Does Absolution Promote Sin? A Conservationist's Dilemma*

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Abstract

This paper shows that households signing up for a green program exhibit an intriguing behavioral rebound effect: a promise to fully offset customers' carbon emissions resulting from electricity usage increases their energy use post-adoption by 1 to 3 percent. The response is robust across empirical specifications, and is consistent with an economic model of rational energy consumption. Our results provide a cautionary tale for designing green product strategies in which the adoption of a product may lead to unexpected consequences.

Keywords: carbon offsets, behavioral rebound, green marketing, energy consumption.

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1 Introduction

To our knowledge, this is the first study to systematically analyze the impact of the introduction of a voluntary carbon offset program on household consumption behavior. We document an intriguing behavioral response to an offset program by evaluating the implementation of a large scale, utility-run carbon offset program in the field. The premise behind the program is that for a relatively small charge a household can entirely offset its carbon footprint from energy consumption by purchasing an "offset". The amount collected by the utility is invested in third party environmental projects such as planting trees or building methane capture plants. The combined effect on enrollees is to decouple their energy use from its carbon emissions, while slightly increasing their energy price.

"Green" products, such as the present offset program, have the potential to provide substantial social benefits by deploying scarce abatement resource towards optimal use. This study focuses on the demand for carbon offsets and its impact on the related demand for electricity. The supply side of the carbon offset market has additional implications on other important industries, including forestry and agricultural. González-Ramírez, Kling, and Valcu (2012) review in some detail the implications of carbon offsets on agricultural policy and highlight the design features of these programs from an agricultural perspective. According to some estimates, offset markets increase farm income by 5 percent (Baker 2010). Quantifying the impact on agricultural markets depends however on a number of factor such as the price of the offset and the productivity of the carbon sequestration technology when applied in agriculture (Graff-Zivin and Lipper 2008). Moreover, farmers shifting supply towards offsets could also have a price impact on US commodities (Brown, Elobeid, Dumortier, and Hayes 2010).

Why consumers exhibit a willingness to pay for carbon offsets is not completely understood, and some drivers of adoption may lead to unforeseen consequences. In such cases, the social benefit may be eroded.[1] We will show that adoption in the program we study is associated with an increase

[1] An extensive literature exists on the so-called "rebound effect", which broadly addresses the poten-

in household energy use post-adoption. If one interprets this effect as causal, which these authors consider to be the simplest explanation, it has implications for the effectiveness of voluntary carbon offset programs. We present a simple economic model which rationalizes this causal interpretation. Conservation is shown to be a substitute to carbon offsets, and the measured effects on energy usage are economically and statistically significant at 1-3 percent of monthly usage during months after enrollment. The behavioral rebound that we observe is robust to a wide array of alternate empirical specifications.

The design of the offset program ensures that the behavioral rebound will not lead to increased GHG emissions *as long as the carbon offset market functions properly.* However, there are reasons to believe that carbon offset markets are exposed to a high degree of information asymmetry and monitoring costs, and thus are likely to perform imperfectly. For example, it is nearly impossible to verify whether abatement investments that underpin offsets are "additional" even if abatement itself can be verified. This is due to the near impossibility of knowing the correct counterfactual. There is an observational equivalence between abatement activity that is additional and activity that is not, and only the agent implementing the investment knows (or may know) the circumstances under which the investment would or would not have occurred. Furthermore, the carbon offset market is exposed to the potential for fraud. There are numerous news stories about fraud in the carbon offset market, especially when the projects are planned in developing countries and not well supervised.[2]

tial general equilibrium effects of energy efficiency and other conservation policies (Borenstein (2015) and Gillingham, , Rapson, and Wagner (2016)). Additionally, recent psychological literature has devoted a substantial effort to document the presence of moral licensing through a variety of laboratory and small scale field experiments (Effron and Monin (2010), Kouchaki (2011), Merritt, Effron, and Monin (2010)). Since consumer choices, especially in the environmental arena reflect social and moral values, this appears to be one promising mechanism that could explain the psychology behind the adoption of carbon offsets and subsequent change in energy consumption (Jacobsen (2010) and Kotchen (2009)). Green markets also affect social welfare, underlying the importance of understanding consumer choice in this setting (Kotchen (2006)).

[2]A particularly poignant story of fraud in the market for "environmental indulgences" was reported by the Christian Science Monitor in April 2010, which revealed how the Vatican was convinced to purchase carbon offsets that would have lead to the Vatican becoming the first carbon free state, but which were never implemented. The purchased offsets were meant to be used for the planting of millions of trees in Hungary. As it turned out the trees were never planted and the Hungarian company abruptly closed down

Under a poorly-performing carbon market, a behavioral rebound along the lines of what we document has the potential to undermine the primary objective of the offset program. Furthermore, the relevance of this behavioral effect is likely to be even more general. Consumers of other products that are, or are perceived as being, environmentally-friendly (e.g. rooftop solar, green electricity, energy efficiency, electric vehicles, etc) may exhibit similar behavior, as has been documented by Kotchen and Moore (2007) and Herberich, List, and Price (2011). As such, this paper contributes a new data point to the ongoing discussion of how second-best environmental policies create incentives that undermine environmental objectives.

The interplay between environmental programs, economic incentives and behavioral factors has been explored and documented across disciplines. While economists have focused mostly on the evaluation of real world examples such as utility programs, the broader psychology and sociology literature focused mostly on smaller experiments (Clayton, Devine-Wright, Stern, Whitmarsh, Carrico, Steg, Swim, and Bonnes 2015)). Ebeling and Lotz (2015) show that in a field experiment and in an Amazon Turk experiment that households defaulted into a green power program had a ten times higher adoption rate. Pichert and Katsikopoulos (2008) also find in a laboratory experiment that most consumers would not switch when defaulted into a green power program. Similarly, in an experimental intervention, Litvine and Wüstenhagen (2011) show that nudges can overcome consumer reticence to invest in green power programs due to uncertainty about the final impact of their purchase.[3]

A common theme in the psychological literature is that monetary incentives may be less effective than psychological nudges when consumers are asked to make environmental choices, particularly

at the end of 2007. See http://www.csmonitor.com/Environment/2010/0420/Carbon-offsets-How-a-Vatican-forest-failed-to-reduce-global-warming

[3]While the experimental literature seems to find that behavioral factors substantially increase the adoption of green power programs, we should caution that the effect may not be universally effective. In an unpublished experiment, one of the authors worked with a major utility and sent 50,000 letters encouraging utility customers to sign up for a green power program using several well-documented behavioral nudges such as social pressure. The experiment did not generate a single adoption but did lead to three complaint letters being sent to the utility and the experiment was not published. This might indicate that publication bias also plays a role in the claimed successes of nudges.

when the price of environmental amenities is low. Bolderdijk, Steg, Geller, Lehman, and Postmes (2013) find that in some cases consumers prefer to be seen as "green" rather than "greedy" in a series of experiments. Behavioral factors have also been shown to be important when consumers choose "green power". The importance of behavioral considerations has now been documented both in experiments (laboratory and field experiments) and in observational studies. When consumers choose to invest in green power programs they ensure that a fraction of their electricity is provided from renewable sources such as solar power. This is a scenario similar to that of carbons offsets since purchasing green power is simply a mechanism to subsidize investment in alternative energy resources. The consumer trusts the utility to use the funds collected today through the green power premium to build new sustainable power plants which will provide clean energy at some point in the future. Kotchen and Moore (2007) provide evidence that environmental concern and altruistic attitudes are correlated with green program enrollment. Jacobsen, Kotchen, and Vandenbergh (2012) show that households enrolling in a utility-sponsored green electricity program exhibit a measurable behavioral change from purchasing just a small amount of green electricity (but not from purchasing more), which provides evidence that non-traditional factors may be at work. Consumers also choose goods with environmental benefits such as energy efficient durables. These purchase decisions have also been shown to be influenced by behavioral factors. Herberich, List, and Price (2011) find that in the purchase of compact fluorescent lightbulbs (CFLs), social pressure appears to operate on the extensive margin (whether or not to purchase a CFL) but not on the intensive margin (how many CFLs to buy).

The design features of the present program both create the possibility of a particularly strong behavioral effect while at the same time ensuring that the environmental outcome is in no danger of being reversed by a behavioral rebound as long as the offset market is functioning. The positive energy rebound may be induced in this setting due to the very low price associated with enrolling in the offset program. This need not generally be the case. There surely exists an offset price that is high enough to induce a financial response that outweighs any behavioral incentives. In the present context, the price of offsets was set equal to the marginal cost of procuring those offsets. On one hand, the price may well be near what can be expected from a profit-neutral voluntary offset program. On the other hand, in practice we may see carbon offsets being part of a portfolio of green power programs which also include investments in renewable energy. In that case the bundle may be offered to consumers at higher prices, and the financial incentive to reduce energy use could more than off-set any behavioral rebound effects.

Either way, while there is almost certainly an improvement in environmental outcomes in our setting, one can envision circumstances under which poorly functioning markets would cause such a rebound to result in inefficiencies. We readily admit that our setting does not allow us to fully eliminate a reverse-causation interpretation of the results (that people enroll in the carbon offset program due to an anticipated near-term increase in their electricity usage). Nonetheless, the possibility that a behavioral rebound is causing the increase in energy demand should be of concern to policymakers and researchers alike, if only to consider measures that will help to achieve the desired outcomes. Thus we view the simple goal of documenting this behavioral feature of the setting as a contribution to the literature and an area that would benefit from further study.

The paper proceeds as follows. Section 2 presents a simple, stylized model which captures the basic economic intuitions. Section 3 describes the program before discussing the data and our sampling framework. Section 4 estimates the short- and long-run impacts of the program, including a dynamic event study of energy use before and after adoption. We also provide a series of econometric robustness checks. Section 5 concludes. Supplementary background material on the voluntary carbon offset market is provided in the Appendix.

2 Conceptual Framework

2.1 An Economic Model of Energy Consumption

A very simple economic model of energy consumption allows us to capture the main stylized predictions about individual behavior when the choice of a carbon offset is offered. Suppose a household i consumes electricity x_i and other goods y_i. Take y_i to be the numeraire, and assume for simplicity that utility is quasi-linear. Further assume that aggregate consumption of electricity is associated with a utility penalty. Concretely, let utility be given by

$$u(x_i) + y_i - \delta_i c(x_i, \sum_{-i} x_{-i}) \tag{1}$$

where $u' > 0$, $u'' < 0$, $\frac{\partial c}{\partial x_i} > 0$, $\frac{\partial^2 c}{\partial x_i^2} > 0$, and $\delta_i > 0$ is a parameter determining the perceived externality from the aggregate consumption of electricity. We define $\bar{X} = \sum_{-i} x_{-i}$ in subsequent notation, which reflects the lack of strategic interaction between actors.[4]

In our model, consumption of electricity imposes a social cost in the form of GHG emissions via c(.). While pollution and global warming are global problems, the extent to which an individual is aware of them or incorporates them into her utility function can vary from individual to individual. This is captured by the parameter δ_i which is heterogeneous across the population.[5] The presence of $\bar{X}$ in $c()$ allows a flexible interpretation of i's perception of cost that includes (but differentiates between) her own contribution to social cost and the externalities associated with the behavior of others. Either way, we assume that individuals are heterogeneous with respect to the δ parameter

[4]The stock of greenhouse gases is large enough that any individual's contribution is infinitesimal. We nonetheless include it in order to keep the model applicable to a broad class of closely-related environmental considerations (e.g. effects on local criteria pollutants).

[5]We interpret the parameter δ as a behavioral parameter but remain agnostic about the precise behavioral/psychological mechanism influencing it. It is possible that δ reflects the degree of understanding or awareness of the social cost of pollution which is a function of education, social, religious and political beliefs. Similarly, since a large share of the cost is likely to be incurred in the future, variation in the δ parameter may reflect individual inter-temporal discount rates and concern for future generations.

and that the extent to which they internalize the social cost is relatively fluid. δ can also be influenced by advertising and informational campaigns.

For simplicity also assume that $u'(0) = \infty$ so the household will always optimally set $x_i > 0$. The household's problem is to maximize utility subject to the budget constraint $px_i + y_i \leq m_i$, where p denotes the price of electricity.[6] Let the value function be denoted by $V_1(p, m, \delta_i)$.

Now consider the introduction of an optional carbon-offset program that eliminates the negative effect of x_i, at an incremental per-unit price of π. If the household decides to adopt the program, the modified problem will be to maximize

$$u(x_i) + y_i - \delta_i c(0, \bar{X}) \tag{2}$$

subject to $(p + \pi)x_i + y_i \leq m_i$. Conditional on adoption, a consumer contributes πx_i dollars to the carbon offset program. Assuming the program is implemented correctly, the utility invests this contribution in environmental programs designed to capture an equal amount of emissions leaving the household emissions neutral (at least as far as the footprint resulting from residential electricity consumption is concerned). We assume that π is chosen so as to exactly balance a household's cost of emissions with the costs of offsetting the same emissions. Thus, the carbon offset program reduces aggregate emissions by the amount previously contributed by the household, x_i. Note also that the extent of the reduction in this utility penalty resulting from enrollment in the offset program depends on the gradient of $c(x_i, \bar{X})$ with respect to x_i, which we denote c_1. Let the value function to this problem be denoted by $V_2(p, \pi, m, \delta_i)$.[7] We now present a series of simple predictions resulting from this stylized model.

[6]For the purpose of our stylized model we ignore non-linear electricity pricing, which is common in the residential electricity market, but does not affect the results of interest.

[7]Note that as a consequence of our stylized model we are ignoring income effects. Basic economic intuitions tell us that, all else equal, higher income households are more likely to sign-up for the program. As we shall see later in the paper this is indeed the case. It is easy to derive the income effect in the current framework under a suitable reformulation of the utility function. This however does not change the other implications of the model and we abstract from the income effect in favor of notational simplicity.

Proposition 1:

1. As δ_i increases, the household is more likely to adopt.

2. As π increases, the household is less likely to adopt.

The first result follows from the Envelope Theorem, $\partial V_1/\partial\delta_i = -c(x_i, \bar{X})$ and $\partial V_2/\partial\delta_i = -c(0, \bar{X})$. Furthermore since $x_i > 0$, $c_1 > 0$ and $c_2 > 0$, it follows that $\partial(V_1 - V2)/\partial\delta_i < 0$. Hence adoption becomes more attractive in δ_i. The second result is immediate because V_1 is unaffected by π, while the Envelope Theorem implies that $\partial V_2/\partial\pi = -x_i < 0$.

This proposition implies that we would expect households who incur a higher disutility from the social cost of aggregate emissions to be more likely to sign up for the program. In the empirical sections we will attempt to quantify this by various indicators of the extent to which individuals are identified as "environmentalists" or engage in activities such as camping and other outdoor recreational activities. Empirically identifying drivers of an individual's perception of the social cost of emissions has important implications for the design of marketing strategies for green goods (Jacobsen 2011). This proposition also shows that as long as $\delta_i > 0$ even individuals with very low disutility from the social cost of carbon emissions may join the program if the price is sufficiently low. In principle, we would expect a broad cross-section of the population to join a carbon offsetting program as long as some awareness of the social cost is present. In reality however, it may be hard for any program to generate sufficient interest even when potential adopters are aware of the social cost, given the countless other demands on individual time and attention whereby programs such as these may have very low salience.

Proposition 2:

1. For sufficiently small π, if a household decides to adopt, consumption of electricity increases post-adoption.

2. If two households decide to adopt, the household with large δ will experience a larger increase in post-adoption consumption.

The first part of this proposition follows since the first-order condition to the household's problem without adoption is $u'(x_i) - \delta_i c_1 = p$ whereas the first-order condition under adoption is $u'(x_i) = p + \pi$. The solutions coincide if $\pi = \delta_i c_1$. Any smaller π will lead to an increase in consumption after adoption as a consequence of $u'' < 0$.

For the second part of this proposition, note that post-adoption consumption is independent of δ so it is sufficient to show that pre-adoption consumption is smaller for larger δ. This follows immediately from Topkis' Theorem applied to the pre-adoption objective function, observing that $\partial^2 U/\partial x_i \partial \delta_i = -c_1 < 0$.

This proposition implies that, conditional on adoption, a household signing up for a carbon offset program may, in fact, increase energy use. While this may seem counter-intuitive, it follows from the fact that adoption depends on awareness of the social cost of carbon emissions. Any program claiming to mitigate these emissions will encourage households to consume more. The social cost of emissions acts as a dampening mechanism on individual consumption. When this mechanism is removed we would expect consumption to increase to the level that would have been chosen if the disutility from the social cost of emissions were set to zero. Naturally, we also have to consider the small impact of the price increase resulting from the introduction of the offsets on overall demand.

The net benefits of a perfectly functioning carbon offsetting program will still be positive. Because offset contributions are proportional to use, the net environmental cost of a household's energy will be zero regardless of the level of post-adoption consumption. However, if we believe the growing body of anecdotal evidence which suggests that the offset market is at times dysfunctional, the net effect can in fact be negative. Proposition 2 also states that when comparing post adoption changes in usage for households with different utility costs of carbon emissions, households with higher pre-adoption costs should increase consumption more post-adoption than households which place less value on the social cost of emissions.

2.2 Behavioral and Psychological Explanations

This very simple economic model captures the economic intuition behind the main result of this paper: households may actually increase their energy consumption (at least temporarily) when adopting a carbon offset program. There are several different models from behavioral science and psychology which are consistent with the empirical evidence provided in this paper.

Altruism and Warm Glow Preferences. At first glance, the provision of carbon offsets shares many similarities with charitable giving (DellaVigna, List, and Malmendier (2012)), which could be employed as a theoretical framework for offsets. The one important difference to charitable giving, however, is the fact that consumption and offsets are linked and may potentially be subject to feedback effects from one to the other (see Kotchen (2009)). Given the existence of a trade-off between the consumption of the private good and the public good, the individual may wish to offset her negative impact on the public good by contributing directly to the public good. Kotchen (2009) shows that this model admits a Nash equilibrium where the provision of offsets is positive above a certain wealth threshold. It does however have the property that if the private good is "more green" then the overall environmental impact can be negative.

Guilt. The availability of offsets may allow individuals to alleviate their guilt while continuing or even increasing their consumption of the socially undesirable good. In a different context Gneezy and Rustichini (2000) find that when asked to pay a fine for delivering their children late for day care, parents actually arrived even later. The opportunity to buy an indulgence at a relatively affordable price may actually decrease the level of individual virtue. Similar results have been documented in responses to enrollment in green electricity programs (e.g. Kotchen (2006), Kotchen and Moore (2007) and Jacobsen, Kotchen, and Vandenbergh (2012)).

Moral Licensing. Consumers often appear to justify actions which may not conform to their self-image (Ayal and Gino 2011). Moral licensing–engaging in self-licensing based on past ethical actions–can be thought of as a mechanism to solve the ethical dissonance experienced by the consumer. According to one definition, moral licensing "occurs when past moral behavior makes people more likely to do potentially immoral things without worrying about feeling or appearing immoral" ((Monin and Miller 2001)). Recently the psychological literature has devoted a substantial amount of effort to document the presence of moral licensing through a variety of laboratory and small scale field experiments (Effron and Monin 2010, Kouchaki 2011, Merritt, Effron, and Monin 2010). Since consumer choices, especially in the environmental arena, reflect social and moral values, this appears to be a promising mechanism that could explain the psychology behind the adoption of carbon offsets and subsequent change in energy consumption (e.g. (Jacobsen 2010)).

3 Offset Program Description and Data

3.1 PG&E's ClimateSmart Program

In June 2007, Pacific Gas and Electric (PG&E), a large California-based utility, launched the ClimateSmart program (hereafter CS program) that offered its customers a means to offset the greenhouse gas (GHG) emissions associated with their usage of electricity. Customers choosing to opt into this program pay an extra $0.00254 per kilowatt-hour (kWh).[8] This price was set by the California Public Utilities Commission (CPUC) and the per kWh charges correspond to an implied price of $9.71 per short ton of CO_2-equivalent emissions.[9] The price remained constant throughout the program implementation. The program was initially launched as a demonstration program which expired on December 31, 2009. It was dormant during 2010 and was formally closed at the end of 2011.

[8] The baseline price of electricity is determined by usage with five tiers ranging between $0.12233 to $0.34180 per kWh.

[9] The price is well-within the range of per ton prices usually encountered for carbon offsets in the offset markets, but substantially lower than common estimates of the social cost of carbon which typically is measured in the $30-50 per ton range, depending on the chosen discount factor.

By the end 2009, PG&E customers had contributed approximately $4.9 million through CS program charges, $2.2 million of which were charged to residential customers which are the focus of this analysis. The remaining contributions come from commercial, agricultural or government entities as well as direct contributions made by PG&E shareholders. These funds were used to invest in GHG emissions abatement projects, offsetting an amount of GHG equal to those associated with the energy use generating the CS program revenue. Marketing activities were subsidized via a small surcharge to PG&E's broad customer base, which is substantially larger than the number of CS program adopters.

As the CS program progressed, PG&E solicited offers for emissions reduction projects. Each of these was subsequently verified by a reputable third-party certification organization called the Climate Action Reserve. As a result of its certification efforts, PG&E offsets associated with the ClimateSmart program are generally viewed as being very high quality.[10] Examples of projects include the reduction of tree harvesting in several Northern California forests and methane capture from a major diary farm and from several landfill projects. The quantity is equivalent to saving 137 million gallons of gasoline, or taking around 225 thousand cars off the road in California for a year.[11]

From the customer acquisition perspective the CS program has been a qualified success. Through 2010, the program had enrolled approximately 30,000 commercial and residential customers. However, when compared to the 6 million potential customers in the PG&E service territory, the adoption rate was low. Geographically, the top five cities for CS program sign-ups were San Francisco (11.0 percent), San Jose (6.3 percent), Oakland (5.7 percent), Berkeley (2.9 percent) and Sacramento (2.2 percent). Retention in the program was strong with less than 0.2 percent active de-enrollments per month.[12]

[10] Climate Action Reserve describes their protocol in detail in a program manual that is publicly available: http://www.climateactionreserve.org/how/program/program-manual/

[11] Source: International Carbon Bank and Exchange, California Energy Commission

[12] Further details on the CS program are available through the detailed annual reports is-

Throughout the program duration, PG&E customers were able to enroll through the company's website. The precise description of the program was easily available to the customers online. Figure 1 shows an example of online marketing which highlights the nature of the carbon offsetting program. The marketing information also highlights the fact that erollees should expect a 100 percent offset as a result of joining the program.

The timing of adoption of the CS product varied in a significant and predictable fashion as function of a variety marketing activities. For the purpose of our analysis, we focus on households enrolling after August 2008, after which point marketing activities were very limited, while the overall awareness of the existence of the program was high. During this period there was no directed outreach from PG&E to individual households, but households were exposed to a television and on-line media campaign centered on the message that a typical California home emits the same amount of GHG over the course of a year as an SUV. It is important to note that the marketing did not target a specific geography during the late period of the program.

We focus exclusively on this period since it provides a setting virtually free from the confounding effects of marketing framing. Enrollment required households to invest effort to identify the program and then enroll in it. The impetus for such effort is idiosyncratic, so enrollment times during this period were fairly random. In the event study analysis which follows, the distribution of adoption timing allows us to flexibly control for the presence of time-varying factors that may otherwise generate confounding concerns. The necessity to seek out the program also selects on households who are, as revealed by their behavior, actively engaged on the margins of environmental concern and energy use.

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sued by PG&E. The figures quoted are from the report for 2010 which is publicly available at: http://www.pge.com/myhome/environment/whatyoucando/climatesmart/programdetails/

3.2 Data

The sample used in this paper consists of residential CS program adopters purchasing electricity from PG&E. We restrict attention to customers on the E1 electric rate, which is the most common residential rate.[13] Our sample consists of 748 customers who enrolled in the CS program between August 2008 and November 2009 (the last month for which new enrollments were processed under the original program) and 13,449 control households, which are also on the E1 rate schedule but never enrolled in the CS program. The sample of control households was drawn at random from the full PG&E database and stratified to reflect the different areas in which PG&E operates and thereby accurately reflect the distribution of customers. This sample was obtained after removing various outliers as explained below.

For each CS program customer we know the date of enrollment. Unfortunately, we do not know if or when a customer de-enrolled because it does not affect their presence in the billing dataset unless they also disconnect their electricity service. The total de-enrollment rate averaged 1-2 percent per month, and happened automatically as a customer moved. To avoid bias from unobserved de-enrollments, we limit our sample to those customers for which we have near-complete billing data between June 2006 and December 2010. We thus eliminate recent movers, who could arguably have different consumption patterns especially in the months before and after a move. All households in our sample have at least 50 (out of 55 possible) months of billing data.[14]

While average household electricity usage in PG&E territory ranges from 600 to 700 kWh per

[13] This rate is not time-of-use dependent. Throughout this analysis we do not consider low-income households. These households are on special variants of this electric rate, and PG&E took extra steps to ensure that they were aware of being in the program. The program was available to all customers on an opt-in basis but low-income customers on special rates were subsequently de-enrolled by the utility.

[14] It is quite common for anomolies to appear in any electricity billing dataset. For example, billing errors may occur that create unrealistic patterns (say, zero dollars in June but twice the seasonally-adjusted expectation for July). We remove these months from the data, but see no justification for dropping the entire household time-series due to such brief idiosyncratic billing events. As such, we allow for a small number of absent months from our pseudo-balanced panel. In any case, results are robust to changes in these minor sampling assumptions.

month, the distribution of electricity consumption in PG&E territory is extremely skewed to the right, with the top 1 percent of users consuming in excess of 4781 kWh per month. We truncate the sample to remove the top 1 percent of users. Furthermore, it is common to see negative billing amounts due to various adjustments. Since it is difficult to interpret these observations, and as they may potentially influence our results, we have also eliminated households with any monthly usage of less than 1 kWh or a billed amount of less than $5. The results that we report subsequently are robust to re-inclusion of these households.

One important limitation of our electricity usage data is that we don't observe the actual billing cycle for each household. In practice billing periods do not overlap perfectly with calendar months. Thus, a usage amount for say November 2007 may include up to two weeks of December 2007. The billing cycle differs from household to household and induces measurement error in the dependent variable at the monthly level.

For each household in our sample we observe its ZIP code of residence (772 unique zip codes) as well as a number of demographic and lifestyle characteristics obtained from a third-party data provider, the marketing services firm, Acxiom.[15] In Table 1 we present the summary statistics for our sample. For each variable we report the mean and standard error for both the group of customers who sign up for the CS program and the control group of non-adopting customers. We also report the p-value of the t-test for the difference in means between the two groups for each variable. It is immediately obvious that the two groups of customers are different from a statistical point of view, though perhaps by not as much as we would have guessed initially.

The mean electricity usage for adopters is smaller than that for the non-adopters (598kWh versus 635kWh). Similarly adopters paid an average monthly bill of $96 while non-adopters paid average monthly bills of $110. The lower than average energy consumption for adopters explains the overall

[15] Data on individual households was purchased by the utility as part of the regular business processes. Given the availability of complete address information in their administrative database we do not think selection to be a major factor.

very low additional cost of signing up to the CS program for the adopting households. In our sample the mean monthly contribution to the CS program was $1.35 (with a standard deviation of $0.79). The maximum observed monthly contribution in our sample was only $8.94.

Since we have eliminated movers from our sample and since younger people are more likely to be mobile, the average household head age in our sample is 51 years for adopters and 56 years for non-adopters. At the same time adopters tend to be wealthier and less likely to have children. In terms of housing characteristics, adopters tend to live in smaller but more expensive homes. They are also less likely to have a swimming pool. California is divided into climate zones which reflect the specific climate in the area. Cooler, coastal climate zones are labeled with lower numbers while hotter more arid zones are labeled with higher numbers.[16] Table 1 shows that adopters are more likely to live in coastal areas where they are substantially less likely to require air conditioning. This also corresponds with the location of the bigger cities. Overall the demographic profile of adopters suggests that adopters are younger, wealthier households who live in more urban, coastal areas (e.g. the San Francisco Bay Area).

Our data also include information on lifestyle choices made by households based on marketing characteristics determined by Acxiom. We report the propensity of households to have expressed interest in environmental or wildlife issues ("*Environmental*"), those making environmentally healthy product purchase decisions or donating funds to environmental causes ("*Green Living*"), as well as two variables relating to charity. "*Charity*" captures whether a household is interested in their community and/or involved in local charitable organizations or activities, and "*Charitable*" is a model based variable designed to predict households with a high propensity to donate to charitable causes.

These variables were acquired at the start of the program and are not affected by the later participation of a household in the CS program. Furthermore, all demographic and lifestyle choice

[16]For example, San Francisco is in zone 3 while zone 12 corresponds to the Northern Central Valley which experiences substantially hotter summers than zone 3.

variables are proprietary to the third party provider, Acxiom, and the precise details of the data construction and predictive models for some of these variables have not been made available to us. These variables are, however, widely used in the private sector and have become a highly valued asset in the marketing activities of firms. Thus, while questions regarding their validity and extent of measurement error remain, we lean towards trusting these variables.[17] Overall, we find that adopters strongly identify with environmental principles as revealed by their interests from subscriptions to their enjoyment of camping and the outdoors. But at the same time they appear to be less green in their actions.

4 Impact of Adoption

In this section we examine how energy use changes in response to adoption of the carbon offset program. We begin by estimating a difference-in-differences model, which reveals the nature of the long-run effect. We then examine short-run effects using first-differences model. Finally, coefficients from an event study provide a flexible representation of the dynamic effects, both before and after adoption. In all specifications, we use the aforementioned pseudo-balanced panel of customer electricity usage data from June 2006 to December 2010. Results of these analyses are unchanged if we use the larger, unbalanced panel.

4.1 Difference-in-Differences

The difference-in-differences approach compares electricity use across households and over time relative to when the CS program was adopted. Specifically, we estimate the following model:

$$ln(k)_{it} = \beta I^{cs}_{it} + \alpha_t + \gamma_i + \epsilon_{it} \quad (3)$$

[17] One of the authors of this paper purchased data on himself from the same provider and the purchased information found the information to be accurate, although it slightly underestimated his environmentalist credentials.

where $ln(k)_{it}$ is the log of monthly electricity consumption (kWh) for household i in month t, I_{it}^{cs} is an indicator variable equal to one if household i is enrolled in the CS program in period t, and zero otherwise, α_t and γ_i are month-by-year and household fixed effects, respectively.[18] The error term, ϵ_{it}, accounts for factors that idiosyncratically perturb electricity usage at the monthly level, and may or may not be observable to the household. We also estimate equation 3 for heterogeneous treatment effects, interacting I_{it}^{cs} with a variety of demographic variables.

It is worth pausing to discuss the interpretation of the results that estimation of this equation will yield, particularly in light of the fact that the CS program is voluntary, and that households must select into it. One possible goal would be to estimate the causal effect of the CS program on electricity usage in the context of random assignment. That is, were we to randomly assign some households to "treatment" (CS) and others to "control", we could estimate the population average treatment effect (PATE) on consumption. The identifying assumption in this case would be a standard orthogonality condition: conditional on time-invariant household characteristics and aggregate period-level effects, $E[I_{it}^{cs}\epsilon_{it}] = 0$. While it may well be the case that this condition holds in our setting (i.e. if adoption of the CS program is orthogonal to electricity use), it's easy to imagine situations under which the condition is not satisfied. For example, a household may feel guilty about an anticipated increase in consumption, and enroll in the offset program as a result.

However, the effect that we seek to estimate is the presence of a behavioral rebound effect on enrollees – the population average treatment effect on treated households (PATT). The voluntary nature of the program is appropriate for the desired hypothesis testing, since the self-selecting nature of the act of enrolling is the very treatment that one might expect to induce the behavioral rebound.[19] Any change in consumption after adoption (or, for that matter, before), may under

[18]We have also used month and year dummies to control for seasonality, which impose a lower computational burden as that specification requires fewer parameters. The results were almost identical. Later we shall discuss more robustness checks to account for seasonality in our specifications.

[19]Further, from a program evaluation perspective, our setting is also appropriate. Under no realistic implementation of this sort of program would the allocation of offsets be random. The same is not true for allocation of permits under a cap-and-trade regime, a very different setting than what we study here.

very reasonable conditions be related to the enrollment decision. Thus, what we are interested in estimating is the effect of selection into the program on usage in surrounding periods (ie. the PATT).

The assumptions under which the equation 3 will retrieve the PATT when estimated using our data are plausible. If households are exposed to a stochastic shock that compels them to enroll in the offset program, and if that shock does not directly alter their electricity use, then $\hat{\beta}$ will be a consistent estimate of the PATT. Many such unobserved shocks are possible – for example, the local newspaper prints a story about the causes and effects of global climate change, compelling them to investigate ways to reduce their carbon footprint.

Estimates of β are displayed in Table 2. Each column provides estimates from a separate regression which explores the degree to which the effect varies with the demographic variables. We find that low-income adopters exhibit an increase in electricity use of 11.6 percent in the long run. No other demographic interaction term produces a change that allows us to reject zero long-run effect with 95 percent confidence.

4.2 First Differences

If persistence of any change in electricity usage due to the CS program adoption is low, then results in Table 2 will understate the near-term response. The treatment effect estimated from a model of first differences will be identified off of changes in behavior during the treatment assignment period, and thus provides an estimate of the effect in the short run (one month). Let $dln(k_{it}) = ln(k_{i,t}) - ln(k_{i,t-1})$. We estimate the following first differences specification:

$$dln(k_{it}) = \beta CS_{it} + \alpha_t + \epsilon_{it}, \tag{4}$$

where CS_{it} is an indicator variable equal to 1 in the month when household i adopts the CS program and 0 otherwise. Again, this is estimated on the pseudo-balanced panel of customer electricity usage data from August 2008 to December 2010. Time-invariant household characteristics are eliminated by differencing, although we estimate treatment effects by demographic category. We also include aggregate time controls at the month-by-year level (not differenced).

Estimates of equation 4 are shown in right half of Table 2. For adopters, carbon offsets appear to be a substitute for conservation. The overall effect in first differences for these households is a 1.8 percent increase, statistically significant at the 95 percent level.[20] This effect is once again driven by young households, who exhibit a 6.0 percent increase in electricity use in the post-adoption month.

4.3 Dynamic Effects: Event Study

Results in Sections 4.1 and 4.2 show an initial increase in consumption of 0-5 percent (depending on the demographic group) and an attenuation of the effect in the long run as an implication of the difference-in-differences results. While it does not appear that the CS program had an economically significant impact in the long run, it may still have substantial transitory effects. In order to exploit the heterogeneous timing of adoption, we conduct an "event study" analysis which allows us to analyze the dynamic effects of program adoption (see [21]). The use of the event study methodology requires us to separately identify the effect of program adoption over time from the seasonality in consumption due to weather patterns.

A strength of the event study approach is its ability to control directly and indirectly for unobserved time-varying aggregate effects. Direct controls come in the form of calendar month dummy variables, but the innovation of the approach is to re-align the time series into proximity to the event of interest (in our case, enrollment in the ClimateSmart Program). Enrollment that occurs in different months for different households reduces the avenues of contamination that are possible

[20] Note that effects in this direction would imply an upward sloping demand curve if price were the driving factor.

[21] Jacobson, Lalonde, and Sullivan (1992)

from time-varying confounders. The approach is most reliable when adoption occurs at random intervals throughout the time series. Indeed, over the period from August 2008 to the end of 2009, households adopt at very different points in their annual seasonal consumption pattern. We confirm that the actual adoption over time is as close as possible to a random uniform pattern by constructing a time-to-adoption variable, τ_i, as the number of months between July 2008 and the month of adoption. We estimate a duration model for τ_i conditional on the observable demographics, choose a simple Weibull specification and regress $ln(\tau_i)$ on the list of demographics, lifestyle variables and home characteristics discussed above.[22]

Table 3 reports the estimated coefficients from several models which differ in their included set of covariates. While a few covariates appear to be statistically significant, they are typically not significant across different specifications. Moreover the extent to which they explain the timing of adoption is doubtful since the model fit is extremely poor, with an adjusted R^2 of between 0 and 0.02 and thus even the statistically significant variables are economically not significant. This provides encouraging support to our assertion that adoption occurred more or less at random and timing of adoption cannot be predicted from the observables.

We can therefore proceed to estimate the following equation:

$$ln(k)_{it} = \sum_{j=\underline{m}}^{\overline{m}} \xi_j D_{it}^j + \alpha_t + \gamma_i + \epsilon_{it} \tag{5}$$

where D_{it}^j are a set of indicator variables set equal to one if, in calendar month t, household i is j months after its CS program adoption month. The event window is defined as $j \in [\underline{m}, \overline{m}]$, and we normalize the coefficient of event month prior to adoption to zero. Additional indicators corresponding to "outside the event window" allow us to fully capture the dynamic effects of treatment. The underlying assumption here is that, conditional on time-invariant household characteristics and aggregate month-specific shocks, all households that are j months away from enrolling in the

[22] The results are not affected by the choice of the Weibull model. More complicated duration models do not change the results.

offset program are identical (in expectation). We take a flexible approach to determining the event window, allowing $\overline{m} = -\underline{m} = 24$ and therefore defining the largest possible event window as comprising 2 years on either side of the adoption event. The specification thus includes an indicator variable for +25 which captures all periods more than 24 months from the adoption of the CS program. Similarly we include an indicator variable for -25, which captures all periods more than 24 months before adoption. Estimating the event study using a large window allows us to "zoom" in or out as desired to most clearly present the graphical results. The above specification for the estimation of the response pattern relative to the time of adoption implicitly models the response as a piecewise linear function of relative time to adoption, with no restrictions on the variation or pattern of the response over time.

We estimate equation 5 and plot the resulting estimates in event time (where 0 denotes the month prior to adoption) in Figure 3, using an 18-month event window. The continuous line denotes the estimates for the event period dummies while the dashed line corresponds to the confidence bounds for the estimates on the indicator variables. The estimates exhibit three features that provide interesting context to the previous results. In the months leading up to enrollment, households are engaged in increasing conservation efforts; consumption decreases by 4 percent over that 6 month period. At the time of adoption, conservation ends and households increase consumption by approximately 3 percent. This "rebound" is persistent, as can be seen by the relatively flat consumption profile in the six months after enrollment. The higher usage of adopters over the control group then levels out and becomes increasingly imprecise as distance from the event grows.

The true short-term effect of enrollment on usage – the difference in usage under enrollment (observed) versus that under non-enrollment (unobserved) – depends on what one believes to be the appropriate counterfactual. One's interpretation may thus be influenced by whether the measured trend before adoption is statistically significant. If the downward trend of enrollee usage continues in the absence of enrollment, this would lead the "true" effect to be larger than the point estimates of the event time dummy coefficients (which are estimated relative to zero). An F test comparing the restricted model where the slope before adoption is constant to the unrestricted model rejects the restricted model with a p-value of 0.15. While not overwhelmingly significant, it does lend support to the interpretation that households are engaged in conservation behavior before adoption, after which time they exhibit a rebound effect. It is unlikely the case that the adoption of the carbon offset leads to a temporary reduction in consumption during the adoption month and that households returns to their pre-adoption level afterwards.

Our difference-in-differences and first-difference specifications suggest that responses may be heterogeneous across demographics. We investigate this further by interacting the event-time indicators with observable demographics. Results from these specifications are presented in Table 4. It is clear from the increased statistical significance that the nonparametric controls for aggregate calendar-month variation that are inherent in the event study approach are effectively soaking up residual variation that was present in the earlier specifications. It is to be expected that the coefficients on event month 1 do not precisely match the first-difference coefficients, since the specifications differ on at least two important dimensions. The event study controls differently for calendar time effects, and normalization of the event time zero coefficient potentially influences each of the event-period coefficient estimates

For the most part the results conform to the pattern observed in the base case, but with varying degrees of pre-adoption conservation and event-month rebound. In the six months preceding adoption, households of various demographic groups conserve electricity by 2-10 percent. Much of the amount conserved is reversed in the months following adoption, as enrollees increase electricity usage relative to non-adopters.

These results once again demonstrate that the strongest increase in consumption at event time is exhibited by young households, who change consumption by 5 percent in that period. This effect persists for several months. Heterogeneity along demographic lines also reveals a treatment-period increase for wealthy households and those in small or medium dwellings. These effects are approximately 3 percent in magnitude, and likely reflect a strong correlation between the demographic variables (age, income and home size) and geographic location (the San Francisco Bay Area, where adoption rates were highest). In some cases the effect dissipates after quite quickly (e.g. in the mid-to-high age and middle-income categories), consistent with the difference-in-differences and first differences specifications. In others it appears to linger, and even grow over time (e.g. older and higher-income households).

4.3.1 Robustness Checks

In order to bolster the claim that our results are not being driven by some unobserved factors, we also perform several placebo tests. Any difference-in-differences type strategy requires that there be no systematic variation in the outcome variable that is spuriously correlated with treatment. In the context of an event study, it is difficult to imagine how such fluctuations would survive the time controls and conversion to event time. Nonetheless, we implement an array of placebo tests on adopting households to reinforce this point. We estimate placebo treatment effects using samples partitioned into the pre-adoption and post-adoptions months. We implement these placebo tests by drawing from 50 simulated samples with randomly assigned placebo treatment status and placebo adoption date. Specifically, for each simulation we randomly assign ten percent of adopting households into placebo treatment status, and separately randomly assign to them a placebo adoption month. We repeat this separately for months before and after the actual adoption decision by the households in the sample. The difference-in-difference and first-differences coefficient estimates and standard errors are averaged and presented in Table 5. Each of the coefficient estimates is economically and statistically indistinguishable from zero. In Figure 4 we present estimates of the event study coefficients that arise from Placebo regressions using this methodology. It is clear that there are no measurable effects of the placebo on monthly electricity consumption. These results provide additional evidence that our main treatment effect estimates are not artifacts of spurious correlations in the data.

In addition, we performed an extensive array of robustness checks. These are too numerous to present in the manuscript, but the alternate specifications produced results that are qualitatively and quantitatively consistent with our main results. These included additional model specifications involving consumption levels as opposed to log consumption, alternative normalizations of individual consumption observations, and several different models involving inverse probability weights.[23] We retrieve similar results when estimating our baseline specifications on alternate samples, such as the re-inclusion of electricity usage outliers and the use of an unbalanced panel that allows for the inclusion of households that may have recently moved. In the Appendix, we also show that our results are robust to other approaches to seasonality corrections, such as multiple time-varying interactive effects and H-P filtering.

5 Discussion and Conclusions

In this paper we document a behavioral effect with potential consequences for the deployment of environmental programs: selecting into a good behavior may lead to increased energy consumption. This area has recently attracted attention in a number of fields under the broad headline of "incongruous actions". These actions may be particularly unfortunate for the valuation of environmental projects, where it is often known as a "rebound effect". While the cost of carbon emissions is in principle offset by the consumers through their participation in the program, there remains uncertainty as to its eventual success due to imperfect abatement and the ongoing doubts over fraud and market imperfections.

Our particular focus in this paper is voluntary carbon offsets. These are provided to individuals if they contribute a surcharge on their energy use in order to mitigate the environmental impact of energy production through investment in carbon abatement projects. We document two main

[23] A specification that included household by month-of-year controls produced results that were essentially noise. However, such an over-specified model does not leave adequate residual variation to credibly identify an effect.

empirical findings. First, many individuals who voluntarily sign up for carbon offsets actually increase their consumption following adoption. Since offsets increase the marginal cost of electricity, the driving force behind this substitutability must be non-monetary. The results are consistent with an intuitive behavioral rebound effect which can be explained by a number of cognitive mechanisms documented by behavioral scientists in related areas. Second, households selecting into the offset program have meaningfully different characteristics than those that do not. The uniquely detailed data available to us at household level allows us to investigate individual heterogeneity and the extent to which it matters for the adoption of environmentally beneficial programs and their subsequent usage.

To our knowledge, this study provides the first evidence of the behavioral rebound effect from a large-scale field deployment of carbon offsets. Thus it confirms the relevance of a growing body of laboratory and field evidence on the unforeseen effects of green programs, which also has implications for program design. To the extent that markets are subject to inefficiency, these sorts of unexpected behavioral responses may decrease the value of the abatement investments.

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Table 1: Summary statistics for ClimateSmart Adopters and Non-adopters

	E1 Rate Customers Adopters (N=748) Non-adopters (N=13,449)				
	Mean		SD		p-value
	Non-adopters	*Adopters*	*Non-adopters*	*Adopters*	
(Average) kWh*	635.507	598.053	360.500	340.831	0.01
(Average) Bill*	109.731	96.712	97.345	81.307	0.00
Age	56.545	51.313	14.306	12.770	0.00
College	0.294	0.295	0.456	0.457	0.93
HHIncome $80k+	0.480	0.539	0.500	0.499	0.00
Children	0.471	0.455	0.499	0.498	0.48
Working Woman	0.467	0.453	0.499	0.498	0.48
HH Size	2.877	2.513	1.471	1.398	0.00
Home Owner	0.966	0.957	0.180	0.204	0.18
Environmental	0.097	0.184	0.296	0.388	0.00
Green Living	0.615	0.575	0.487	0.495	0.03
Charity	0.370	0.400	0.483	0.490	0.10
Charitable	0.547	0.487	0.498	0.500	0.00
Outdoors	0.551	0.623	0.497	0.485	0.00
Wildlife	0.073	0.110	0.261	0.313	0.00
Camping	0.266	0.352	0.442	0.478	0.00
Home Age	38.547	42.937	23.403	27.393	0.00
Heating	0.273	0.298	0.446	0.458	0.13
Cooling	0.111	0.095	0.314	0.293	0.18
Sqft 2500+	0.161	0.146	0.367	0.354	0.35
Home Value $500k+	0.161	0.206	0.368	0.405	0.00
Pool	0.139	0.111	0.346	0.314	0.03
ClimateZone	6.670	5.864	4.459	4.199	0.00

* The average kWh and bill amounts for adopters are computed for the pre-adoption periods

Table 2: Difference-in-Differences and First Difference Estimates

	Difference-in-Differences					First Difference				
Column:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Base case	0.006	-0.001				0.018*	0.018*			
	(0.008)	(0.007)				(0.009)	(0.010)			
Age(Young)			0.024					0.060**		
			(0.016)					(0.024)		
Age(MidL)			-0.009					0.016		
			(0.013)					(0.017)		
Age(MidH)			-0.001					-0.008		
			(0.015)					(0.015)		
Age(Old)			-0.02					-0.001		
			(0.021)					(0.027)		
Sqft(H)				-0.002					0.039	
				(0.017)					(0.031)	
Sqft(M)				0					0.031*	
				(0.012)					(0.017)	
Sqft(L)				0.008					0.013	
				(0.015)					(0.015)	
Income(H)					0.017					-0.009
					(0.023)					(0.029)
Income(M)					0.002					0.017
					(0.026)					(0.034)
Income(L)					0.116**					-0.04
					(0.052)					(0.064)
Household FEs	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Month-by-Year FEs	Y		Y	Y	Y	Y		Y	Y	Y
MoY-by-County FEs		Y					Y			
R-squared	0.093	0.234	0.099	0.093	0.093	0.128	0.203	0.137	0.128	0.128
Observations	414,253	407,862	369,706	414,253	414,253	395,456	389,363	352,972	395,456	395,456
Number of IDs	14,442	14,219	12,887	14,442	14,442	14,442	14,219	12,887	14,442	14,442

*Each entry is a point estimate (with standard error in parentheses) from a regression of ln(kWh) on an adoption indicator interacted with the demographic categorical indicator noted in the row header. * Significant at the 0.10 level, ** Significant at the 0.05 level, *** Significant at the 0.01 level. Standard errors clustered at the HH level*

Table 3: Weibull model for the time to adoption during the period July 2008 to November 2009.

Column:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Age	-0.00120*		-0.00128*	-0.00148**	-0.00178**	-0.00138*	-0.00170**
	(0.001)		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
College	0.00296		0.00139	0.00101	-0.00251	-0.00013	-0.00545
	(0.018)		(0.018)	(0.018)	(0.019)	(0.018)	(0.019)
HHIncome $80k+	0.01199		0.01192	0.01304	0.00448	0.01418	0.00651
	(0.018)		(0.018)	(0.018)	(0.020)	(0.018)	(0.019)
Children	-0.03054		-0.03247	-0.02837	-0.03617	-0.02981	-0.04006*
	(0.020)		(0.021)	(0.021)	(0.022)	(0.022)	(0.023)
Working Woman	0.01238		0.00676	0.00570	0.00075	0.00651	0.00191
	(0.017)		(0.018)	(0.018)	(0.019)	(0.018)	(0.020)
HH Size	0.00247		0.00297	0.00156	0.00388	0.00163	0.00365
	(0.007)		(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Home Owner	-0.00918		-0.01555	-0.01701	-0.19112***	-0.01929	-0.18918***
	(0.049)		(0.050)	(0.052)	(0.055)	(0.052)	(0.055)
Environmental			-0.02187	-0.03080	-0.02953	-0.02442	-0.03084
			(0.021)	(0.022)	(0.023)	(0.026)	(0.027)
Green Living			0.02725	0.01756	0.02232	0.01801	0.02467
			(0.021)	(0.027)	(0.028)	(0.027)	(0.029)
Charity				0.03123	0.04128*	0.03440*	0.04473*
				(0.020)	(0.023)	(0.021)	(0.023)
Charitable				0.00129	-0.00645	0.00343	-0.00935
				(0.025)	(0.026)	(0.026)	(0.028)
Outdoors						-0.04651*	-0.06227**
						(0.024)	(0.025)
Wildlife						-0.02156	-0.00612
						(0.029)	(0.033)
Camping						0.04533**	0.06281***
						(0.020)	(0.022)
Home Age		-0.00033			-0.00009		-0.00016
		(0.000)			(0.000)		(0.000)
Heating		0.00952			-0.00050		0.00179
		(0.017)			(0.019)		(0.019)
Cooling		0.00302			0.00589		0.00630
		(0.026)			(0.030)		(0.029)
Sqft 2500+		-0.00660			-0.02373		-0.01991
		(0.024)			(0.027)		(0.028)
Home Value $500k+		0.03602*			0.05643**		0.05747***
		(0.019)			(0.022)		(0.022)
Pool		0.00228			-0.00012		-0.00875
		(0.023)			(0.025)		(0.026)
log(kWh)							0.00471
							(0.020)
Constant	3.07218***	3.00769***	3.07198***	3.07887***	3.26251***	3.08860***	3.24835***
	(0.061)	(0.018)	(0.063)	(0.064)	(0.070)	(0.065)	(0.151)
Observations	516	569	516	516	447	516	447
Adjusted R-squared	0.00	0.00	0.00	0.00	0.01	0.01	0.02

*Robust standard errors in parentheses. * Significant at the 0.10 level, ** Significant at the 0.05 level, *** Significant at the 0.01 level.*

Table 4: Event study: Heterogeneous dynamics

Event Study: Heterogeneous Dynamic Program Effects (Demographics)

	Age(Young)	Age(MidL)	Age(MidH)	Age(Old)	Sqft(L)	Sqft(M)	Sqft(H)	Income(L)	Income(M)	Income(H)
Months From Event										
-6	0.079***	0.028	0.038	0.101**	0.045	0.051*	-0.02	0.039	0.033	0.077***
	(0.030)	(0.030)	(0.029)	(0.040)	(0.027)	(0.027)	(0.042)	(0.045)	(0.028)	(0.022)
-5	0.097***	0.021	0.02	0.105**	0.04	0.056**	0.002	0.012	0.042	0.074***
	(0.033)	(0.030)	(0.028)	(0.043)	(0.028)	(0.027)	(0.039)	(0.046)	(0.028)	(0.022)
-4	0.047	0.023	-0.004	0.101**	0.013	0.033	-0.009	-0.038	0.044	0.041**
	(0.029)	(0.028)	(0.027)	(0.046)	(0.027)	(0.025)	(0.038)	(0.048)	(0.028)	(0.020)
-3	0.045	0.050**	-0.003	0.083*	0.022	0.029	0.025	0.036	0.037	0.043**
	(0.028)	(0.024)	(0.025)	(0.043)	(0.024)	(0.023)	(0.035)	(0.041)	(0.026)	(0.018)
-2	0.036	0.009	-0.011	0.04	0.008	0.023	0.021	0.015	0.026	0.007
	(0.022)	(0.021)	(0.020)	(0.035)	(0.019)	(0.020)	(0.032)	(0.035)	(0.021)	(0.014)
-1	-0.002	0.029*	0.018	0.042	0.009	0.019	0.057*	0.031	0.023	0.016
	(0.016)	(0.016)	(0.018)	(0.032)	(0.016)	(0.014)	(0.032)	(0.027)	(0.016)	(0.013)
0	0	0	0	0	0	0	0	0	0	0
1	0.051**	0.018	0.017	0.041	0.032**	0.033**	0.041	0.016	0.033**	0.030**
	(0.022)	(0.015)	(0.016)	(0.031)	(0.015)	(0.016)	(0.029)	(0.024)	(0.017)	(0.013)
2	0.055***	0	0.043**	0.057*	0.021	0.031*	0.028	0.013	0.03	0.039**
	(0.021)	(0.021)	(0.018)	(0.034)	(0.019)	(0.018)	(0.031)	(0.028)	(0.020)	(0.015)
3	0.081***	0.014	-0.001	0.065*	0.022	0.028	0.036	-0.02	0.031	0.050***
	(0.028)	(0.025)	(0.024)	(0.039)	(0.024)	(0.022)	(0.037)	(0.035)	(0.024)	(0.019)
4	0.059*	-0.001	-0.02	0.051	-0.005	0.011	-0.007	-0.026	0.004	0.037*
	(0.032)	(0.028)	(0.025)	(0.037)	(0.026)	(0.024)	(0.038)	(0.041)	(0.026)	(0.022)
5	0.072**	-0.002	0.004	0.095**	0.023	0.024	-0.011	0.017	0	0.060***
	(0.031)	(0.027)	(0.025)	(0.041)	(0.025)	(0.024)	(0.039)	(0.039)	(0.026)	(0.021)
6	0.076**	-0.002	0.008	0.098**	0.037	0.03	-0.032	0.02	-0.008	0.070***
	(0.031)	(0.029)	(0.027)	(0.039)	(0.027)	(0.024)	(0.040)	(0.043)	(0.027)	(0.021)

** Significant at the 0.10 level, ** Significant at the 0.05 level, *** Significant at the 0.01 level. Standard errors clustered at the HH level. All specifications include household and month-by-year controls.*

Table 5: Placebo Tests on Adopting Households

Sample:	Adopter Households, Pre-Adoption Periods		Adopter Households, Post-Adoption Periods	
Specification:	DinD	FD	DinD	FD
Placebo Treatment	0.000	0.001	0.003	0.006
	(0.007)	(0.008)	(0.023)	(0.025)
HH FEs	Y	Y	Y	Y
Month-by-year FEs	Y	Y	Y	Y
R-Squared	0.135	0.126	0.106	0.114
Observations	242,473	231,006	16,493	15,623

*Placebo effect estimates calculated from the mean of NS=50 simulation draws. Means of the standard errors (clustered at the household level) across simulation draws are reported in parentheses. * Significant at the 0.10 level, ** Significant at the 0.05 level, *** Significant at the 0.01 level.*

Figure 1: Example of marketing material explaining the nature of the carbon-offset program.

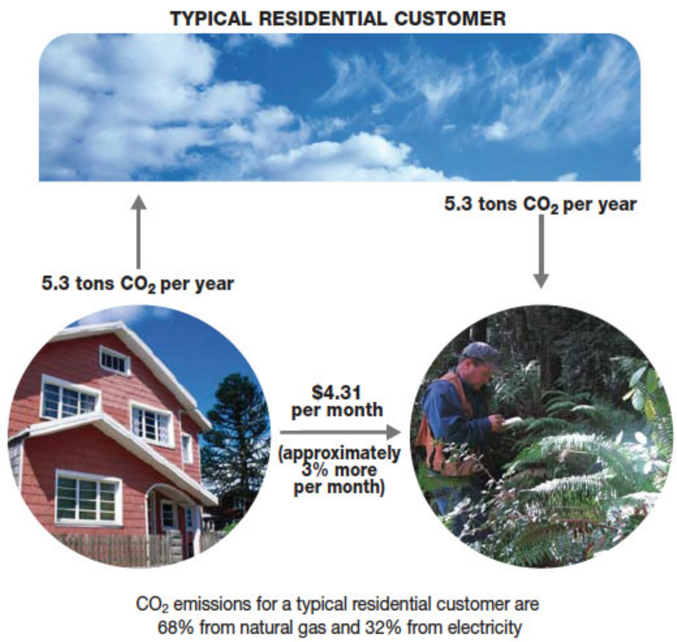


Figure 2: Histogram of Adoption Timing

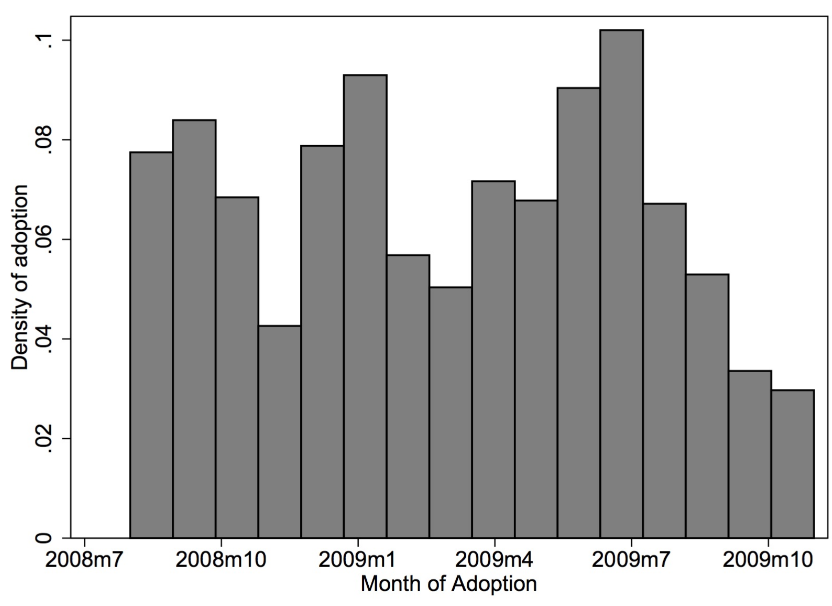


Figure 3: Estimation of the dynamic effect of adoption in event time

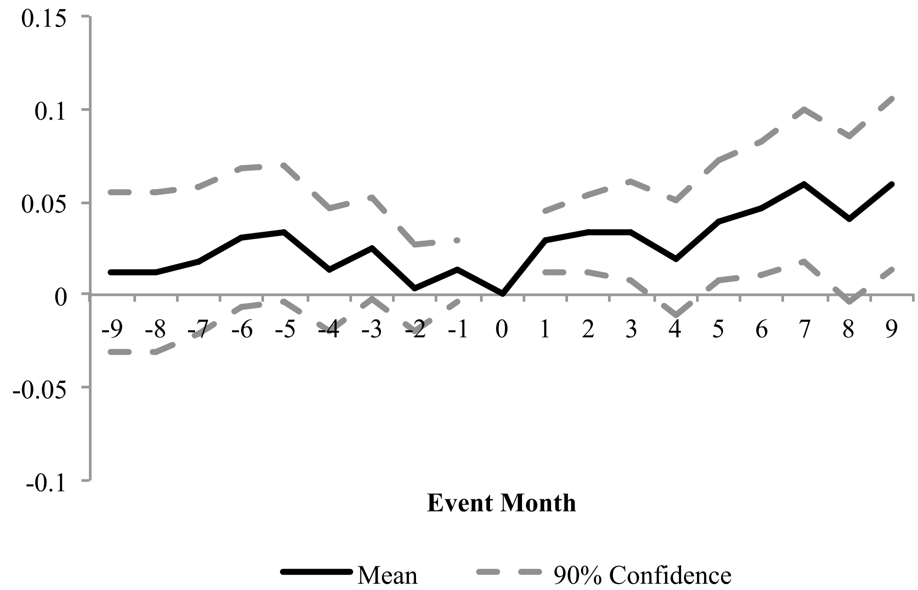
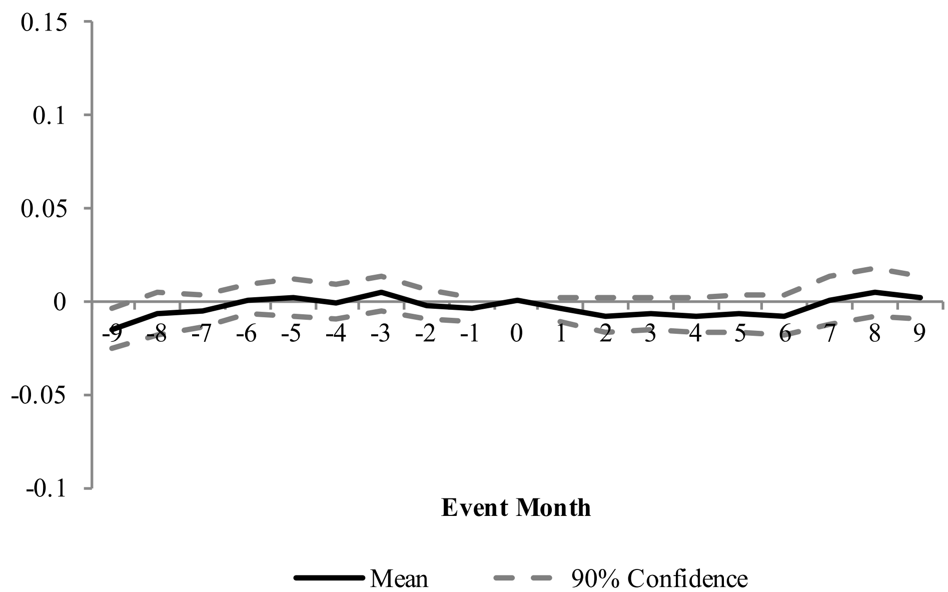


Figure 4: Placebo Event Study (100 Placebo simulation draws)



A Appendix

A.1 Background on the Voluntary Carbon Offset Market

By definition one carbon offset corresponds to the removal or neutralization of one metric ton of CO_2 or an equivalent amount of other gases such as methane, nitrous oxide, hydrofluorocarbons, perfluorocarbons or sulphur hexafluoride, all of which contribute to the greenhouse effect.

The basic intuition behind this market is that since carbon emissions contribute to a global stock, abatement in one part of the world is equivalent to abatement elsewhere. If marginal abatement costs differ across regions, it should be possible to offset one's emissions in an indirect, cost effective way. Each carbon offset is generated as a result of a specific environmental project, most of which can be located at a considerable distance from the buyer of the carbon offset. The range of environmental projects which can offset carbon emissions is vast and range from clean energy generation such as wind power to forest conservation to livestock waste management. Companies engaging in these environmental projects can claim to have produced offsets as long as the carbon removed is in excess of what would have been occurred in the absence of the offset. For example, one cannot label a project as generating carbon offsets if it would have happened without the funding generated by carbon offsets. The most popular types of projects involve either agricultural land use or forestry and many are located in developing countries.

In order to guarantee the validity of the carbon offsetting claims, common practice in the industry is to gain third-party certification for projects. In some cases buyers transact directly with the offset generator, while in other cases sophisticated markets have developed which allow carbon offsets to be traded. At the present, numerous companies are involved in marketing and trading carbon offsets, and numerous concerns about permit legitimacy underscore the importance of the verification process. It is often very difficult to accurately verify an alleged certification, since offsets are often purchased for future projects and the baseline level of emissions is often debatable. To address these concerns, PG&E engaged a reputable third-party certification organization, Climate Action Reserve[24], to verify the emissions reductions associated with its offsets. PG&E circus a broad solicitation to purchase offsets, and each ton of greenhouse gas abated must then be certified by the Climate Action Reserve. Nonetheless, the implications of the findings in this study should be considered in the context of the broader market.

While a majority of the carbon market consists of regulated markets such as the EU Emission Trading Scheme, which covers the emissions of several thousand energy intensive European companies, a relatively small fraction of the market consists of voluntary carbon offsets. In 2011 the voluntary carbon market had a total value of $576 million down from a peak of $728 million in 2008 ((Bloomberg 2012)). In spite of the decline in this market resulting from the recent financial crisis, the voluntary offset market is particularly popular in North America where offsets are purchased through numerous over-the-counter contracts. Since American buyers appear to prefer more local projects, the majority of carbon offsetting projects red to the voluntary market are now to be found within the US.

Individual consumers are currently offered a number of ways in which they can offset their carbon footprint. Airline passengers are routinely asked if they wish to offset their carbon footprint resulting from air travel (sometimes at considerable cost). Voluntary carbon offsetting programs have also recently been offered to residential consumers in order to offset the carbon footprint resulting from everyday energy use at home.

[24]http://www.climateactionreserve.org/

A.2 Seasonality Corrections

A.2.1 Interactive Effects

One concern is that the seasonal controls (aggregate month-by-year effects) may be insufficiently capturing important sources of heterogeneity in time-varying unobservables. For example, if young households are more vulnerable to economic shocks or adverse weather conditions, electricity usage patterns within this cohort may exhibit higher volatility which is not fully captured by the mean time effects we control for. This would represent an unobserved source of heterogeneity which may potentially bias our estimation. At the core of this problem is the extent to which the assumption on the error term in equation 5 is correct. We can think of the total error term as given by:

$$u_{it} = \alpha_t + \gamma_i + \epsilon_{it}, \tag{6}$$

where ϵ_{it} is iid across households and time.

We are concerned that the true model may in fact have interactive effects of the form:

$$u_{it} = \gamma_i + \omega_i F_t + \epsilon_{it}. \tag{7}$$

where F_t is an aggregate effect that is scaled by some demographic variable, ω_i. Such a model is not directly estimable, but the presence of demographic variables offers a way to test the validity of this concern and, at least partially, to eliminate it. We can consider various proxies for ω_i without necessarily requiring the unobserved trends F_t to vary over each individual. We can divide the sample into different groups (e.g. by age quartiles) an estimate a model under the following assumption:

$$u_{it} = \gamma_i + \omega(1(i \in Group\ g))F_t + \epsilon_{it}, \tag{8}$$

where ω is now observable for each i and F_t is approximated by year-month specific indicator variables.

In Table A.1, we present the coefficients from equations 3 and 4, but with month and years controls interacted with an array of demographic characteristics, and each cell again represents the estimate of β from a separate regression. The results are consistent with the baseline difference-in-differences (zero effect) and first differences estimates (1.5-2.5 percent rebound).

It also possible that the true model has more than one interactive effect and may be multidimensional with an error term that has an unknown nt factor structure. As an additional robustness check we have implemented the control variable approach described in (Pesaran 2006) and (Harding and Lamarche 2011)). The result was also very similar to the baseline specification indicating that our estimation is robust to a variety of interactive effects specifications.

A.2.2 Time Series Filtering

Finally, it is possible that the interactive effects specifications discussed above may not capture all the relevant heterogeneity. One such case is when the error term can be decomposed into two components, ϵ_{it} which is iid across observations and another component v_{it} which is non-stationary and exhibits seasonality and trending behavior. This case requires us to filter the time series of electricity consumption for each household. We use the H-P (Hodrick-Prescott filter) commonly used in macroeconomics. Consider some arbitrary household. To remove the trend from log electricity consumption for that household $ln(k_t)$, we decompose $ln(k_t) = \tau_t + c_t$, where τ_t is the trend, and c_t is the cyclical component. τ_t is estimated by solving

$$\min_{\{\tau_t\}} \sum_{t=1}^{T}(x_t - \tau_t)^2 + \mu \sum_{t=2}^{T-1}[(\tau_{t+1} - \tau_t) - (\tau_t - \tau_{t-1})]^2 \tag{9}$$

The parameter μ penalizes variation in the first difference of the trend and is set to 6.25, 1,600, and 129,600 ((Hodrick and Prescott 1997); (Ravn and Uhlig 2002)). We then use the filtered residual $c_t = ln(k)_t - \tau_t$ as the new dependent variable in equation 5. Figure A.1 shows the intuition behind the H-P filter for the time series of consumption for one arbitrary household. Notice the effect of the choice of the smoothing parameter μ. This suggests that a small value of μ is required in order to remove the cyclical component. It is important to note an important limitation of this approach. Since this method is applied at the individual level it will remove to a large degree any pre and post adoption trends in behavior (such as pre-adoption conservation and the disappearance of the "rebound effect" post-adoption. The individual filtering approach however allows us to focus on the immediate post-adoption period and remains reliable in detecting an immediate jump in consumption after the household enrolls in the CS program.

In Figure A.2 we present the results for the event study conducted on residuals from different applications of the H-P filter with various degrees of smoothing μ. We see that even though the H-P filter removes some of the pre and post adoption trends it continues to clearly identify a discontinuity in usage at the time of adoption. The effect is slightly diminished but we still observe a 2 percent increase in consumption post adoption.

A.3 Profile of Adopters

Given the surprising and robust behavioral response resulting from adoption, it is important to understand if customers may differ along predictable demographic dimensions. This has substantial managerial implications for program design and customer targeting. In order to investigate how selection into adoption is driven by observable characteristics of the households, we construct a household specific variable T_i, which equals one if household i signed up for the CS program, and zero otherwise. We use households that never sign up ($T_i = 0$) as our reference group and we formally identify the model by normalizing the corresponding coefficients. We wish to model the probability of adopting the CS program by household i conditional on observed covariates x_i, $Pr(T_i = 1|x_i)$. We encounter one important technical limitation however. By construction, our sample is non-random. In fact our very data request from PG&E was not formulated as a random sample of all PG&E customers. Given the low number of CS program adopters relative to PG&E's large residential customer base this would have been impractical as adoption would have been a very rare event in a random sample. As such we chose to sample conditional on adoption status. The final sample contains a sizable proportion of CS program adopters and a very small but representative sample of the population of non-adopters subject to the restrictions on residence imposed earlier to achieve balance.

This sampling framework is usually referred to as "choice based sampling" or "retrospective sampling" since it uses the ex-post outcomes as part of the sampling frame. It is well known in this setting that estimation by maximum likelihood leads to inconsistent parameter estimates (Manski and Lerman, 1977). While several approaches are available to address this issue, consistent estimates are typically obtained by pseudo-maximum likelihood where observations are weighted by a factor $\mu_j = n_j/(NPr(T_j))$, where n_j corresponds to the observed sample in group j. N and $Pr(T_j)$ however are population parameters denoting the total population of possible adopters and $Pr(T_j)$ the unconditional probability of adoption in period j. These quantities are not observed in the sample (we cannot simply assume that the ratio of adopters to non-adopters from a short-run program corresponds to the respective population adoption ratios).

In order to avoid controversies over population priors we rely on a stronger functional form assumption and assume that $Pr(T_i = 1|x_i)$ can be written in a multiplicative intercept form (Hsieh, Manski and McFadden, 1985). The logit model is a particular example of the multiplicative intercept form and thus we assume that:

$$Pr(T_i = 1|x_i) = \frac{exp(c_1 + x_i'\beta_1)}{1 + exp(c_1 + x_i'\beta_1)}, \tag{10}$$

where β_1 is a parameter vector which measures the extent to which the observed covariates explain adoption. Note that the coefficient β_0 is normalized to 0. Thus, the results can be easily interpreted. For each covariate of interest a positive coefficient in β_1 tells us that a particular regressor makes it more likely that a household with that characteristic will adopt. The estimated intercept coefficient, c_1, is however inconsistently estimated and is a function of the unknown parameter, μ. This imposes some restrictions as it prevents us from computing marginal effects without imposing out-of-sample priors on this unobserved parameter.

With these econometric subtleties in mind, let us now turn our attention to Table A.2. This table presents estimates of the slope coefficients of the adoption equation for different specifications with an increasing number of explanatory demographics. The results are very similar across different specifications. All else equal, adopters in our sample are younger, wealthier, and live in homes with a smaller number of inhabitants. The first two attributes act as one might expect. Since the adopters enrolled through the PG&E website, this shows that younger households are more likely to invest the effort in searching and finding environmental programs. Similarly, wealthier households appear more willing to incur the costs of search and the (albeit small) increase in electricity prices from enrollment. The mechanism for the importance of household size is open to interpretation. Perhaps there are costs to coordinating with many inhabitants. Autonomy may also play a role; one may view it less advantageous to enroll in an energy-related program when the energy use decisions are made by many different people.

Softer household characteristic also appear to be important, consistent with the findings of (Costa and Kahn 2013). Adoption is strongly predicted by the *Environmental* interest variable. This is consistent with the stylized model in Section 2 as this variable proxies for δ, the extent to which a household is aware of the social cost of carbon emissions. It is interesting to note however that *Green Living* is not a significant predictor for adoption and in fact it has a negative sign. This may indicate complementarities across domains, whereby if a household already is involved to a substantial degree in other environmental activities they are less likely to adopt a new one. An alternative interpretation may have these households preferring to conserve as part of their lifestyle, rather than purchasing conservation in the form of offsets from an external source. As expected, lifestyle variables related to a perceived interest in the outdoors or activities related to wildlife or camping are also important predictors of adoption. Their presence is likely to also be correlated with the degree to which a household perceives carbon emissions and global warming to be a potential utility cost.

The extent to which a household is involved in the community and local charities also appears to be a strong predictor of adoption. This is also consistent with our model as it reflects overall awareness and concern for the local community. By contrast the propensity to contribute substantial amounts to charitable causes is a negative predictor for adoption. This indicates that adoption into the program is not seen as a charitable contribution or expression of altruism. This is not indicative of a contradiction since it is common to think of contributing to the community financially as being very different than contributing time or effort. The propensity for adoption is substantially higher among high income households. It is interesting to note that adoption does not appear to be driven by the level of education. The presence of children is insignificant as a driver of adoption. While the relatively weak impact of children on adoption may also be explained by the higher age in our sample as a consequence of the time series balance requirement, most of these households will have adult children. Perhaps it shows that they discount the welfare of future generations to a substantial degree, although there are a variety of possible explanations. We find no statistically significant differences between renters and owners. This may also be a consequence of our balance requirement since renters are more likely to be excluded from our sample due to their transitory dwelling patterns.

Table A.1: Robustness of model specifications to interactive trends.

Difference in difference specifications

Time Control Interactions:	None	Age	Income	Climate Zones	Avg. Usage
ClimateSmart	0.00087	-0.00864	-0.00068	-0.00300	0.00558
	(0.008)	(0.008)	(0.008)	(0.007)	(0.007)
HH FEs	Y	Y	Y	Y	Y
Month-by-year FEs	Y	Y	Y	Y	Y
R-Squared	0.094	0.094	0.094	0.094	0.094
Observations	665,201	665,201	665,201	665,201	665,201

** Significant at the 0.10 level, ** Significant at the 0.05 level, *** Significant at the 0.01 level.*
Standard errors clustered at the HH level.

First difference specifications

Time Control Interactions:	None	Age	Income	Climate Zones	Avg. Usage
ClimateSmart	0.01528*	0.01992**	0.01715*	0.01299	0.01917**
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
HH FEs	Y	Y	Y	Y	Y
Month-by-year FEs	Y	Y	Y	Y	Y
R-Squared	0.120	0.103	0.108	0.173	0.104
Observations	635,761	576,568	635,761	635,114	635,761

** Significant at the 0.10 level, ** Significant at the 0.05 level, *** Significant at the 0.01 level.*
Standard errors clustered at the HH level

Figure A.1: Example of different smoothing parameters in the HP filter.

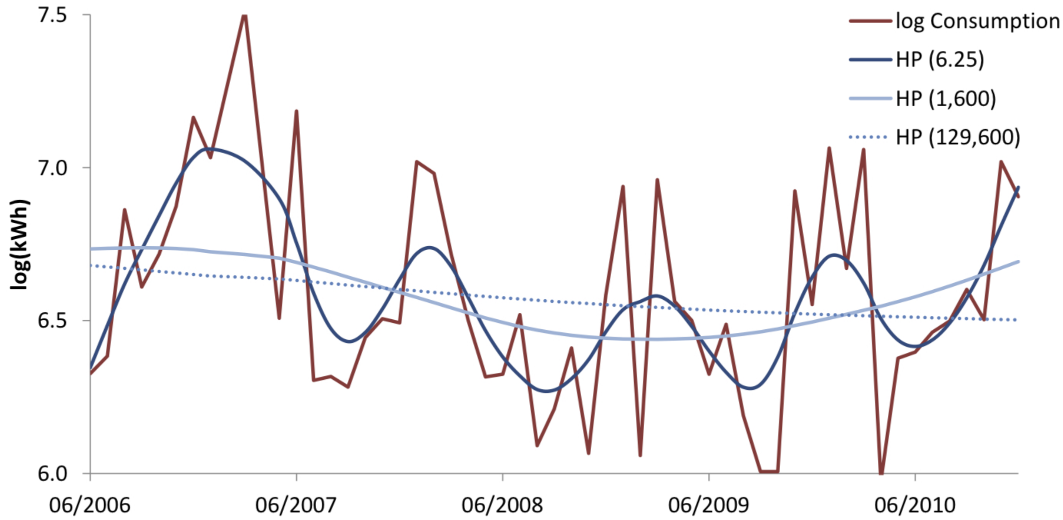


Figure A.2: Estimation of the dynamic effect of adoption in event time after filtering individual observations.

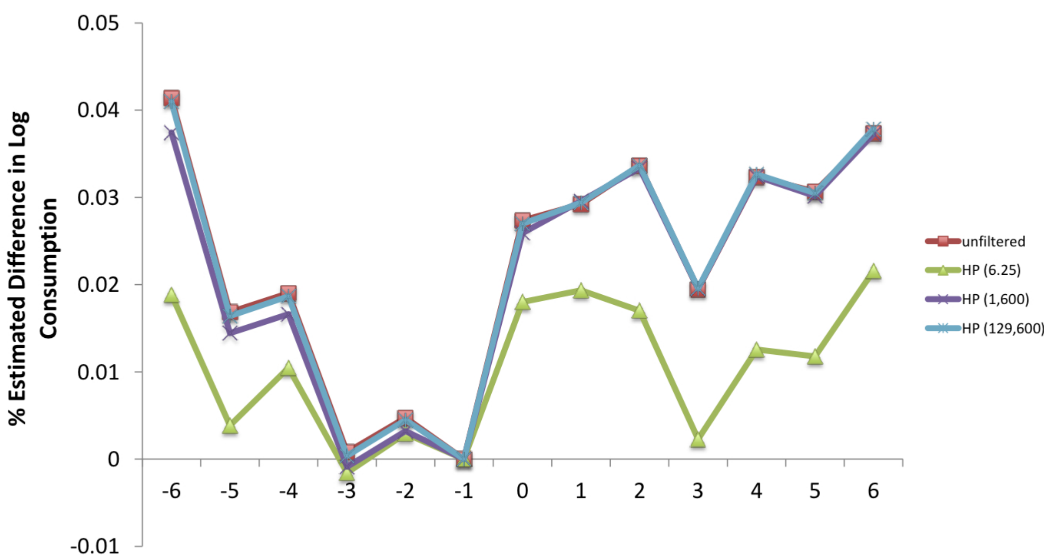


Table A.2: Logit model of ClimateSmart adoption.

Column:	(1)	(2)	(3)	(4)	(5)
log(kWh)	-0.17302***	-0.12429	-0.17130*	-0.19701**	-0.17220
	(0.061)	(0.089)	(0.089)	(0.090)	(0.111)
Age		-0.03010***	-0.03448***	-0.03450***	-0.03709***
		(0.004)	(0.004)	(0.004)	(0.004)
College		-0.04015	-0.04151	-0.04335	-0.07702
		(0.097)	(0.097)	(0.097)	(0.105)
HHIncome $80k+		0.21283**	0.26492***	0.26281***	0.26869**
		(0.094)	(0.094)	(0.094)	(0.109)
Children		-0.04317	0.09821	0.12764	0.13624
		(0.106)	(0.112)	(0.113)	(0.121)
Working Woman		-0.07088	-0.06821	-0.08552	-0.06526
		(0.094)	(0.097)	(0.097)	(0.106)
HH Size		-0.18015***	-0.20402***	-0.21210***	-0.20545***
		(0.043)	(0.043)	(0.044)	(0.047)
Home Owner		0.15370	0.23859	0.22109	-0.22102
		(0.262)	(0.265)	(0.265)	(0.488)
Environmental			0.95556***	0.78533***	0.77090***
			(0.115)	(0.135)	(0.148)
Green Living			-0.20375	-0.22710	-0.30806*
			(0.146)	(0.145)	(0.159)
Charity			0.36800***	0.30067***	0.26789**
			(0.107)	(0.109)	(0.116)
Charitable			-0.18636	-0.20152	-0.16751
			(0.132)	(0.132)	(0.145)
Outdoors				0.20561	0.11840
				(0.129)	(0.139)
Wildlife				0.13045	0.11862
				(0.160)	(0.177)
Camping				0.20871*	0.29793**
				(0.113)	(0.123)
Home Age					0.00811***
					(0.002)
Heating					0.12859
					(0.107)
Cooling					-0.02528
					(0.164)
Sqft 2500+					0.05690
					(0.148)
Home Value $500k+					0.16769
					(0.124)
Pool					-0.20900
					(0.142)
Constant	*-1.80536****	*-0.15083*	*0.18766*	*0.23730*	*0.38445*
	(0.384)	*(0.567)*	*(0.574)*	*(0.574)*	*(0.846)*
Observations	14,197	10,266	10,266	10,266	8,583
Pseudo R-squared	0.00128	0.0276	0.0501	0.0528	0.0613

*Robust standard errors in parentheses. * Significant at the 0.10 level, ** Significant at the 0.05 level, *** Significant at the 0.01 level*