbulbs with energy-saving Compact Fluorescent Lightbulbs (CFLs), shows a statistically significant in savings between treatment and control. The standard errors are very tight, allowing us to rule out any economically significant differences. The CFL program, for example, appears to generate 0.00224 kWh/day incremental savings in the continued treatment group. Using the estimates in the bottom row, which combine the savings across all programs, the upper bound of the 90 percent confidence interval on savings is 0.006 kWh/day. By contrast, the continued group's treatment effect in the program's third year was -0.842 kWh/day, an increment of -0.181 compared to the year before.

Aside from changing the rate at which households participate in energy efficiency programs, the Opower program could also change the timing of their participation. In other words, the program could move forward investments that would have happened later in the absence of the program. To test this, Column 5 reports estimated savings for calendar year 2011 only, pro-rating each participant's savings over only the part of the year after their recorded date of program participation. There are no statistically or economically significant differences.

The utility also runs "weatherization" programs, including installation of new insulation and re-sealing of heating and cooling system ducts. These are not included in Table 7 because the utility assumes for their internal calculations that these programs only affect natural gas consumption, not electricity consumption. Participation is statistically and economically identical in treatment and control.

Of course, these data only reflect households that participate in utility-sponsored programs. For large investments such as clothes washers, refrigerators, and insulation, the subsidies are large, so suppliers have strong incentives to report their customers' investment in order to collect the subsidy. Thus, these data are likely to be good measures of large changes to physical capital stock.

5.3 Surveys of Self-Reported Actions

Other than large changes to physical capital stock, there are many other ways to conserve energy. One way to attempt to measure these is through surveys of self-reported actions. In the past two years, Opower has surveyed about six thousand people in treatment and control groups in six sites nationwide, including 800 people in the utility we study in this paper. The surveys are conducted via telephone on behalf of the utility, and for practical reasons no effort is made to obscure the fact that the surveys are about energy use and conservation behaviors. Completion rates are typically between 15 and 25 percent of attempts. Because these are self-reported actions that reflect only a small share of the experimental population, we discuss these data only briefly, and they should be interpreted with great caution.

Table 8 presents the survey data. The left three columns contain results from all six survey sites, while the right three columns contain results only from the site we study in this paper. Within each set of three columns, the first presents the mean number of people who report taking the action in the past 12 months. The second column presents the difference in probability of taking the action between treatment and control groups. The third column presents the difference in probability after controlling for five respondent characteristics: gender, age, whether homeowner or renter, education, and annual income.

About 80 percent of people report taking any steps to reduce energy use in the past 12 months. Those who say that they have taken any steps are asked a series of questions about whether they have taken particular actions in the past 12 months. The particular actions vary by survey, but many of the same actions are queried in multiple sites. In the utility we study, respondents were asked about 11 actions. We group actions into three categories: *repeated actions* such as switching off power strips and turning computers off at night, *physical capital changes* such as purchasing Energy Star appliances, and *intermittent actions* such as replacing air filters on air conditioning or heating systems.

The remarkable result of these surveys is that there is very little difference in self-reported actions across treatment and control groups. The only difference that is consistent across different surveys is that treatment group households are more likely to report having a home energy audit. Audits often include installation of new compact fluorescent lightbulbs, which typically last several years until they must be replaced, and also are typically required before moving forward with larger investments such as weather sealing or new insulation. One survey, at a utility in a warmer climate than the one we study, finds that the treatment group is more likely to use fans to keep cool instead of running air conditioners. Across all sites, the treatment group is more likely to report participating in utility energy efficiency programs, but the difference is not statistically significant in the specification that conditions on observables. In fact, in the utility we study, the treatment group reports being *less* likely to have taken any steps to reduce energy use, although this is not the case in other sites, and the difference is also not statistically significant when including observable characteristics.

We increase the power of these tests by testing whether treatment is more likely to take any of the actions within our three categories. When aggregating in this way across actions and across sites, our standard errors are small enough to rule out with 90 percent confidence that the intervention increases self-reported probability of taking energy conservation actions by more than one to two percent. Thus, these results show that self-reported actions do not differ in any way that would be economically-meaningful. Furthermore, the lack of statistical significance would only be further reinforced by adjusting the p-values for multiple hypothesis testing.

There are multiple interpretations of these results. First, it is difficult to learn much from surveys of self-reported actions, due to demand effects, selected samples, and the fact that different respondents might interpret questions differently. Second, it is possible that the intervention does not change the types of actions that people take to conserve energy, but instead changes the intensity with which some people take these actions. In other words, an important impact of the intervention may be not to give information about new ways to conserve, but instead to increase attention and motivation to conserve in more of the same ways. This would be less consistent with a model under which the intervention acts through information provision and more consistent with a model under which the intervention draws attention. When the initial intervention is removed, attention wanes, suggesting a model under which attention is "malleable" in response to stimuli.

5.4 Summary: The Dynamics of Household Responses

Taken together, our analyses of short-run and long-run treatment effects along with information on weather, program participation, and self-reported actions, start paint a picture of how consumers respond to the Opower intervention. As the initial reports arrive, some consumers are immediately motivated to conserve. They must be taking actions that are feasible within a short period of time, probably changing utilization choices by adjusting thermostats, turning off lights, and unplugging unused electronics. However, the behaviors "backslide" toward their pre-intervention levels over a few months, perhaps because households lose motivation as the reports fade into the past, or because they learn that the actions they were taking are more difficult or save less energy than they had thought. At least some households are repeatedly motivated to conserve as additional reports arrive. This process of action and backsliding is a repeated version of the phenomena documented by Agarwal *et al.* (2011) and Haselhuhn *et al.* (2012), who document that consumers learn to avoid credit card and movie rental late fees after incurring a fee, but they act as if this learning depreciates over time.

After the first few reports, this repeated action and backsliding attenuates, and decay of treatment effects can only be observed over a longer period after reports are discontinued for part of the treatment group. This means that in the intervening one to two years between the initial reports and the time when the reports are discontinued, consumers invest in some form of new "capital stock" that decays at a much slower rate. The program participation data shows that very little of this capital stock is large changes to physical capital such as insulation or major appliances. This capital stock might take the form of a wide variety of smaller changes, such as installing energy efficient Compact Fluorescent Lightbulbs.

Much of this capital stock may also reflect changes to consumers' utilization habits, which Becker and Murphy (1988) call "consumption capital." This stock of habits lowers the marginal cost of conservation behavior on any given day, because the choices become ingrained with some amount of automaticity. However, just as in the Becker and Murphy (1988) model and most models of capital stock, consumption capital also decays. This story is consistent with the results of Charness and Gneezy (2009), who show that financial incentives to exercise have some long-run effects after the incentives are removed, suggesting that they induced people to form new habits of going to the gym.

6 Cost Effectiveness and Program Design

In this section, we assess the importance of long-run persistence for cost effectiveness and for program design. We use a simple measure of cost effectiveness: the dollar cost to produce and mail the reports divided by the kilowatt-hours of electricity conserved. Although cost effectiveness is the most common metric by which many types of programs are assessed, we emphasize that this is not the same as a welfare evaluation. In this context, consumers might experience additional unobserved costs and benefits from the intervention: they may spend money to buy more energy efficient appliances or spend time turning off the lights, and they might be more or less happy after learning how their energy use compares to their neighbors'. Furthermore, the treatment causes a reduction in natural gas use in addition to its effects on electricity; we have left this for a separate analysis.

In a first analysis, we calculate the cost effectiveness of the existing program using only the sample data, which allows us to demonstrate the importance of persistence with zero additional assumptions. Second, we calculate the cost effectiveness of different potential program designs using additional assumptions about discount rates and persistence.

6.1 In-Sample Cost Effectiveness

Table 9 presents the in-sample cost effectiveness estimates: total program costs divided by total electricity savings observed between the beginning and end of the sample. Calculating cost effectiveness only over the sample period allows us to demonstrate the importance of different assumptions about persistence without needing to predict future effects. If we were to extrapolate further into the future, assumptions about persistence would make even more of a difference.

We assume that the cost per report is \$1 and that there are no fixed costs of program implementation. The savings estimates are simply the average treatment effects for each period estimated in column 1 of Table 3 multiplied by the length of each period. For example, scenario 1 reflects the observed results for the continued treatment group. The total electricity savings are (0.452 kWh/day)·365 days + (0.660 kWh/day)·(365 days) + (0.842 kWh/day)·(700 days)=995 kWh. Standard errors are calculated using the Delta method. For simplicity, there is no time discounting

Opower competes against other energy conservation programs, and cost effectiveness is one of the most important metrics for comparison. There are some benchmark numbers available, although they are controversial (Allcott and Greenstone 2012). The American Council for an Energy Efficient Economy estimates that in 14 states with aggressive energy conservation programs, state average cost effectiveness ranged from 1.6 to 3.3 cents per kilowatt-hour (Friedrich *et al.* 2009). Arimura *et al.* (2011) estimate cost effectiveness to be about 5.0 cents/kWh at a discount rate of five percent.

For scenario 1, dividing a total cost of \$32.10 per household by the 995 kWh of observed savings gives a cost effectiveness of 3.23 cents/kWh. Scenario 2 reflects the observed results for the dropped treatment group. The costs of this treatment are substantially lower, and because the effects are so persistent, the savings are almost as large. As a result, the in-sample cost effectiveness is significantly improved relative to the continued group: 2.15 cents per kilowatt-hour.

Of course, these estimates benefit from hindsight: up to sampling error, we now know exactly what the effects were. When deciding whether to adopt a program, policymakers must make a series of assumptions about efficacy during treatment and long-run persistence after treatment is discontinued. Scenarios 3 and 4 calculate cost effectiveness under the two alternative extreme assumptions. The first assumption is zero persistence: after the treatment stops, the effects end immediately. While this assumption may seem unduly conservative in light of our empirical results, this is the implicit assumption under which many ongoing Opower programs have been evaluated in the absence of these results. The second assumption is full persistence over the remainder of the sample. Under zero persistence, cost effectiveness is 4.42 cents/kWh, while under full persistence, cost-effectiveness is 2.07. Counter to the way the programs have often been evaluated, the empirical estimates over this sample period are closer to the full persistence assumption.

The bottom panel of Table 9 displays average cost savings per household and if aggregated over the 35,000 treatment group households. The average household in the continued and dropped groups, respectively, consumed \$100 and \$83 less electricity between October 2008 and the end of August 2012. If these treatments had been applied to all of the 35,000 households in the treatment group, the total savings over the sample would be \$3.48 and \$2.92 million, respectively. If the entire treatment group were dropped after two years and experienced zero persistence or full persistence, the total electricity cost savings would have been \$1.42 and \$3.03 million, respectively.

These results highlight two important issues. First, the alternative extreme assumptions around persistence can make more than a 100 percent difference in cost effectiveness. Second, these assumptions could influence program adoption decisions: all of these numbers are within the ranges reported above for competing energy conservation programs.

6.2 Program Design

The in-sample estimates above demonstrate the importance of persistence with no additional assumptions, but they are not very useful for program design. For example, comparing scenarios 1 and 2 appears to show that cost effectiveness is improved if reports are discontinued. However, these calculations do not take account of the additional energy conservation that will be almost surely be observed in the future. In this subsection, we compare the cost effectiveness of different potential program designs while making out-of-sample assumptions about persistence. Because some of the electricity consumption reductions are now much further in the future, time discounting is important. All dollars costs and consumption reductions are now discounted to the beginning of the program at a five percent discount rate.

Table 10 compares four different program designs. The first is a one-shot intervention - one report, with no future interventions. We assume that there is an initial effect of 0.30 kWh/day, which decays linearly at 0.75 kWh/day per year, consistent with estimates for the quarterly group from Table 4. As a result, the effect is assumed to have fully disappeared in approximately 0.4 years. The next three designs are one, two, and four-year interventions. As in the previous table, effect sizes during treatment are taken directly from the parameter estimates in Table 3. Because Table 4 shows that there is no statistically significant decay between reports after the initial period, we assume the long-run decay rate δ^{LR} estimated in Table 5 for each of these programs that last one year or longer. One might hypothesize that δ^{LR} decreases continuously in absolute value with the length of the intervention, and with additional empirical experimentation, this assumption could be refined.

Under these assumptions, the first design, a one-shot intervention, is by far the least cost-effective, at 4.52 cents/kWh. Furthermore, if we additionally assumed that the intervention has fixed costs, these costs would all load onto this one report, further worsening cost effectiveness. Increasing the length of the intervention to four years improves cost effectiveness to 1.58 cents/kWh.

We conceptually distinguish two fundamental channels through which repeated intervention changes outcomes. First, holding constant the intensity of consumers' responses, repeated intervention can change the *composition* of those responses to changes in capital stock. In other words, holding constant the effect size during the intervention, repeating the intervention can increase the persistence of the effect after the intervention is discontinued. As a tangible example, receiving one report might cause consumers to turn off their lights every night - until they lose motivation. Receiving multiple reports might eventually cause consumers to buy an automatic switch that turns off the lights for them every night.

The second channel is that holding constant the decay rate, repeated intervention increases the *intensity* of responses during treatment. This effect might act on the intensive margin within households or on extensive margin across households: the same consumers may become motivated to turn off more lights each night, or additional consumers may eventually become motivated to begin conserving.

The bottom panel of Table 10 illustrates the incremental effects of repeated intervention through these two channels. Lengthening the intervention from one report to one year reduces the decay rate from δ to δ^{LR}. If this had not occurred, savings after the year of treatment would have been δ/δ^{LR} the post-treatment savings actually observed. This composition effect adds up to 238 kWh. Repeated intervention also increases the absolute value of the ATE during treatment from 0.3 to 0.45. Had this not occurred, savings during the year of treatment would have been 2/3 of the savings actually observed. This intensity effect adds up to 56 kWh. Total incremental savings is 429 kWh per household, and incremental cost is $8.10, meaning that incremental intervention has a cost effectiveness of 1.88 cents/kWh.

Given that we assume no change in δ after the first year, there is no composition effect as the treatment duration increases from one to two years. Instead, all of the changes act through increasing the intensity of the response and the residual, which reflects the mechanical effects of increasing the duration of treatment at the same ATE. In this example, the composition effect - inducing consumers to change "capital stock" so as to generate persistent effects - is the most significant driver of improved cost effectiveness. After the composition effect is exhausted, incremental treatment is also cost effective, but this is because of the intensity effect: in this case, treatment effects during the intervention remain large.

7 Conclusion

In this paper, we study one part of a massive and policy-relevant set of randomized control trials designed to encourage people to conserve energy. Aside from the specific relevance of our results to policymakers and economists involved in the energy industry, we believe that there are four broader takeaways for behavioral scientists. First, our data provide a clear illustration of how individuals can be repeatedly stimulated to action by continued interventions, but they repeatedly backslide over time. Second, our data also show clearly how repeated intervention eventually causes changes in the composition of people's responses, which can result in more persistent effects. Persistent changes in outcomes might result from habitual behavior change, or they may result from changes in physical capital or other technologies that change outcomes without additional action. For example, repeated encouragement to increase savings rates could eventually cause people to get in the habit of transferring money to 401ks every month, or it could alternatively cause people to change their default allocations. The resulting increase in savings is the same, as is the decrease in energy use in our example.

Third, our cost effectiveness analysis provides simple evidence on how long-run persistence can materially change whether a program is cost-effective or not. This suggests that in some cases, it may be worthwhile to delay decisions about scaling up a program until long-run effects can be measured. Fourth, we conceptually distinguish between two benefits of repeated interventions, the intensity effect and the composition effect. In our example, we show that the greatest improvement in cost effectiveness happens as the intervention is continued long enough for the composition effect to kick in. This suggests that an important part of the future research agenda on behavioral interventions is to more carefully identify when and how people form new "capital stocks" that cause persistent changes in outcomes.

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Tables

Table 1: Descriptive Statistics

Location	
Region	West
Average January Heating Degrees	25.0
Average July Cooling Degrees	2.2
Narrative	
Baseline period begins	January 1, 2007
First reports generated	October 8, 2008
Last report generated for dropped group	September 10, 2010
End of sample	August 31, 2012
Number of Households	
Treatment: Continued	23,399
Treatment: Dropped	11,543
Control	43,945
Total	78,887
Number of Observations	4,988,798
Baseline Usage (kWh/day)	
Mean	30.3
Standard deviation	13.50
Treatment - Control	0.045
(Standard error)	(0.097)
Dropped - Continued	0.062
(Standard error)	(0.154)
Inactive Households	
Share of Households	0.20
Treatment - Control	0.00071
(Standard error)	(0.0029)
Dropped - Continued	-0.00605
(Standard error)	(0.0045)
Opting Out of Treatment	
Share of treatment group that opts out	0.018
Share of continued group that opts out after October 2010	0.0055

Table 2: Descriptive Statistics for Short-Run Analysis

Narrative			
Wave	1	1	2
Start Date	October 8, 2008	October 8, 2008	February, 2011
Frequency	Monthly	Quarterly	Bimonthly
Number of Households			
Treatment	24,851	9,923	21,970
Control	33,003	10,995	21,891
Total	57,854	20,918	43,861
Number of Observations	102,285,975	36,716,378	61,081,187

Table 3: Short-Run Effects at Arrival Window

Table 3a: First Four Reports

	Monthly	Monthly DD's	Quarterly	Quarterly DD's	Bimonthly	Bimonthly DD's
	(1)	(2)	(3)	(4)	(5)	(6)
TS^a	0.248	0.247	0.151	0.146	0.027	0.106
	(0.028)***	(0.028)***	(0.031)***	(0.03)***	(0.052)	(0.034)***
TS^0	-.079	-.058	-.079	-.095	-.049	-.047
	(0.024)***	(0.023)**	(0.029)***	(0.028)***	(0.033)	(0.029)
TS^1	-.192	-.220	-.218	-.248	-.162	-.188
	(0.03)***	(0.028)***	(0.041)***	(0.034)***	(0.038)***	(0.035)***
T	-.556	-.542	-.393	-.469	-.362	-.472
	(0.065)***	(0.119)***	(0.067)***	(0.091)***	(0.059)***	(0.098)***
$T{\cdot}1(CDD > 0)$				-.00007		-.045
				(0.039)		(0.052)
$T{\cdot}CDD$				0.019		0.008
				(0.01)*		(0.014)
$T{\cdot}1(0 < HDD \leq 5)$				0.042		0.101
				(0.04)		(0.038)***
$T{\cdot}1(5 < HDD \leq 35)$		0.274		0.036		0.355
		(0.166)*		(0.075)		(0.083)***
$T{\cdot}HDD \cdot 1(5 < HDD \leq 35)$		-.013		0.002		-.017
		(0.004)***		(0.006)		(0.009)*
$T{\cdot}1(HDD > 35)$				0.314		-.140
				(0.21)		(0.322)
Obs.	8515691	8515691	1.93e+07	1.93e+07	9610563	9610563

Table 3b: After First Four Reports

	Monthly	Monthly DD's	Quarterly	Quarterly DD's	Bimonthly	Bimonthly DD's
	(1)	(2)	(3)	(4)	(5)	(6)
TS^a	0.102	0.095	0.057	0.042	0.02	0.039
	(0.014)***	(0.013)***	(0.023)**	(0.021)**	(0.068)	(0.068)
TS^0	-.031	-.034	-.017	-.014	-.070	-.162
	(0.008)***	(0.007)***	(0.02)	(0.02)	(0.047)	(0.05)***
TS^1	-.049	-.051	-.058	-.042	-.263	-.277
	(0.01)***	(0.009)***	(0.023)**	(0.024)*	(0.06)***	(0.061)***
T	-.815	-.778	-.627	-.498	-.755	2.115
	(0.059)***	(0.062)***	(0.092)***	(0.117)***	(0.096)***	(0.315)***
$T{\cdot}1(CDD > 0)$		-.011		-.041		-9.438
		(0.022)		(0.05)		(1.109)***
$T{\cdot}CDD$		0.004		0.006		0.998
		(0.007)		(0.012)		(0.298)***
$T{\cdot}1(0 < HDD \leq 5)$		0.042		-.017		-2.273
		(0.024)*		(0.055)		(0.278)***
$T{\cdot}1(5 < HDD \leq 35)$		0.108		0.066		-2.247
		(0.046)**		(0.087)		(0.312)***
$T{\cdot}HDD \cdot 1(5 < HDD \leq 35)$		-.009		-.012		-.031
		(0.004)**		(0.006)**		(0.009)***
$T{\cdot}1(HDD > 35)$		-.121		-.082		-3.313
		(0.128)		(0.188)		(0.405)***
Obs.	7.00e+07	7.00e+07	4.37e+07	4.37e+07	9352415	9352415

Notes: Tables 3a and 3b present the estimates of Equation (3) for the first four reports and all remaining reports, respectively. In each pair of estimates, the left column does not control for degree days, while the right column does. The outcome variable is electricity use, in kilowatt-hours per day. Standard errors are robust, clustered by household. *, **, ***: Statistically significant with 90, 95, and 99 percent confidence, respectively.

Table 4: Short-Run Effects Between Reports

Table 4a: First Four Reports

	Monthly	Monthly DD's	Quarterly	Quarterly DD's	Bimonthly	Bimonthly DD's
	(1)	(2)	(3)	(4)	(5)	(6)
TS^w	-.110	-.203	-.061	-.033	-.055	-.084
	(0.034)***	(0.031)***	(0.036)*	(0.035)	(0.047)	(0.046)*
dTS^w	1.168	4.221	0.774	0.738	1.610	1.445
	(1.265)	(1.310)***	(0.196)***	(0.191)***	(0.423)***	(0.408)***
T	-.416	-.459	-.399	-.498	-.403	-.469
	(0.064)***	(0.119)***	(0.072)***	(0.093)***	(0.066)***	(0.103)***
$\text{T}\cdot 1(CDD > 0)$				-.004		-.030
				(0.039)		(0.051)
$\text{T}\cdot CDD$				0.018		0.008
				(0.011)*		(0.014)
$\text{T}\cdot 1(0 < HDD \leq 5)$				0.036		0.105
				(0.04)		(0.039)***
$\text{T}\cdot 1(5 < HDD \leq 35)$		0.382		0.03		0.338
		(0.168)**		(0.074)		(0.085)***
$\text{T}\cdot HDD \cdot 1(5 < HDD \leq 35)$		-.015		0.003		-.015
		(0.004)***		(0.006)		(0.009)*
$\text{T}\cdot 1(HDD > 35)$				0.283		-.084
				(0.21)		(0.324)
Obs.	8515691	8515691	1.93e+07	1.93e+07	9610563	9610563

Table 4b: After First Four Reports

	Monthly	Monthly DD's	Quarterly	Quarterly DD's	Bimonthly	Bimonthly DD's
	(1)	(2)	(3)	(4)	(5)	(6)
TS^w	0.024	0.019	0.039	0.053	-.485	-.340
	(0.012)**	(0.011)*	(0.036)	(0.036)	(0.105)***	(0.079)***
dTS^w	0.511	0.503	0.112	0.114	0.249	0.434
	(0.33)	(0.321)	(0.149)	(0.15)	(0.542)	(0.534)
T	-.783	-.748	-.661	-.541	-.563	2.115
	(0.056)***	(0.061)***	(0.09)***	(0.118)***	(0.092)***	(0.315)***
$\text{T}\cdot 1(CDD > 0)$		-.015		-.042		-9.440
		(0.022)		(0.05)		(1.109)***
$\text{T}\cdot CDD$		0.005		0.005		0.997
		(0.007)		(0.012)		(0.298)***
$\text{T}\cdot 1(0 < HDD \leq 5)$		0.044		-.024		-2.272
		(0.024)*		(0.055)		(0.278)***
$\text{T}\cdot 1(5 < HDD \leq 35)$		0.11		0.067		-2.377
		(0.046)**		(0.088)		(0.31)***
$\text{T}\cdot HDD \cdot 1(5 < HDD \leq 35)$		-.009		-.013		-.019
		(0.004)**		(0.006)**		(0.008)**
$\text{T}\cdot 1(HDD > 35)$		-.108		-.096		-3.009
		(0.127)		(0.187)		(0.391)***
Obs.	7.00e+07	7.00e+07	4.37e+07	4.37e+07	9352415	9352415

Notes: Tables 4a and 4b present the estimates of Equation (4) for the first four reports and all remaining reports, respectively. In each pair of estimates, the left column does not control for degree days, while the right column does. The outcome variable is electricity use, in kilowatt-hours per day. Standard errors are robust, clustered by household. *, **, ***: Statistically significant with 90, 95, and 99 percent confidence, respectively.

Table 5: Household-Level Repeated Effects

	Monthly	Quarterly	Bimonthly	Combined	Controls	Exclude Outliers
	(1)	(2)	(3)	(4)	(5)	(6)
$T\Delta Y_{h-1}$	0.022	0.033	0.011	0.02	0.025	0.023
	(0.006)***	(0.014)**	(0.015)	(0.006)***	(0.007)***	(0.005)***
ΔY_{h-1} (Monthly)	-.106			-.105	-.107	-.105
	(0.004)***			(0.004)***	(0.005)***	(0.004)***
ΔY_{h-1} (Quarterly)		-.045		-.041	-.042	-.041
		(0.009)***		(0.008)***	(0.008)***	(0.007)***
ΔY_{h-1} (Bimonthly)			0.052	0.047	0.045	0.038
			(0.01)***	(0.008)***	(0.008)***	(0.006)***
T (Monthly)	-.112			-.115		-.090
	(0.043)***			(0.043)***		(0.042)**
T (Quarterly)		-.127		-.132		-.118
		(0.064)**		(0.064)**		(0.064)*
T (Bimonthly)			-.053	-.039		-.040
			(0.043)	(0.039)		(0.036)
Obs.	178959	91277	115631	385867	385867	385472

Notes: This table presents the estimates of Equation (5). The outcome variable is change in electricity use after vs. before report arrival, in kilowatt-hours per day. Standard errors are robust, clustered by household. *, **, ***: Statistically significant with 90, 95, and 99 percent confidence, respectively.

Table 6: Long-Run Effects

	Levels	Changes	Weather	Trends	Controls	Balanced
	(1)	(2)	(3)	(4)	(5)	(6)
TP^0	-.025	-.025	-.025	-.025	-.025	-.023
	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.033)
TP^1	-.452	-.452	-.452	-.452	-.452	-.466
	(0.043)***	(0.043)***	(0.043)***	(0.043)***	(0.043)***	(0.044)***
TP^2	-.660			-.660	-.617	-.657
	(0.051)***			(0.051)***	(0.068)***	(0.071)***
$T{\cdot}(P^2 + P^3)$		-.660	-.595			
		(0.051)***	(0.068)***			
EP3	-.842	-.181	-.247	-.842	-.842	-.837
	(0.068)***	(0.053)***	(0.071)***	(0.068)***	(0.068)***	(0.07)***
DP3	-.612	0.049	0.053	-.489	-.456	-.448
	(0.087)***	(0.076)	(0.075)	(0.101)***	(0.102)***	(0.102)***
DrP^3				0.131	0.117	0.127
				(0.058)**	(0.056)**	(0.051)**
$HDD{\cdot}(TP^2 + DP^3)$			-.004		-.003	-.002
			(0.004)		(0.004)	(0.004)
$CDD{\cdot}(TP^2 + DP^3)$			-.012		-.008	0.005
			(0.025)		(0.025)	(0.027)
Obs.	4042155	4042155	4042155	4042155	4042155	3526102

Notes: This table presents the estimates of Equation (7). The outcome variable is monthly average electricity use, in kilowatt-hours per day. Standard errors are robust, clustered by household. *, **, ***: Statistically significant with 90, 95, and 99 percent confidence, respectively.

Table 7: Program Participation

	Savings (kWh/day)	Number Installed	Continued - Control (kWh/day)	Treatment - Control (kWh/day)	First Year: Continued-Control kWh/day
	(1)	(2)	(3)	(4)	(5)
Clothes Washer	0.35	1606	0.00035	0.00024	0.00024
			(0.0004)	(0.00035)	(0.00023)
Compact Fluorescent Lightbulbs	2.23	260	0.00224 **	0.00126	0.00051
			(0.00113)	(0.00096)	(0.00038)
Refrigerator Decommissioning	1.32	250	0.0004	0.00038	-0.00013
			(0.00058)	(0.00051)	(0.00022)
Showerhead	0.22	187	0.00008	0.00007	0.00003
			(0.00009)	(0.00007)	(0.00003)
Freezer Decommissioning	1.52	102	0.00037	0.00057	0.00018
			(0.00044)	(0.00039)	(0.00017)
Heat Pump	1.61	54	-0.00019	-0.00014	-0.00003
			(0.00037)	(0.00036)	(0.00016)
Water Heater	8.20	28	-0.00099	-0.00091	-0.00099
			(0.00116)	(0.00105)	(0.00065)
New Refrigerator	1.80	7	-0.0001	-0.00013	-0.00004
			(0.00013)	(0.00011)	(0.00004)
Windows	12.2	5	0.00089	0.00053	0.00007
			(0.00082)	(0.00056)	(0.00007)
Conversion to Gas Heat	28.1	1	-0.00064	-0.00064	-0.00009
			(0.00064)	(0.00064)	(0.00009)
All		2500	0.00241	0.00123	-0.0002
			(0.00219)	(0.00191)	(0.00094)

Notes: This table presents data on participation in the utility's energy conservation programs for calendar year 2011. Standard errors are robust. *, **, ***: Statistically significant with 90, 95, and 99 percent confidence, respectively.

Table 8: Self-Reported Actions

	All Sites			This Site		
	Mean	T-C	T-C|X	Mean	T-C	T-C|X
"In the past twelve months, have you..."						
Taken any steps to reduce energy use?	0.77	0.010 (0.012)	-0.001 (0.015)	0.81	-0.055 (0.030)*	-0.035 (0.035)
Repeated Actions	0.62	0.005 (0.008)	0.011 (0.010)	0.59	0.004 (0.027)	0.011 (0.030)
Adjusted your thermostat settings?	0.63	0.012 (0.015)	0.007 (0.019)			
Unplugged devices and chargers?	0.65	-0.020 (0.039)	-0.013 (0.044)	0.65	-0.020 (0.039)	-0.013 (0.044)
Switched off power strips or appliances when unused?	0.59	0.002 (0.014)	0.011 (0.018)	0.51	0.013 (0.041)	0.022 (0.047)
Turned off lights when unused?	0.96	0.005 (0.009)	0.006 (0.010)			
Hung laundry to dry?	0.42	0.010 (0.024)	0.001 (0.027)			
Used energy saving or sleep features on your computer?	0.56	0.008 (0.021)	0.021 (0.029)			
Turned off computer at night?	0.65	-0.034 (0.023)	-0.030 (0.026)	0.60	0.018 (0.040)	0.025 (0.046)
Used fans to keep cool?	0.80	0.072 (0.034)**	0.086 (0.039)**			
Physical Capital Changes	0.55	-0.002 (0.008)	0.002 (0.010)	0.54	-0.003 (0.017)	0.014 (0.019)
Replaced incandescent light bulbs with LEDs?	0.70	0.013 (0.038)	0.016 (0.043)	0.70	0.013 (0.038)	0.016 (0.043)
Purchased Energy Star appliances?	0.74	0.002 (0.016)	0.019 (0.022)	0.77	0.012 (0.035)	0.063 (0.039)
Disposed of a second refrigerator or freezer?	0.26	-0.001 (0.015)	0.030 (0.019)	0.16	0.013 (0.029)	0.030 (0.033)
Installed light timers or sensors?	0.30	-0.018 (0.038)	-0.014 (0.043)	0.30	-0.018 (0.038)	-0.014 (0.043)
Replaced incandescent light bulbs with CFLs?	0.81	0.000 (0.013)	-0.017 (0.017)			
Added insulation or replaced windows?	0.54	-0.039 (0.024)	-0.055 (0.029)*			
Had a home energy audit?	0.19	0.057 (0.022)***	0.058 (0.026)**			
Installed a programmable thermostat?	0.79	-0.033 (0.032)	-0.025 (0.037)	0.79	-0.033 (0.032)	-0.025 (0.037)
Intermittent Actions	0.62	0.006 (0.012)	0.007 (0.017)	0.56	-0.005 (0.031)	0.000 (0.034)
Tuned up your AC system?	0.63	-0.016 (0.018)	-0.014 (0.024)	0.61	-0.032 (0.040)	-0.050 (0.044)
Used a programmable thermostat?	0.59	0.009 (0.028)	0.076 (0.047)			
Added weather-stripping or caulking around windows?	0.60	0.008 (0.018)	-0.009 (0.025)	0.51	0.022 (0.041)	0.050 (0.047)
Cleaned or replaced heating or AC system air filters?	0.70	0.017 (0.038)	0.010 (0.044)			
Participated in any utility energy effiency programs?	0.19	0.018 (0.010)*	0.010 (0.013)	0.61	0.007 (0.040)	0.028 (0.046)
N	5856			800		

Notes: This table presents survey data on self-reported energy conservation actions. The three columns at left present aggregated results across six sites, while the three on the right present results from the utility we study in the rest of the paper. Standard errors are robust. *, **, ***: Statistically significant with 90, 95, and 99 percent confidence, respectively.

Table 9: In-Sample Cost Effectiveness Estimates

Scenario	1	2	3	4
Discontinue Reports?	No	Yes	Yes	Yes
Assumed Persistence	-	Observed	Zero	Full
Electricity Savings and Costs				
Total electricity savings during treatment (kWh)	995	406	406	406
(Standard Error)	(53.5)	(24.3)	(24.3)	(24.3)
Total electricity savings after treatment	0	428.4	0	462
(Standard Error)	(0)	(60.9)	(0)	(35.7)
Total savings (kWh)	995	834	406	868
	(53.5)	(65.6)	(24.3)	(43.2)
Total cost ($)	32.1	18.0	18.0	18.0
Cost Effectiveness				
Cost Effectiveness (cents/kWh)	3.23	2.15	4.42	2.07
(Standard Error)	(0.17)	(0.17)	(0.26)	(0.1)
Electricity Cost Savings				
Per household electricity savings ($)	100	83	41	87
(Standard Error)	(5.4)	(6.6)	(2.4)	(4.3)
Treatment group electricity savings ($millions)	3.48	2.92	1.42	3.03
(Standard Error)	(0.19)	(0.23)	(0.08)	(0.15)

Notes: See text for details.

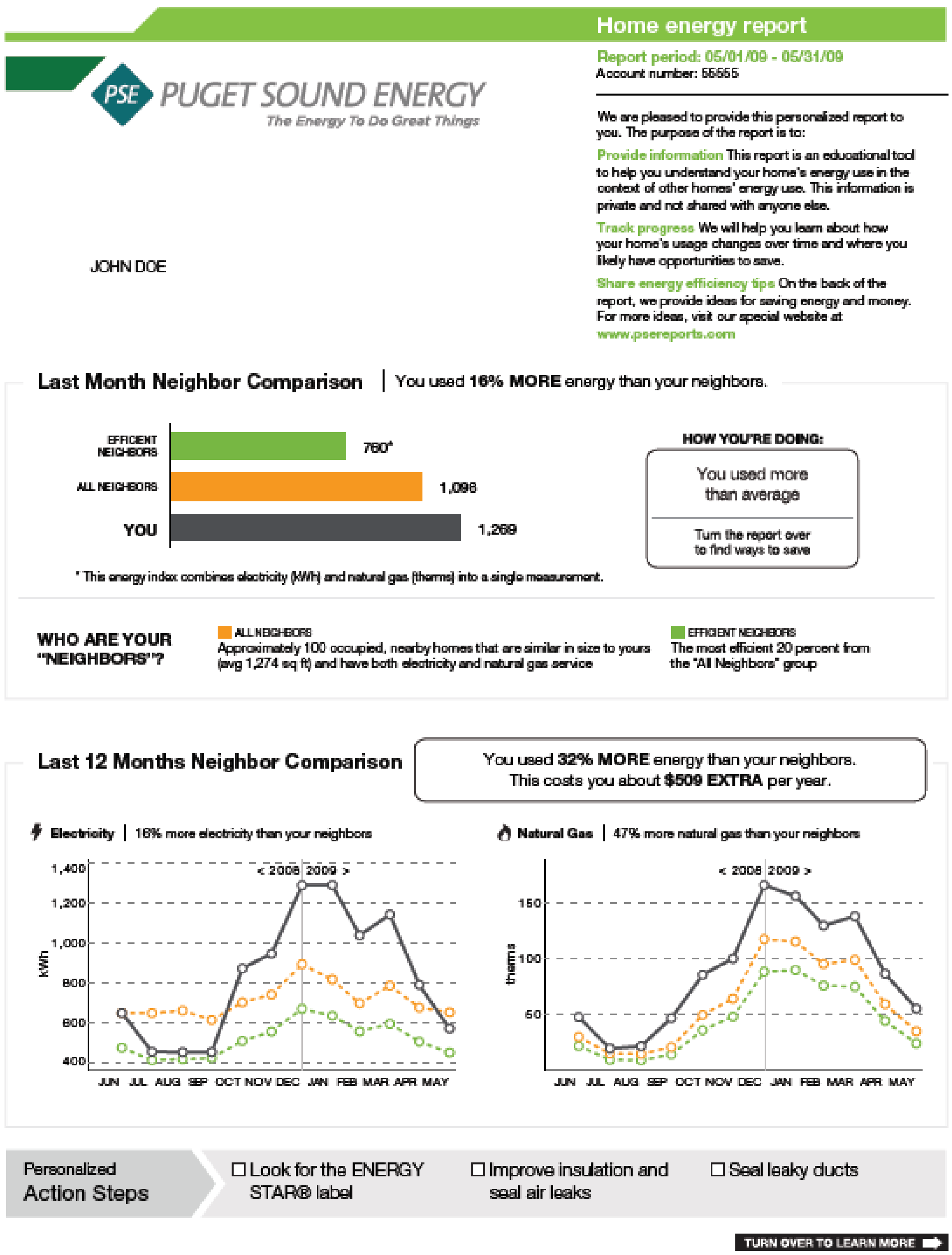
Table 10: Cost Effectiveness and Program Design

Design	1	2	3	4
Length of hypothetical program	One report	One year	Two years	Four years
Savings and Costs				
Savings during treatment (kWh PDV)	0	167	398	948
|ATE| at end of treatment (kWh/day)	0.30	0.45	0.66	0.84
Decay rate (kWh/day per year)	0.75	0.12	0.12	0.12
Years from end of treatment to zero effect	0.40	3.8	5.5	7.0
Savings after treatment (kWh PDV)	22	284	559	813
Total savings (kWh PDV)	22	450	958	1,760
Total cost ($ PDV)	1.0	9.1	17.7	30.4
Cost Effectiveness (cents/kWh)	4.61	2.01	1.85	1.72
Incremental Effects				
Composition effect (kWh PDV)		238	0	0
Intensity effect (kWh PDV)		56	125	205
Residual effect (kWh PDV)		134	382	598
Incremental savings (kWh PDV)	22	429	507	803
Incremental cost ($ PDV)	1.0	8.1	8.6	12.7
Incremental cost-effectiveness (cents/kWh)	4.61	1.88	1.70	1.58

Notes: See text for details.

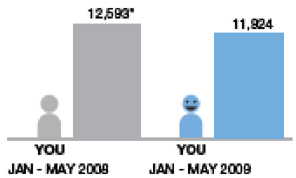
Figures

Figure 1: Home Energy Report



Personal Comparison

How you're doing compared to last year:



* This energy index combines electricity (kWh) and natural gas (therms) into a single measurement.

So far this year, you used **5% LESS** energy than last year.

★ You're on pace to use less in 2009.

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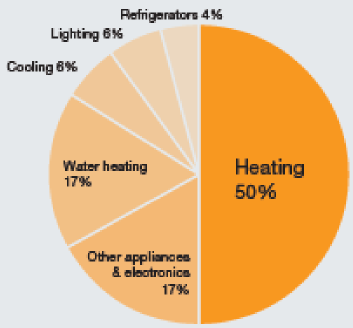
Understanding your energy use

Heating is the largest use of energy for a typical household in the Puget Sound area, accounting for up to 50% of total energy use. To maximize your savings, focus on the biggest users first.

Typical annual energy use in the Puget Sound area*



Other appliances & electronics include dishwashers, washing machines, dryers, computers, TVs & entertainment systems.

*Based on a typical household with air conditioning.

Top Tips For Saving	Save up to
☐ **Look for the ENERGY STAR® label** Next Steps: Look for the ENERGY STAR label when shopping for appliances and electronics.	$600/yr
☐ **Improve insulation and seal air leaks** Next Steps: Start with the places easiest to access, such as an attic.	$305/yr
☐ **Seal leaky ducts** Next Steps: Use mastic (a special adhesive) or duct tape to seal all accessible duct joints.	$170/yr
☐ **Recycle your second refrigerator** Next Steps: Try rearranging your main fridge to fit everything from your second fridge.	$145/yr
☐ **Turn off computer at night** Next Steps: Program your computer to automatically turn off after periods of inactivity.	$75/yr
☐ **Set your thermostat wisely** Next Steps: Set your thermostat 10 degrees off from your preferred setting when you're away or sleeping.	$65/yr
☐ **Install efficient showerheads** Next Steps: Get a new efficient showerhead and bathroom faucet aerator for free! Visit psereports.com for details.	$45/yr

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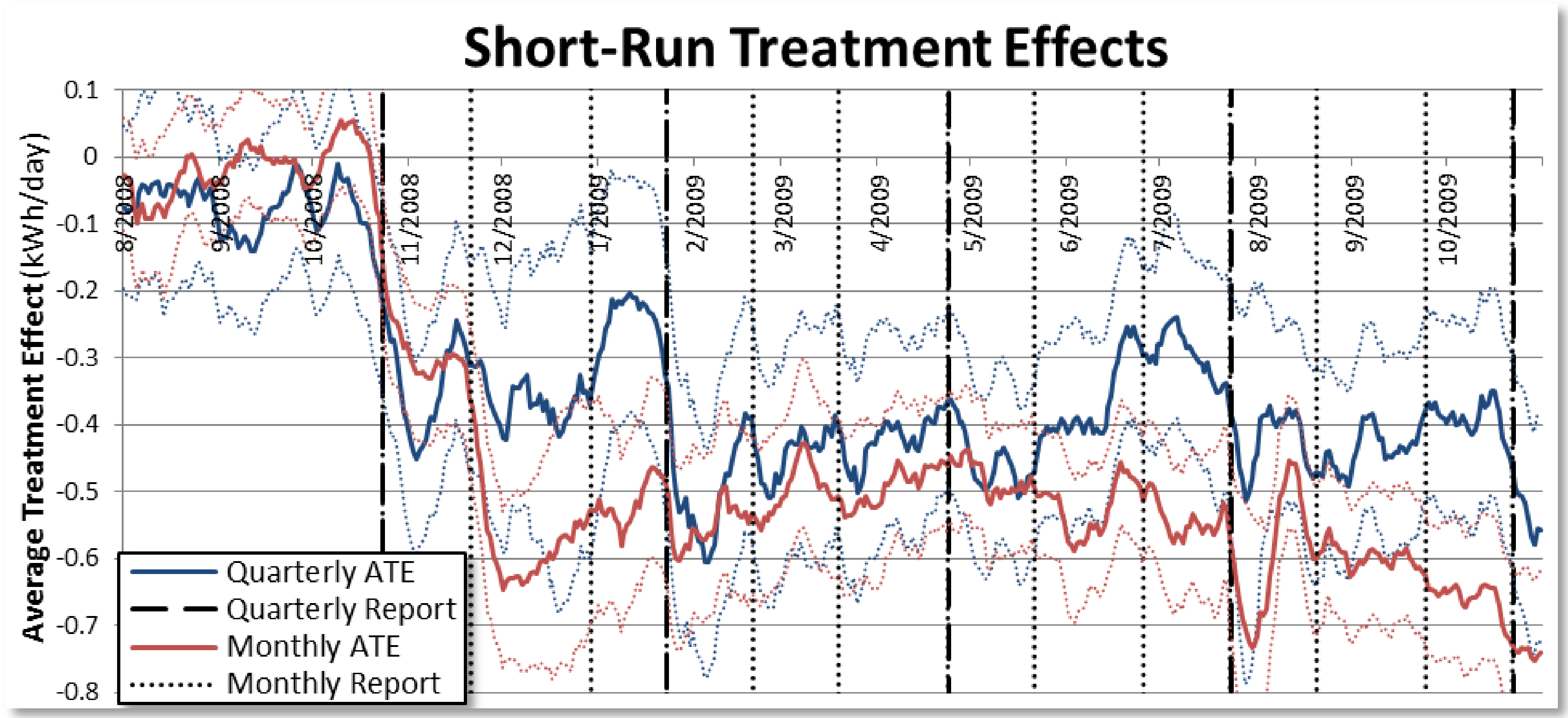
Figure 2: Short-Run Treatment Effects



Notes: This figure plots the smoothed ATEs for each day of the first year of treatment for the monthly and quarterly treatment groups, as estimated by Equation (1). The dotted lines reflect 90 percent confidence intervals, with robust standard errors clustered by household.

Figure 3: Short-Run Effects in Event Time

Figure 3a: First Four Reports

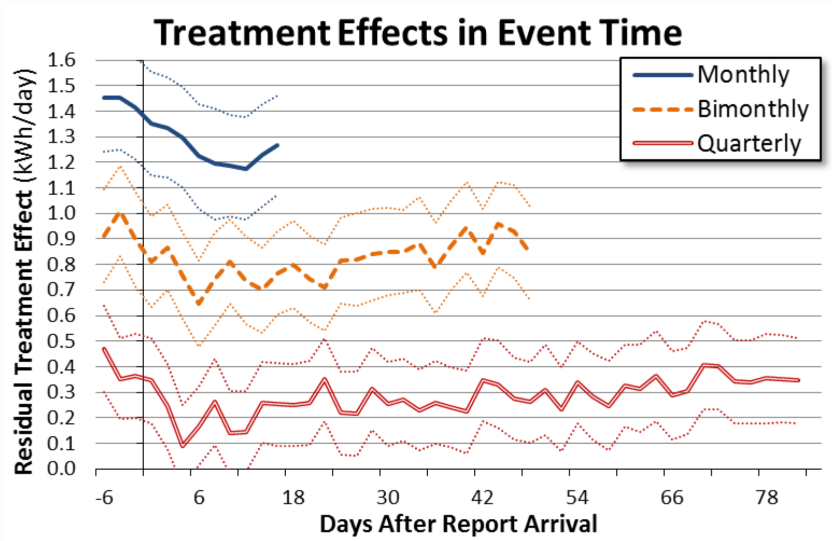
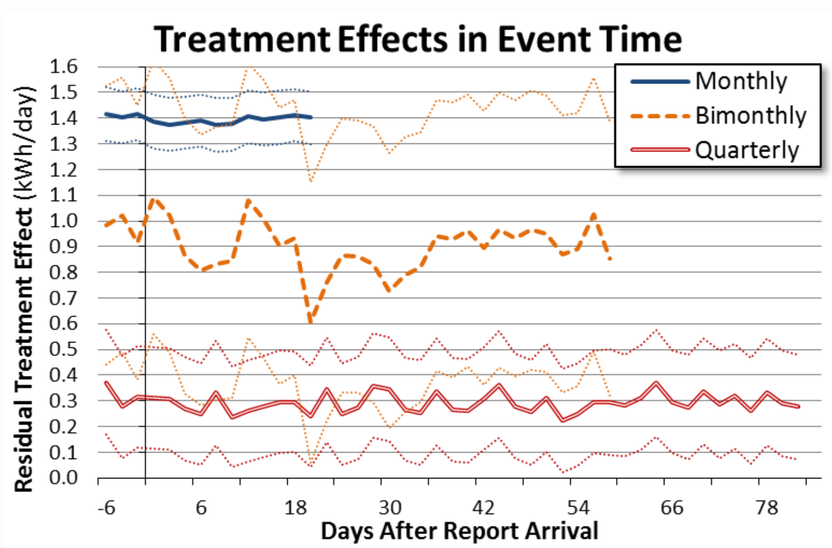
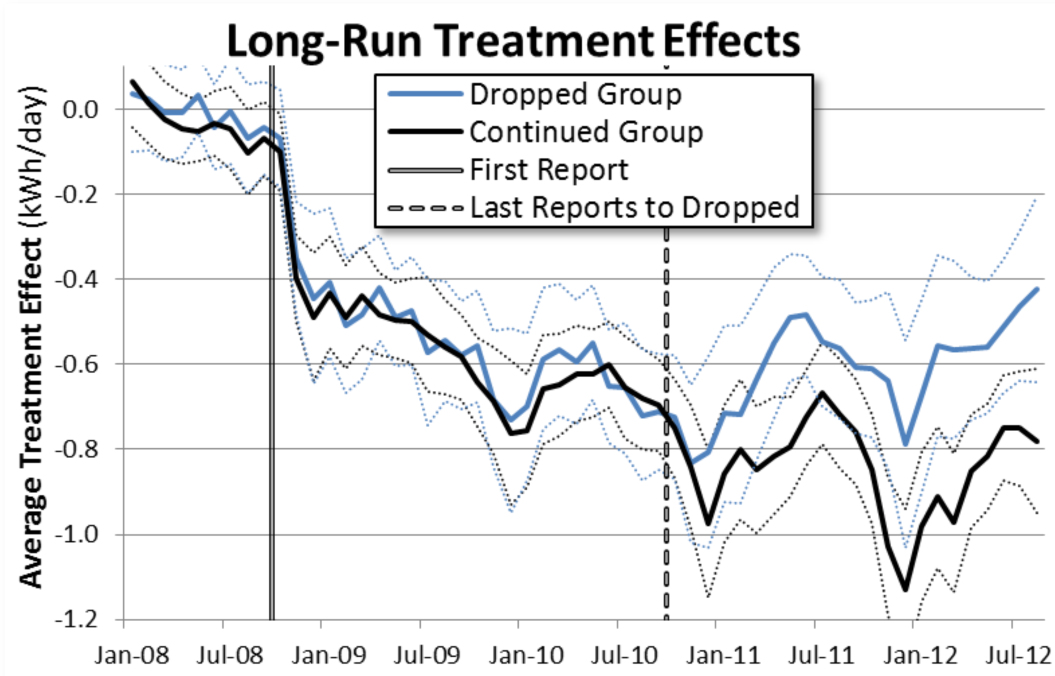


Figure 3b: After First Four Reports



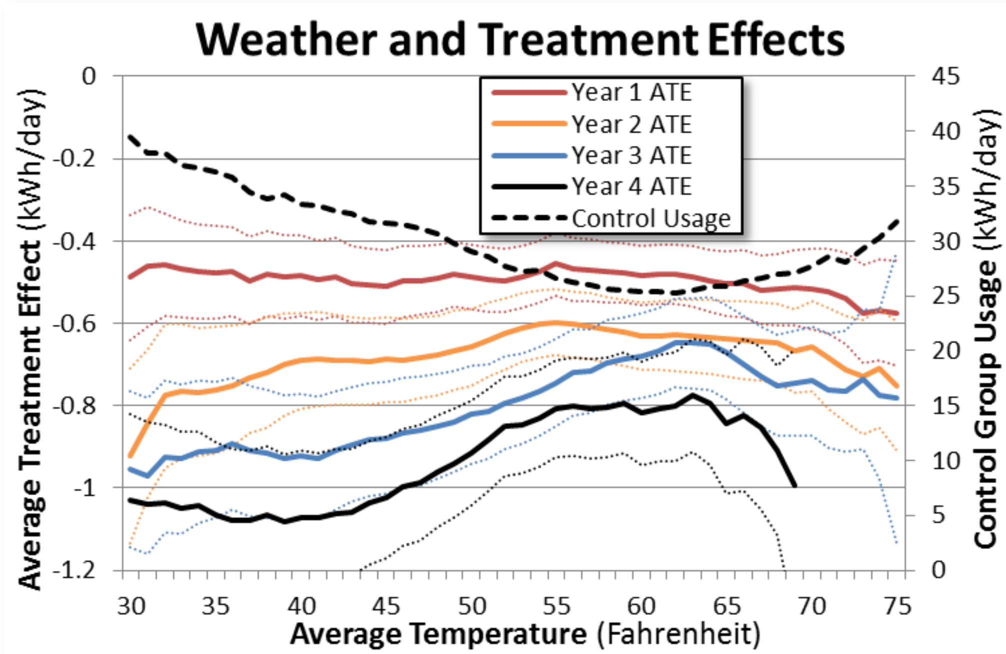
Notes: Figures 3a and 3b plot the ATEs in event time for the first four reports and all remaining reports, respectively, as estimated by Equation (2). The dotted lines reflect 90 percent confidence intervals, with robust standard errors clustered by household.

Figure 4: Long-Run Treatment Effects



Notes: This figure plots the ATEs for each month of the sample for the continued and dropped groups, estimated by Equation (6). The dotted lines reflect 90 percent confidence intervals, with robust standard errors clustered by household.

Figure 5: Weather Effects



Notes: The dashed line plots average control group consumption as a function of daily mean temperature over the sample period. The four solid lines plot ATEs for each of the four post-treatment years, smoothed using a rectangular kernel with halfwidth of two degrees. The dotted lines reflect 90 percent confidence intervals, with robust standard errors clustered by household.

Appendix: For Online Publication

The Short-Run and Long-Run Effects of Behavioral Interventions: Experimental Evidence from Energy Conservation

Hunt Allcott and Todd Rogers

8 Appendix Tables

Table A1: Short-Run Effects at Arrival Window

Table A1a: First Four Reports

	M Outliers	M YControl	Q Outliers	Q YControl	B Outliers	B YControl
	(1)	(2)	(3)	(4)	(5)	(6)
TS^a	0.244	0.245	0.142	0.152	0.107	0.083
	(0.028)***	(0.028)***	(0.03)***	(0.03)***	(0.033)***	(0.034)**
TS^0	-.059	-.069	-.095	-.093	-.047	-.037
	(0.023)***	(0.023)***	(0.028)***	(0.028)***	(0.028)*	(0.029)
TS^1	-.217	-.207	-.241	-.257	-.189	-.161
	(0.028)***	(0.029)***	(0.034)***	(0.036)***	(0.034)***	(0.035)***
T	-.566	0.545	-.463	-.636	-.427	0.5
	(0.117)***	(0.331)*	(0.09)***	(0.294)**	(0.094)***	(0.326)
$T{\cdot}1(CDD > 0)$			-.0002	0.002	-.025	-.035
			(0.039)	(0.039)	(0.049)	(0.052)
$T{\cdot}CDD$			0.019	0.015	0.002	0.008
			(0.01)*	(0.011)	(0.013)	(0.014)
$T{\cdot}1(0 < HDD \leq 5)$			0.039	0.044	0.104	0.097
			(0.04)	(0.04)	(0.036)***	(0.038)**
$T{\cdot}1(5 < HDD \leq 35)$	0.308	-.191	0.035	0.056	0.329	0.149
	(0.163)*	(0.162)	(0.075)	(0.074)	(0.079)***	(0.066)**
$T{\cdot}HDD \cdot 1(5 < HDD \leq 35)$	-.014	-.003	0.002	-.0004	-.017	0.01
	(0.004)***	(0.004)	(0.006)	(0.006)	(0.009)*	(0.006)*
$T{\cdot}1(HDD > 35)$			0.264	0.204	-.103	0.792
			(0.202)	(0.188)	(0.309)	(0.222)***
$T{\cdot}\overline{Y}^{T==0}$		-.025		0.006		-.042
		(0.009)***		(0.01)		(0.012)***
Obs.	8514078	8515691	1.93e+07	1.93e+07	9590651	9610563

Table A1b: After First Four Reports

	M Outliers	M YControl	Q Outliers	Q YControl	B Outliers	B YControl
	(1)	(2)	(3)	(4)	(5)	(6)
TS^a	0.095	0.076	0.042	0.042	0.018	0.155
	(0.013)***	(0.012)***	(0.021)**	(0.021)**	(0.065)	(0.068)**
TS^0	-.032	-.034	-.012	-.014	-.161	-.001
	(0.007)***	(0.007)***	(0.02)	(0.02)	(0.049)***	(0.049)
TS^1	-.048	-.039	-.040	-.041	-.264	-.195
	(0.009)***	(0.009)***	(0.024)*	(0.024)*	(0.06)***	(0.06)***
T	-.771	-.029	-.500	-.524	2.166	4.458
	(0.062)***	(0.181)	(0.116)***	(0.304)*	(0.299)***	(0.443)***
$\text{T}\cdot 1(CDD > 0)$	-.012	-.026	-.037	-.041	-9.597	-9.415
	(0.022)	(0.022)	(0.05)	(0.05)	(1.079)***	(1.109)***
$\text{T}\cdot CDD$	0.004	0.019	0.006	0.006	1.030	1.011
	(0.007)	(0.007)***	(0.012)	(0.012)	(0.296)***	(0.298)***
$\text{T}\cdot 1(0 < HDD \leq 5)$	0.043	0.033	-.012	-.016	-2.321	-2.225
	(0.024)*	(0.024)	(0.055)	(0.055)	(0.26)***	(0.278)***
$\text{T}\cdot 1(5 < HDD \leq 35)$	0.112	0.018	0.065	0.069	-2.291	-2.653
	(0.045)**	(0.045)	(0.087)	(0.091)	(0.295)***	(0.308)***
$\text{T}\cdot HDD \cdot 1(5 < HDD \leq 35)$	-.010	0.003	-.012	-.013	-.031	0.033
	(0.004)**	(0.004)	(0.006)**	(0.006)**	(0.008)***	(0.006)***
$\text{T}\cdot 1(HDD > 35)$	-.138	0.241	-.069	-.095	-3.343	-1.139
	(0.125)	(0.119)**	(0.186)	(0.173)	(0.382)***	(0.371)***
$\text{T}\cdot \overline{Y}^{T==0}$		-.029		0.001		-.101
		(0.007)***		(0.01)		(0.012)***
Obs.	7.00e+07	7.00e+07	4.37e+07	4.37e+07	9332423	9352415

Notes: Tables A1a and A1b present the estimates of Equation (3) for the first four reports and all remaining reports, respectively. In each pair of estimates, the left column excludes outliers, while the right column controls for the interaction of the treatment effect with control group average usage. The outcome variable is electricity use, in kilowatt-hours per day. Standard errors are robust, clustered by household. *, **, ***: Statistically significant with 90, 95, and 99 percent confidence, respectively.

Table A2: Short-Run Effects Between Reports

Table A2a: First Four Reports

	M Outliers	M YControl	Q Outliers	Q YControl	B Outliers	B YControl
	(1)	(2)	(3)	(4)	(5)	(6)
TS^w	-.200	-.174	-.027	-.034	-.081	-.057
	(0.031)***	(0.033)***	(0.035)	(0.035)	(0.043)*	(0.046)
dTS^w	4.118	2.699	0.705	0.737	1.217	1.094
	(1.297)***	(1.294)**	(0.188)***	(0.191)***	(0.375)***	(0.395)***
T	-.485	0.661	-.494	-.532	-.417	0.491
	(0.117)***	(0.327)**	(0.093)***	(0.286)*	(0.097)***	(0.325)
$\text{T}\cdot 1(CDD > 0)$			-.004	-.003	-.014	-.022
			(0.039)	(0.039)	(0.048)	(0.051)
$\text{T}\cdot CDD$			0.019	0.017	0.003	0.009
			(0.011)*	(0.012)	(0.013)	(0.014)
$\text{T}\cdot 1(0 < HDD \leq 5)$			0.033	0.036	0.107	0.099
			(0.04)	(0.04)	(0.037)***	(0.039)**
$\text{T}\cdot 1(5 < HDD \leq 35)$	0.415	-.101	0.029	0.034	0.314	0.137
	(0.164)**	(0.166)	(0.075)	(0.073)	(0.082)***	(0.07)*
$\text{T}\cdot HDD \cdot 1(5 < HDD \leq 35)$	-.015	-.004	0.003	0.002	-.016	0.011
	(0.004)***	(0.005)	(0.006)	(0.005)	(0.009)*	(0.006)*
$\text{T}\cdot 1(HDD > 35)$			0.234	0.26	-.053	0.837
			(0.203)	(0.186)	(0.309)	(0.228)***
$\text{T}\cdot \overline{Y}^{T==0}$		-.026		0.001		-.042
		(0.009)***		(0.01)		(0.012)***
Obs.	8514078	8515691	1.93e+07	1.93e+07	9590651	9610563

Table A2b: After First Four Reports

	M Outliers	M YControl	Q Outliers	Q YControl	B Outliers	B YControl
	(1)	(2)	(3)	(4)	(5)	(6)
TS^w	0.021	0.019	0.052	0.053	-.312	-.088
	(0.011)*	(0.011)*	(0.036)	(0.036)	(0.077)***	(0.074)
dTS^w	0.422	0.535	0.108	0.116	0.152	1.160
	(0.319)	(0.32)*	(0.149)	(0.148)	(0.524)	(0.533)**
T	-.741	0.027	-.541	-.530	2.165	4.417
	(0.061)***	(0.181)	(0.117)***	(0.304)*	(0.299)***	(0.436)***
$\mathrm{T}\cdot 1(CDD > 0)$	-.016	-.030	-.037	-.042	-9.600	-9.414
	(0.022)	(0.022)	(0.05)	(0.051)	(1.079)***	(1.109)***
$\mathrm{T}\cdot CDD$	0.005	0.021	0.006	0.006	1.029	1.011
	(0.007)	(0.007)***	(0.012)	(0.012)	(0.296)***	(0.298)***
$\mathrm{T}\cdot 1(0 < HDD \leq 5)$	0.045	0.034	-.019	-.024	-2.320	-2.224
	(0.024)*	(0.024)	(0.055)	(0.056)	(0.26)***	(0.278)***
$\mathrm{T}\cdot 1(5 < HDD \leq 35)$	0.114	0.014	0.066	0.065	-2.424	-2.620
	(0.045)**	(0.045)	(0.088)	(0.092)	(0.293)***	(0.307)***
$\mathrm{T}\cdot HDD \cdot 1(5 < HDD \leq 35)$	-.010	0.004	-.012	-.013	-.018	0.031
	(0.004)**	(0.004)	(0.006)**	(0.006)**	(0.007)**	(0.007)***
$\mathrm{T}\cdot 1(HDD > 35)$	-.124	0.269	-.083	-.090	-3.053	-1.153
	(0.125)	(0.118)**	(0.186)	(0.173)	(0.369)***	(0.371)***
$\mathrm{T}\cdot\overline{Y}^{T==0}$		-.030		-.0004		-.099
		(0.007)***		(0.01)		(0.011)***
Obs.	7.00e+07	7.00e+07	4.37e+07	4.37e+07	9332423	9352415

Notes: Tables A2a and A2b present the estimates of Equation (4) for the first four reports and all remaining reports, respectively. In each pair of estimates, the left column excludes outliers, while the right column controls for the interaction of the treatment effect with control group average usage. The outcome variable is electricity use, in kilowatt-hours per day. Standard errors are robust, clustered by household. *, **, ***: Statistically significant with 90, 95, and 99 percent confidence, respectively.

Table A3: Effects for Placebo Reports

	Unconditional	Weather
	(1)	(2)
TS^a	0.059	0.05
	(0.021)***	(0.021)**
TS^0	-.026	-.025
	(0.016)	(0.016)
TS^1	-.015	-.019
	(0.02)	(0.02)
T	-.638	-.508
	(0.093)***	(0.118)***
T$\cdot 1(CDD > 0)$		-.037
		(0.05)
T$\cdot CDD$		0.005
		(0.012)
T$\cdot 1(0 < HDD \leq 5)$		-.022
		(0.055)
T$\cdot 1(5 < HDD \leq 35)$		0.07
		(0.087)
T$\cdot HDD \cdot 1(5 < HDD \leq 35)$		-.013
		(0.006)**
T$\cdot 1(HDD > 35)$		-.099
		(0.189)
Obs.	4.37e+07	4.37e+07

Notes: This table presents the estimates of Equation (3) for the quarterly group, for reports that the monthly group received but the quarterly group did not. The sample includes the period after the quarterly group's first four reports. The left column does not control for degree days, while the right column does. The outcome variable is electricity use, in kilowatt-hours per day. Standard errors are robust, clustered by household. *, **, ***: Statistically significant with 90, 95, and 99 percent confidence, respectively.

Table A4: Persistence by Subgroup

	Heterogeneous Effects	Trends	Weather
	(1)	(2)	(3)
D	-.683	-.797	-.701
	(0.099)***	(0.116)***	(0.124)***
Dr^2		0.127	0.094
		(0.068)*	(0.066)
D·(Quarterly Frequency)	0.254	0.26	0.26
	(0.173)	(0.208)	(0.208)
Dr·(Quarterly Frequency)		-.007	-.007
		(0.121)	(0.121)
$\mathrm{D}\cdot\widetilde{Y}^b$	-.596	-.560	-.392
	(0.138)***	(0.173)***	(0.191)**
$\mathrm{Dr}^2 \cdot \widetilde{Y}^b$		-.040	-.028
		(0.11)	(0.095)
$\mathrm{D}\cdot HDD \cdot \widetilde{Y}^b$			-.008
			(0.01)
$\mathrm{D}\cdot CDD \cdot \widetilde{Y}^b$			-.246
			(0.168)
$\mathrm{D}\cdot HDD$			-.005
			(0.005)
$\mathrm{D}\cdot CDD$			0.036
			(0.077)
$\mathrm{HDD}\cdot\widetilde{Y}^b$			0.021
			(0.03)
$\mathrm{CDD}\cdot\widetilde{Y}^b$			0.209
			(0.205)
HDD			-.005
			(0.017)
CDD			-.113
			(0.067)*
Obs.	1084738	1084738	1084738

Notes: This table presents the estimates of Equation (7), allowing α and δ^{LR} to differ for monthly vs. quarterly groups and as a function of $\widetilde{Y}^b$, which is baseline usage normalized to mean 0, standard deviation 1. The variable r^2 is analogous to r, but it is defined as the time in years since reports were discontinued at the end of the intervention's second year. The sample is limited to the third and fourth years after the intervention begins, including only the control group and the dropped group. The outcome variable is monthly average electricity use, in kilowatt-hours per day. Standard errors are robust, clustered by household. *, **, ***: Statistically significant with 90, 95, and 99 percent confidence, respectively.