Success, Failure, and Information: How Households Respond to Energy Conservation Goals

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Abstract

This paper investigates how households respond to repeated energy conservation goals. I track households' program participation and electricity use decisions across successive energy conservation challenges offered by a large electrical utility company. Using two complementary empirical strategies, I find that the policy causes households to reduce their electricity use but these reductions rebound close to pre-program levels as their participation ends. I also find that households' decisions whether to re-enroll in the program and attempt a subsequent goal are sensitive to their success or failure in achieving their energy conservation goal but, conditional on their success, do not depend on their reductions in electricity use. As a result, households make the opposite decision from what the policy design incentivizes; households facing an increased likelihood of achieving their next conservation goal are less likely to re-enroll and attempt another goal. Importantly for designing general incentive policies, this suggests that households use simple heuristics in making decisions rather than incorporating the full information on their success and effort that is readily available to them.

Keywords: Goal-setting, Energy Conservation, Electricity, Financial Rewards
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1 Introduction

Many governments and companies use energy conservation and demand-side management (DSM) policies to help customers modify their electricity consumption or to address environmental externalities. These policies are increasingly enabled by new technologies like smart meters and complement the growing deployment of renewable energy. Energy conservation and DSM policies fall into two broad and often overlapping categories. First, policies providing information through more transparent or frequent information on prices and quantities,[1] or in specific ways based on behavioural nudges like social norm comparisons.[2] Second, policies that change incentives by altering price schedules non-linearly over time or quantity, or by setting targets or rewards for achieving specific objectives.[3] Policies where participants can repeatedly attempt goals are widespread outside of energy conservation and contain features of both these categories: they change incentives by providing financial and/or non-financial rewards for achieving goals, typically provide information to participants on their progress and/or degree of success, and often leverage behavioural nudges based on messages of success or failure.

This paper considers a long-running energy conservation program that repeatedly offers households financial rewards for achieving annual energy conservation goals. BC Hydro's Team Power Smart program has been in place for over 10 years in the Canadian province of British Columbia and has been undertaken by tens of thousands of participants. The program offered households the opportunity to attempt annual electricity conservation 'challenges' in exchange for $75 financial rewards if they succeeded in reducing their electricity use by 10% compared to the previous year. At the completion of each energy conservation challenge, all households were given information on their degree of energy conservation — their degree of success — and whether they passed or failed their 10% conservation goal. Importantly, all households, whether successful or not, were then given the opportunity to attempt another 10% electricity conservation goal for another $75 in the following year. This structure allows households' extensive margin participation decisions to be observed alongside their intensive margin energy conservation decisions, and provides an excellent setting for studying how consumers respond to information, respond to success versus failure in achieving goals, and the long-run persistence of energy conservation.

The program studied here is especially relevant to the role of information in electricity conservation, and goal setting and financial reward programs. Households do not typically observe real time energy prices or amounts used, and thus how they interpret and respond to information on their use is particularly important. For example, policies that change

[1] Gans et al. (2013); Sallee (2014); Kahn and Wolak (2013); Datta and Gulati (2014); Sexton (2015); Wichman (2017); Schleich et al. (2017); Byrne et al. (2018b); Martin and Rivers (2018); Jessoe and Rapson (2014)

[2] Particularly relevant policies include Abrahamse et al. (2007); Allcott (2011); Allcott and Rogers (2014); Delmas and Lessem (2014); Dolan and Metcalfe (2015); Ito (2015); Allcott and Kessler (2017); Gillingham and Tsvetanov (2018).

[3] Byrne et al. (2018a); Wolak (2011, 2006); Ito (2014); Borenstein (2009)

price schedules require consumers to be sufficiently informed of their own energy use and prices to respond to the incentive as intended. Previous work finds that consumers may be inattentive to their energy use (Palmer and Walls, 2015), and may respond to information in complex and unexpected ways: the simple arrival of a bill (Gilbert and Graff Zivin, 2013), billing frequency (Sexton, 2015), or letters comparing households to their neighbors (Allcott, 2011) can alter electricity use. These examples raise the question of what aspects of information consumers are responding to in making their decisions; specifically, whether they use the full set of information available to them, or respond only to simplified subsets of that information, and in possibly sub-optimal ways. This includes how people respond to setting goals with their inherent outcomes of success or failure.

Goal setting is widespread across many settings and has been used, including with financial rewards, to encourage energy conservation (Ito, 2015; Harding and Hsiaw, 2014).[4] The program studied in this paper allows an additional margin of response to be studied: the extensive-margin decision whether to continue participation and attempt another goal. The decision whether to participate, and the impact of success or failure on subsequent participation, is particularly important to study as many policies are voluntary or involve repeated goals. Outside of energy use it is known that success and failure have important impacts on subsequent decisions, for example on whether to attempt additional competitions (Buser and Yuan, 2019; Gill and Prowse, 2014), the difficulty of the subsequent goals chosen, (Buser, 2016) and attitudes to risk (Thaler and Johnson, 1990).

Self-selection creates concerns that rewards paid to participants are "non-additional" and that energy conservation credited to a policy would have happened in the policy's absence (Boomhower and Davis, 2014). This additional decision-to-participate margin, compared to mandatory policies or experiments, also complicates causal identification. I address the inherent identification challenges of self-selection by employing two complementary empirical approaches — event-study and fuzzy regression-discontinuity strategies — and I find two principle results.

First, I use an event-study model and 10 year panel to estimate the short-run reductions and long-run persistence of changes in electricity use. I find that an initial conservation goal is associated with an immediate 4.4% average reduction in electricity use which lasts throughout the twelve months of the initial challenge. However, while electricity use continues to decline among households that re-enroll it rebounds close to pre-program levels as households end their participation in the program. This rebound shows that households tend to make only short-run adjustments rather than permanent investments or create persistent habits, and that the ongoing incentive of additional goals and financial rewards is important for long-run lower electricity use.

Second, I compare household's decisions whether to re-enroll and attempt a subsequent goal to their energy conservation achieved under their initial goal. I find that success in the initial goal causes an approximately 50% increase in the probability they attempt a

[4] A larger literature, primarily from psychology, studies energy use and goal setting, typically in experimental settings with limited sample sizes. See for example Abrahamse et al. 2007, McCalley and Midden (2002) and van Houwelingen and van Raaij (1989).

subsequent goal, yet information on their actual conservation effort and degree of success — which affects their likelihood of achieving the next goal — has no effect on their decision to re-enroll. This responsiveness only to success or failure creates a large discontinuity in the probability that they re-enroll and attempt an additional goal, and as a result households make the opposite decision from what the program design incentivizes; households facing an increased likelihood of achieving their next conservation goal and receiving the reward are less likely to re-enroll and attempt another one. I then exploit this discontinuity in re-enrollment in a fuzzy-regression discontinuity design, and find that attempting an additional conservation goal causes large additional reductions in energy use. Consistent with the event-study estimates, this further supports that the continued incentive of the goal and reward is important in causing long-run lower electricity use.

Importantly, despite the program's voluntary nature and well-founded concerns about non-additional reductions in energy use, I find no evidence that households are responding strategically in order to receive credit for reductions that would have happened even in the absence of the program. In contrast, self-selection in a voluntary policy with a considerable component of luck — which is widespread in energy conservation policies due to large shocks to energy use from weather changes or idiosyncratic sources — can increase the policy's cost-effectiveness. This occurs when self-selection raises the share of participants who are responding to the incentive and reduces the share who succeed only due to idiosyncratic shocks.

The strong responsiveness to success versus failure found in this paper is particularly important, as many decisions involve the repeated process of exerting effort towards an objective, observing information on the outcome, and deciding whether to try again. The large discontinuity in decisions to attempt a subsequent goal, and lack of response to their previous effort, suggests that households are using a simple heuristic in making participation decisions rather than respond continuously to the goal and financial reward incentive and the information they have available. Helpfully, the program had an unusual feature where the ex-ante announced goal for success (a 10% reduction) that households expected differed from the ex-post threshold (a 9.5% reduction) used to tell and reward customers for passing the energy conservation challenge. This feature provides additional evidence that the discontinuity in self-selection into re-enrolling is due to households' responsiveness to their success or failure, and not due to unobservables such as heterogenous attention and attempted sorting at the 10% target.

The rebound in electricity use contrasts with the findings of Ito (2015), and with Harding and Hsiaw (2014)'s findings for households setting realistic goals.[5] These differing results

[5]Several other papers have considered financial rewards and energy conservation. Dolan and Metcalfe (2015) undertake a randomized controlled trial (RCT) and find large financial rewards cause a large conservation over the two months of their treatment period. They find persistent effects over the two post-program months they observe. Several papers, primarily from the psychology literature, have also undertaken randomized control trials of financial rewards for energy conservation (Mizobuchi and Takeuchi, 2012; Midden et al., 1983; McClelland and Cook, 1980; Winett et al., 1978). However, these consider very short timeframes, small sample sizes, and unrepresentative electricity users and are of limited use in understanding how households respond to financial rewards. Gerard et al. (2015) study a

suggest that whether households respond to conservation goals and financial rewards with persistent reductions may depend on the way electricity is used within a home and vary significantly by region. Alternatively, the context in which a financial reward/goal setting program is offered — for example a mandatory program during an electricity crisis as in Ito (2015), or a voluntary program as part of routine electricity use as in this paper — may determine whether households respond with short-run reductions or persistent changes. This paper's finding of continued reductions in electricity use when households re-enroll, and a rebound when they don't, is similar to the "action and backsliding" found by Allcott and Rogers (2014) in households' responses to repeated home energy reports.

The remainder of this paper is organized as follows. Section 2 describes the institutional setting, design of the Team Power Smart electricity conservation program, and data. Section 3 gives an overview of the two empirical approaches used in this paper. Section 4 describes the event study empirical strategy and results. Section 5 explores households' decisions whether to leave the program, describes the fuzzy-regression discontinuity strategy, and presents its results. Section 6 discusses the cost effectiveness of the program, and I conclude in Section 7.

2 Institutional Setting, Program Design, and Data

BC Hydro is Canada's second largest integrated electrical utility company and serves 1.7 million residential customers covering 95% of the population in British Columbia (BCH, 2014). BC Hydro is owned and has a mandate set by the provincial government of British Columbia. The B.C. government, through its Clean Energy Act, has required that BC Hydro "[meet] at least 66 per cent of the expected increase in demand through conservation and efficiency by 2020" (BCH, 2014). As part of their efforts to achieve this mandate, BC Hydro launched a new conservation initiative—a program targeting ongoing behaviours called Team Power Smart.[6] Team Power Smart is a voluntary program promoted and summed up by BC Hydro with "*Looking to save money on your electricity bills? Become a member of Team Power Smart and challenge yourself to reduce your home's electricity use by 10% in the next year. If you're successful, you can earn a [$75] reward.*"[7] This electricity conservation challenge requires households to reduce their aggregate electricity use over the 12-month challenge by 10% relative to a conservation target. Each household's conservation target is their own annual electricity use over the preceding 12 months,

suite of mandatory incentives introduced in response to an electricity supply crisis in Brazil, including financial rewards, but cannot disentangle the effect of different policies.

[6]It is possible for households to join Team Power Smart to view their electricity use online without undertaking a conservation challenge. For simplicity, I will use Team Power Smart to refer to those households which also undertake a conservation challenge. I do not observe households which registered online without undertaking a conservation challenge.

[7]BC Hydro Team Power Smart website landing page, accessed June 2017. The reward value for challenges studied in this paper is $75 CDN which was reduced to $50 in September 2014. I exclude households undertaking a challenge under the new $50 reward value. $75 CDN is equivalent to approximately $75-$55 USD over the period of study.

adjusted for changes in heating degree days to help prevent households from being unduly penalized or rewarded for changes in weather. Beginning a challenge requires a minimal time cost of registering online. Households can start a challenge in any month of the year as long as they have 12 months of electricity use in their current home to establish their target. Online signup ensures that all participants can view their progress towards their conservation target through the BC Hydro website and access a variety of tips and suggestions for reducing their electricity use. The online account provides households with feedback on both their monthly and cumulative progress towards their annual 10% conservation target.[8] Because it is the aggregate annual conservation that matters for success in a challenge, households can miss their 10% target in any month and still pass the challenge.

Upon completing the 12 months of the challenge BC Hydro undertakes a final evaluation of household's cumulative conservation. BC Hydro applies the final weather adjustment, accounts for bi-monthly billing and any idiosyncratic factors, and evaluates whether the household passed or failed their challenge. While the conservation target advertised to customers is 10%, BC Hydro evaluates final success or failure against a 9.5% conservation threshold. Households that reduced their electricity use by greater than or equal to 9.5% below their target pass their challenge while the rest fail. BC Hydro notifies all customers of how much their electricity use changed alongside their success or failure and gives successful households the choice of a rebate through either a cheque or credit applied to their account. Household's degree of electricity conservation and their success or failure is provided to participants both through a letter at the end of their conservation challenge and through an online portal. The majority of households regularly log into their online portal so their responses are not due to a lack of awareness of their progress or success (Kassirer et al., 2014).

Structure of Additional Conservation Challenges

A novel feature of Team Power Smart is that all households have the option of re-enrolling in additional annual conservation challenges. Upon completion of each challenge both households that pass and fail their challenge are given the same option to start a subsequent conservation challenge for another $75 rebate. Each subsequent challenge follows the same process as the initial conservation challenge; households have a goal of another 10% conservation target measured and weather adjusted relative to their previous 12 months of electricity use. The new reduction target is independent of whether the prior 12 months contained a challenge or not, and independent of whether the prior challenge was a success or failure. The baseline for a household immediately starting an additional challenge would be the 12 months of the just completed challenge, while a household waiting 4 months before starting their next challenge would have a baseline set by the average of their last 8 months of their previous challenge and the 4 months of the gap prior to starting their next challenge. Because each additional 10% conservation challenge

[8] An example of the online portal is shown in Appendix Figures A.1a and A.2b.

is evaluated relative to the prior 12 months, the reduction in electricity use achieved by a household during a challenge affects their incentives on when and whether to undertake a subsequent challenge. Under the reasonable assumption of increasing marginal costs to electricity conservation, the greater the conservation achieved during a challenge the greater the incentive to postpone a subsequent challenge or leave the program.

Data and Household Characteristics

Under a non-disclosure agreement with BC Hydro I obtained an anonymized sample of monthly electricity billing records for 10,000 Team Power Smart program participants and 20,000 non-participants from January 2006 to December 2015.[9] The panel includes customers' Team Power Smart program participation history including the number of conservation challenges, each challenges start and end date, whether the challenge was successful, and the building and heating type of the household. Individual household characteristics including building type, number of bedrooms, assessed value, floor space, and the postal codes Forward Sortation Area were obtained from the provincial property assessment corporation, BC Assessment. Removing duplicate accounts, erroneous data, dropping households with electricity use more than 5 standard deviations from the mean, and removing households that joined Team Power Smart after the reward value was changed in September 2014, left a sample of 8,877 households participating in Team Power Smart.

Average monthly electricity bills among participant households are \$62—making the rebate reward of \$75 equivalent to 10% of a household's annual electricity bill, in addition to their bill savings. Differences in electricity use between participants and non-participants is almost entirely a composition effect; after controlling for building and heating type the average electricity use among participant households is 9.3 kWh (p-value 0.08), or 1.1%, higher than non-participants during the pre-program year of 2006.[10]

Outcomes During Multiple Conservation Challenges

Table 1 summarizes the decisions and outcomes of participant households across multiple conservation challenges. During the initial three challenges 60-62% of households decide to re-enroll in an additional challenge.[11] Consistent with an increasing difficulty of achieving additional reductions in electricity use, the unconditional probability of passing

[9] I define participant households as those which participate in Team Power Smart prior to the panel end in December 2015, and non-participant households as those which do not participate. The sample was selected from households which did not move over the panel period.

[10] The idiosyncratic and unknown process of advertising the voluntary Team Paper Smart program means differences in household characteristics between participant and non-participant households are not necessarily due to self-selection. These differences are described in Appendix A.2.

[11] The probability of re-enrolling declines in part mechanically among later challenges due to the limited panel length.

Table 1: **Probability of Challenge Outcomes**

Challenge number	HH's in Chal.	Probability Of Re-Enrolling All	if Failed Challenge	if Passed Challenge	Probability Of Passing All	if Failed Prev. Chal.	if Passed Prev. Chal.
1	8,877	0.62	0.55	0.77	0.34		
2	5,531	0.60	0.56	0.71	0.31	0.33	0.29
3	3,346	0.60	0.56	0.71	0.28	0.30	0.24
4	2,014	0.54	0.51	0.64	0.26	0.28	0.23
5	1,091	0.46	0.41	0.60	0.24	0.27	0.17
6	498	0.38	0.36	0.44	0.24	0.25	0.21
7	188	0.27	0.26	0.28	0.29	0.28	0.31
8	50	0.12	0.07	0.33	0.18	0.20	0.13
9	6	0.00	0.00	0.00	0.33	0.33	0.33

Notes: HH's in Chal. is the number of households undertaking their first, second, etc, challenge. Probability of Re-Enrolling is the probability of re-enrolling in a subsequent conservation challenge, conditional on being in the current challenge. Probability of Re-Enrolling if Failed [Passed] Challenge is the probability of re-enrolling conditional on failing [passing] the current challenge. The Probability of Passing is for a household's current challenge, while the Probability of Passing if Failed [Passed] Prev. Chal. is the probability of passing the current challenge conditional on the Fail or Pass status of the previous challenge.

a conservation challenge declines with additional challenges.[12] Households are more likely to re-enroll in another challenge if they pass, rather than fail, their current challenge. In contrast, households are less likely to pass their next challenge if they passed their previous challenge.[13] This pattern matches the incentive structure previously discussed; passing a challenge requires achieving the 9.5% conservation threshold, which makes passing the next challenge harder compared to not achieving the initial target.[14]

[12]This decline is statistically significant. Regressing an indicator for whether households passes their challenge on the Challenge number finds that decline in the probability of passing is statistically significant at the 1% level, (coefficient on *Challenge* of -0.0212, t-statistic of -9.91), but loses significance if restricted to challenges 6 through 9 (coefficient on *Challenge* of 0.0067, t-statistics 0.28).

[13]This difference, from Table 1, between the probability of passing conditional on a households success and the probability conditional on failure in the previous challenge is statistically significant. Regressing an indicator for passing the challenge on an indicator for success in the previous challenge finds that prior success is associated with a 4.65% (t-statistic -5.69) lower average probability of success in the following challenge. This difference is of a similar magnitude and statistically significant at the 5% level for individual Challenges 2-5, after which differences lose statistical significance.

[14]As households choose the start date of subsequent conservation challenges they could strategically establish a new baseline before undertaking their next challenge, or sign up after periods of unusual weather. In Appendix A.3 I find no evidence this is occurring.

3 Empirical Strategies

There are three principal challenges to estimating the causal effect of an energy conservation challenge. First, as in all voluntary programs, households may self-select into Team Power Smart based on observable and unobservable time invariant characteristics. This self-selection could make non-participant households an unsuitable counterfactual for electricity use among participant households, had they not participated in the program. Secondly, households may start their first conservation challenge based on shocks to their past electricity consumption or expectations of their future consumption. For example, households may select into the program in response to a particularly cold winter which caused a large electricity bill. Households may also take advantage of anticipated reductions in their electricity use such as the purchase of an efficient dryer or leaving on holiday. By signing up in advance or in conjunction with their anticipated reduction in electricity use, a household could receive credit for reductions in electricity use that were not caused by the conservation program. Lastly, all households have the option of continuing to additional conservation challenges. This makes the persistence of energy savings and the causal effect of subsequent conservation challenges dependent on households' decisions to select into additional challenges.

To address these challenges I employ two complementary empirical strategies. In Section 4 I use an event study research design to estimate the monthly average changes in electricity use associated with participation in Team Power Smart. By estimating changes over time within households, this strategy identifies program effects independent of self-selection into the program on observable and unobservable time invariant characteristics. For clarity, I use "program effects" to refer to changes in electricity use that are associated with participation in conservation challenges but which may or may not be causally due to the program. I restrict "treatment effect" to refer to standard causal effects. Plotting these estimates provides a visual time trend of the changes in electricity use leading up to the initial conservation challenge, during the months of each challenge, and over the months after a household leaves the program. These trends provide insight into potential self-selection into the program based on households' expectations of future electricity use and past shocks to their consumption, along with the persistence of reductions in electricity use and the effect of subsequent conservation challenges. This strategy provides detailed information on households' electricity conservation decisions and self-selection into the program, and finds no evidence that reductions in electricity associated with participation are not causally due to the program. However, because self-selection cannot be ruled out it cannot strictly identify causal treatment effects.

To identify causal treatment effects, Section 5 exploits a discontinuity in the probability that households continue to a second conservation challenge. This discontinuity is caused by households that just succeed in their conservation challenge being significantly more likely to continue to a second challenge compared to those households that just failed their challenge. I use this discontinuous probability change in a fuzzy Regression Discontinuity (RD) design which, under several identifying assumptions, identifies the Local Average

Treatment Effect (LATE) of a second conservation challenge despite self-selection on time-invariant characteristics, expectations of future electricity use, or shocks to households' past consumption (Lee and Lemieux, 2010). By comparing households' decisions whether to re-enroll in a subsequent challenge to their intensive margin reductions in electricity use, Section 5 also provides insights into how households respond to information on their conservation challenge success.

4 Event Study Empirical Strategy

The general event study model is of the form

$$y_{it} = \sum_{\tau=-T}^{T} \beta_\tau D_{i,t-\tau+1} + \alpha_i + d_t + \epsilon_{it} \tag{1}$$

where y_{it} is the log of monthly electricity use for individual i in month t, α_i is an individual fixed effect, and d_t is an indictor for date t. $D_{i,t-\tau+1}$ is a dummy variable equaling 1 if individual i in month t began treatment in month $t-\tau+1$, where τ is the measure of event time in months.[15] I define the start of treatment as the month a household undertakes its first conservation challenge, $\tau = 1$. β_τ are the non-parametric program effects τ months lag or lead of treatment and cover all periods in the panel of length T. Event study models can only identify the full set of program effects $\{\beta_\tau\}_{\tau=-T}^{T}$ up to a constant. That is, event study models can only identify changes in program effects relative to a baseline level.[16] In my preferred specification, I define the baseline as the second year before each household undertakes their initial conservation challenge. Using a baseline of the second year allows important pre-treatment trends in the 12 months preceding participation in the program to be estimated. Results are robust to other baseline periods; see Figure B.8 in the Appendix for a baseline of the third year pre-treatment. For the baseline of the second pre-treatment year, I estimate equation (1) while defining $\{\beta_\tau \equiv 0\}_{\tau=-12}^{-23}$. The non-parametric estimates $\hat{\beta}_\tau$ identify the average percentage change in monthly electricity use within a household relative to their average electricity use during the baseline period.

Estimating causal effects in the event-study model requires treatment to be as good as randomly assigned conditional on time-invariant characteristics. This fails if participant and non-participant households would not have parallel trends in the absence of treatment. In Appendix (B.1), I show that trends are parallel prior to beginning a conservation challenge. Households starting a conservation challenge in response to past consumption

[15] For example, consider an observation in December 2009 for a household that began treatment (began its initial conservation challenge) in October 2009. For this household and observation, $t = December\,2009$ and $t - \tau + 1 = October$ 2009. This finds $D_{i,t-\tau+1} = 1$ only for $\tau = 3$.

[16] This can be seen by adding a constant to all β_τ and noting that this constant is then collinear with both the full set of individual and date fixed effects.

shocks or expectations of future electricity use, such as from purchasing a new energy efficient appliance, will also violate the parallel trends assumption. Shocks to past consumption can be tested by examining pre-treatment trends. As I discuss in Section 4.1, I find no evidence households begin a conservation challenge in response to increases in their past consumption such as large electricity bills from unusually cold winters. Whether households begin a challenge based on their expected future consumption cannot be tested. For example, it is not possible to distinguish between a household participating in a conservation challenge which causally reduces their electricity use from a household purchasing an efficient dryer and the latter participating to take advantage of their upcoming decline in electricity use. In this case, reductions in electricity use that coincide with the start of participation in Team Power Smart would not be causal. Lacking a suitable instrument for initial participation or a discontinuity in eligibility for mandatory participation such as used by Ito (2015), the specific channel of self-selection based on expectations of future electricity use cannot be ruled out. Instead, as I discuss below, the pattern of estimated program effects provides evidence that such self-selection is unlikely to be substantial and that the estimated reductions are primarily causally due to the Team Power Smart program.

4.1 Event Study Estimates—Initial Conservation Challenge

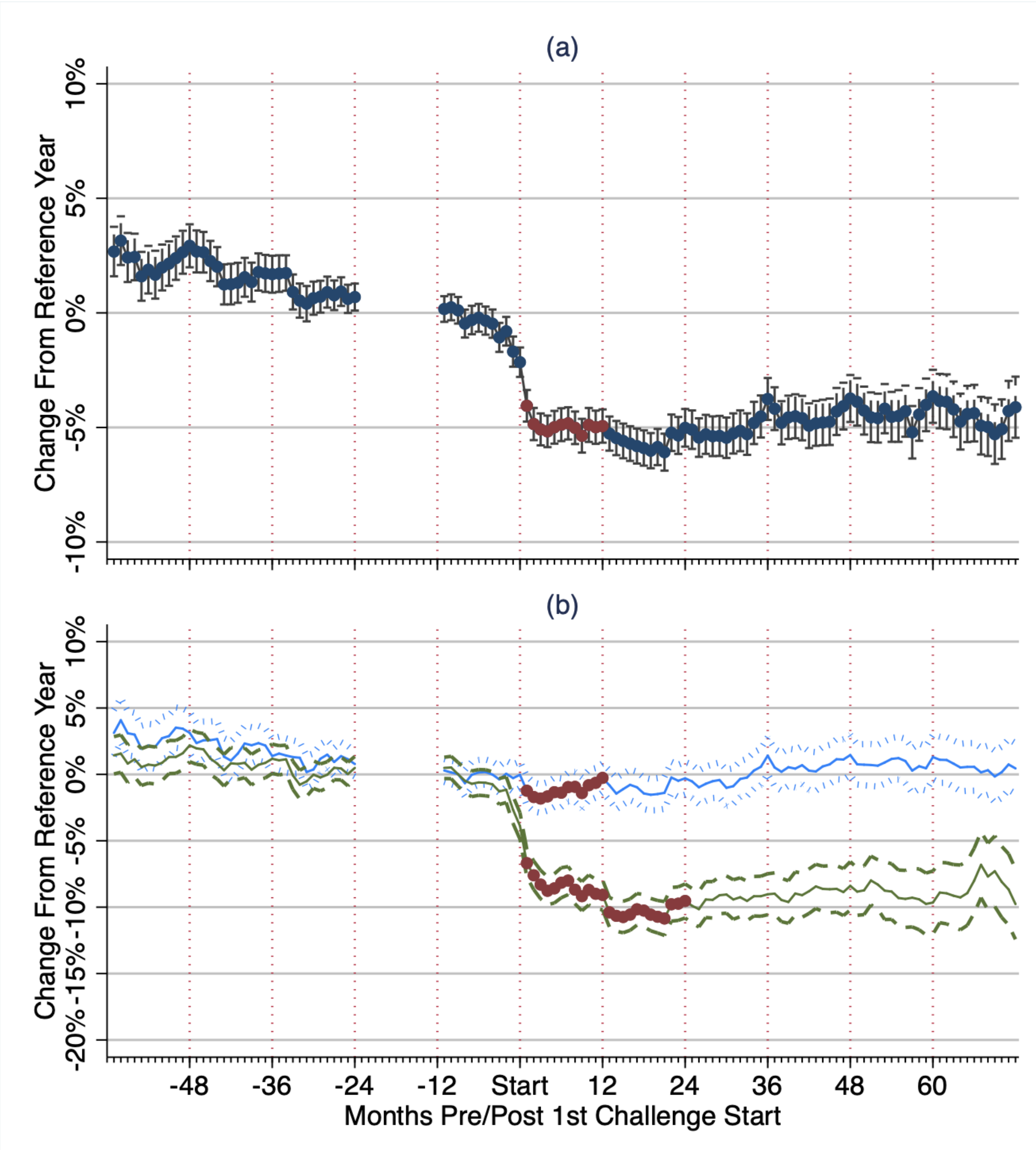
This section presents the results of estimating the event study model, equation (2), for all participant households and a comparison group of non-participant households.[17] To visualize the trend of program effects over event-time τ, I plot the estimates $\hat{\beta}_\tau$ in Figure 1(a).[18] The pattern of monthly estimates provides substantial insight into the short run and long run reductions in electricity use and whether reductions are causally due to the Team Power Smart program. The estimates in Figure 1(a) can be separated into three time intervals: the pre-treatment period leading up to the initial conservation challenge (months -60 to 0), the twelve months of the initial conservation challenge (months 1 to 12), and the months after the initial conservation challenge is completed (months 13 to 72).

In the second time interval, the twelve months of the initial conservation challenge show a

[17] Due to the small sample size for months far from the initial conservation challenge I pool monthly indicators before and after the ± 5 year estimation window around the initial conservation challenge into separate indicators. Results are robust to this simplification. To ensure a closer comparison group of non-participants to participant households, in the results below I also use a random sub-sample of non-participant households with the same composition of building type and heating characteristics as participant households. Results are robust to including all non-participant households; see Figure B.7 in the Appendix.

[18] Table E.8 of the Appendix lists all point estimates.

Figure 1: **Estimated Program Effects**: All households & re-enrolling households



Notes: Estimated program effects $\hat{\beta}_\tau$ with 95% confidence intervals from equation (2). The pre-treatment period is denoted by the months prior to *Start* ($\tau \leq 0$). The visual gap in estimates between months $\tau = -11$ and $\tau = -23$ is the excluded reference period. $\hat{\beta}_\tau$ identify the percent change in electricity use relative to the average electricity use within a household during this excluded reference year. Estimates include individual and date fixed effects and I cluster standard errors at the household level. Points estimates are in the Online Appendix. (a) Estimates, including the period after the initial challenge concludes ($\tau > 12$), contain both households ending participation and those that re-enroll. (b) Estimates, plotted here as a line for clarity, in blue are households that undertake a single challenge and then end their program participation. Estimates in green are for households that undertake at least two conservation challenges and continue to their second challenge within 12 months of completing their initial challenge. Not shown are estimates θ_g for electricity use during the gap between the first and second challenges. Months 13-24 are estimates of the average change in electricity use among households in their second conservation challenge independent of any gap between challenges.

substantial average reduction in electricity use of 4.9% relative to the baseline of the second year pre-treatment. BC Hydro measures households' reductions relative to the twelve months preceding a challenge; households achieve a 4.4% reduction relative to this year and their 10% conservation target. The lack of trend in monthly estimates of the initial challenge shows that, on average, households are not strongly increasing or decreasing their electricity conservation during the challenge. This stable conservation is consistent with households making either an initial investment that causes persistent reductions throughout the challenge, or maintaining a constant conservation effort throughout the challenge. The consistent program effects also indicate that self-selection to take advantage of upcoming short-term changes in electricity use, such as a holiday, is not a significant cause of participation; if they were, reductions in energy use would be expected to spike in the initial months of the challenge before partially rebounding.

In the first time interval, the months leading up to the initial conservation challenge show a gradual decline in estimated program effects. This trend could arise for two reasons. First, the pre-treatment trend could indicate a violation of the parallel trends assumption—differences in exogenous trends between the treated and control households—despite the lack of statistically significant different pre-trends estimated in Appendix (B.1). To test this, I estimate specification (1) without non-participant control households. This identifies program effects by comparing currently treated households to a control group composed of households treated at a later date in the panel (Borusyak and Jaravel, 2016). Estimates lose precision due to the smaller set of households but continue to show the same pre-treatment trend, showing this decline is not a due to a violation of the parallel trends assumption.[19] Second, the pre-treatment decline could reflect different time trends among participant households that are not fully controlled for by common date fixed effects. Figures 1 and 2 plot the program effects for households undertaking different numbers of challenges. This shows that the declining pre-treatment trend is limited to households undertaking only one or two challenges and, importantly, does not bias the short or long-run program effect estimates.

A threat to estimated program effects not being causal is households beginning a conservation challenge in response to a high electricity bill, such as after a cold winter. In this case, reversion to the mean would result in reductions in electricity use, relative to the previous year, being credited to the program. If this self-selection occurs it will manifest itself as positive pre-treatment effects in the months immediately prior to the initial conservation challenge. Instead, the opposite effect is found; the last two months (months $\tau = [-1, 0]$) in Figure 1 show a decline in electricity use relative to the previous months. These reductions in electricity use before the program starts do not count towards the conservation credited to households and, in addition, make it harder to achieve the financial reward as they lower the baseline from which the 10% conservation target is measured. This lack of a positive pre-treatment effect in the months prior to the challenge suggests that households do not self-select into the program based on past consumption shocks.

The pre-program decline of months $\tau = [-1, 0]$ may indicate that households are undertak-

[19] I plot estimates in Appendix Figure B.6.

ing electricity conservation prior to the program start. This could indicate self-selection into Team Power Smart as a result of making an energy efficiency investment which would bias the estimated program conservation upwards. There are two reasons such self-selection and bias to the estimated program effects is unlikely to be large. First, participation in Team Power Smart as a result of making an investment would result in persistent reductions in electricity use; in contrast, subsection 4.2 shows that electricity use rebounds as households leave the program. Second, the pre-program roll-off can result mechanically from the billing process. During the period of the panel studied in this paper BC Hydro does not record electricity bills on a fixed monthly basis.[20] Instead, BC Hydro uses a rolling billing period where different houses are billed on different days of not more than 62 days and the use is calendarized to monthly consumption. As a result, reductions that occur after the start of a conservation challenge cannot be separated within a billing cycle from electricity use that occurred prior to the challenge start. This can result in reductions due to a conservation challenge being partially credited to up to the last two months before a household begins its challenge.[21]

The third time interval includes all months after the initial challenge completes, months $\tau = [13..72]$. Pooling all participant households in estimating (1) includes those ending their program participation after their first challenge, those immediately continuing to additional challenges, and those waiting several months to years before starting a subsequent conservation challenge. Estimates $\hat{\beta}_\tau$, $\tau > 12$ are the average change in electricity use across these households, including any rebound in electricity use, and additional treatment effects from subsequent challenges undertaken by households. While Figure 1 shows that participation in Team Power Smart is associated with long-run average reductions in electricity use, this does not distinguish persistent energy savings due to a challenge, from the effects of subsequent challenges. To distinguish these effects I modify the event-study model (1) and estimate it separately for households ending their participation and those re-enrolling in subsequent conservation challenges.

4.2 Event Study Estimates—Additional Conservation Challenges

Comparing estimated program effects across households that undertake different numbers of conservation challenges sheds light on both the process of self-selection into additional challenges and the persistence of energy savings. To do this, I pool households by the number of conservation challenges they undertake and estimate separate event study

[20] BC Hydro began a rolling installation of Smart meters in 2011 which allowed collection of hourly electricity use. The event study estimates of this section includes households beginning their initial challenge prior to February 2013 and so will include some households with electricity use recorded at a higher frequency. However, all data provided has been harmonized and calendarized by BC Hydro to a monthly level regardless of the recording frequency. As a result, challenges over the period studied are recorded as beginning on the first day of a month regardless of when during the month a household signed up for a challenge, or when the electricity meter was read.

[21] Households could also potentially begin reducing their electricity use in response to a conservation challenge but not complete their online registration until some weeks later.

models for each group of households, equation (2)

$$y_{it} = \alpha_i + d_t + \sum_{\tau=-59}^{72} \beta_\tau D_{i,t-\tau+1} + \sum_{g=1}^{8} \theta_g G_{itg} + Pre_{it} + Post_{it} + \epsilon_{it} \quad (2)$$

where y_{it}, α_i, d_t, and $D_{i,t-\tau+1}$ are defined as in the event-study model of equation (1). I pool monthly indicators before and after the ± 5 year estimation window around the initial conservation challenge into, respectively, indicators Pre_{it} and $Post_{it}$. Some households undertaking multiple conservation challenges have a gap in time between when their previous challenge completes and they re-enroll in a subsequent challenge.[22] To account for this variable gap I include the indicator G_{itg} in equation (2). G_{itg} is 1 if household i in month t has completed challenge g but has not yet re-enrolled in challenge $g+1$. This indicator is not necessary for estimating the event study model; instead, it simplifies the comparison of program effects across households with different gap lengths. For households undertaking two or more challenges, including G_{itg} defines the coefficients $\beta_\tau, \tau = [12..23]$ as the program effects of the twelve months of the second conservation challenge regardless of the length of gap between conservation challenges. For households ending their participation after a single challenge, $\beta_\tau, \tau = [12..23]$ are the post-program effects for the first 12 months immediately following the initial challenge. Similarly, including G_{itg} defines $\beta_\tau, \tau = [24..35]$ as the program effects during a third conservation challenge for households that re-enroll, and as the second year post-program effects for households that end their participation. In my preferred specifications I pool all households that re-enroll within 12 months of completing their previous challenge and exclude households with longer gaps between challenges; results are robust to alternate restrictions on challenge gap lengths.

Estimates for Additional Conservation Challenges

I separately estimate equation (2) for six subsets of households depending on how many conservation challenges they undertake; these subsets are not mutually exclusive.[23] In Figure 1(b) I plot estimates $\hat{\beta}_\tau$ for households that end their program participation at a single challenge, and separately for households that re-enroll in a second conservation challenge. These two household groups show significant differences. Households that end their program participation after the initial challenge have average reductions during the challenge of 1.2%. Households that re-enroll in a second conservation challenge have average reductions during their first challenge of 7.2%. This suggests self-selection into additional challenges based on the reductions in electricity use achieved during the initial conservation challenge. Alternatively, it could be that households which are ex-ante likely to continue to additional conservation challenges are also those households that achieve large reductions in energy use.

[22] Appendix Figure A.3 shows histograms of these gaps.

[23] Point estimates are presented in Table E.9 in the Appendix.

Estimates $\hat{\beta}_\tau$ in Figure 1(b) for households that re-enroll are the average program effect including both households ending their participation after their second conservation challenge, and those that re-enroll in a third challenge. Figure 2(a) separates these households and plots estimates $\hat{\beta}_\tau$ for those undertaking only two conservation challenges against those that re-enroll in a third challenge. Both these household groups have similar conservation during their first challenge. However, during their second conservation challenge those households that end their participation show a rebound in their use over the months of the challenge. Their electricity use returns close to the pre-program levels as the challenge ends. In comparison, households that continue to a third conservation challenge continue to decrease their electricity use during their second challenge.

Figure 2(b) shows this pattern repeats again between households ending program participation after the third challenge and those re-enrolling in a fourth challenge.[24] Electricity use remains similar across both groups of households until the last challenge, it continues to decrease among those who re-enroll and rebounds among those who leave the program. This rebound does not return to the pre-program use; electricity use remains persistently lower than pre-program levels by approximately 6.5%.[25] In addition, the rebound occurs during the months leading up to the end of their final conservation challenge instead of after the challenge has completed. This is consistent with households stoping their conservation effort, or "giving up," prior to the end of the challenge.

4.3 Program Effects by Household Characteristics and Season

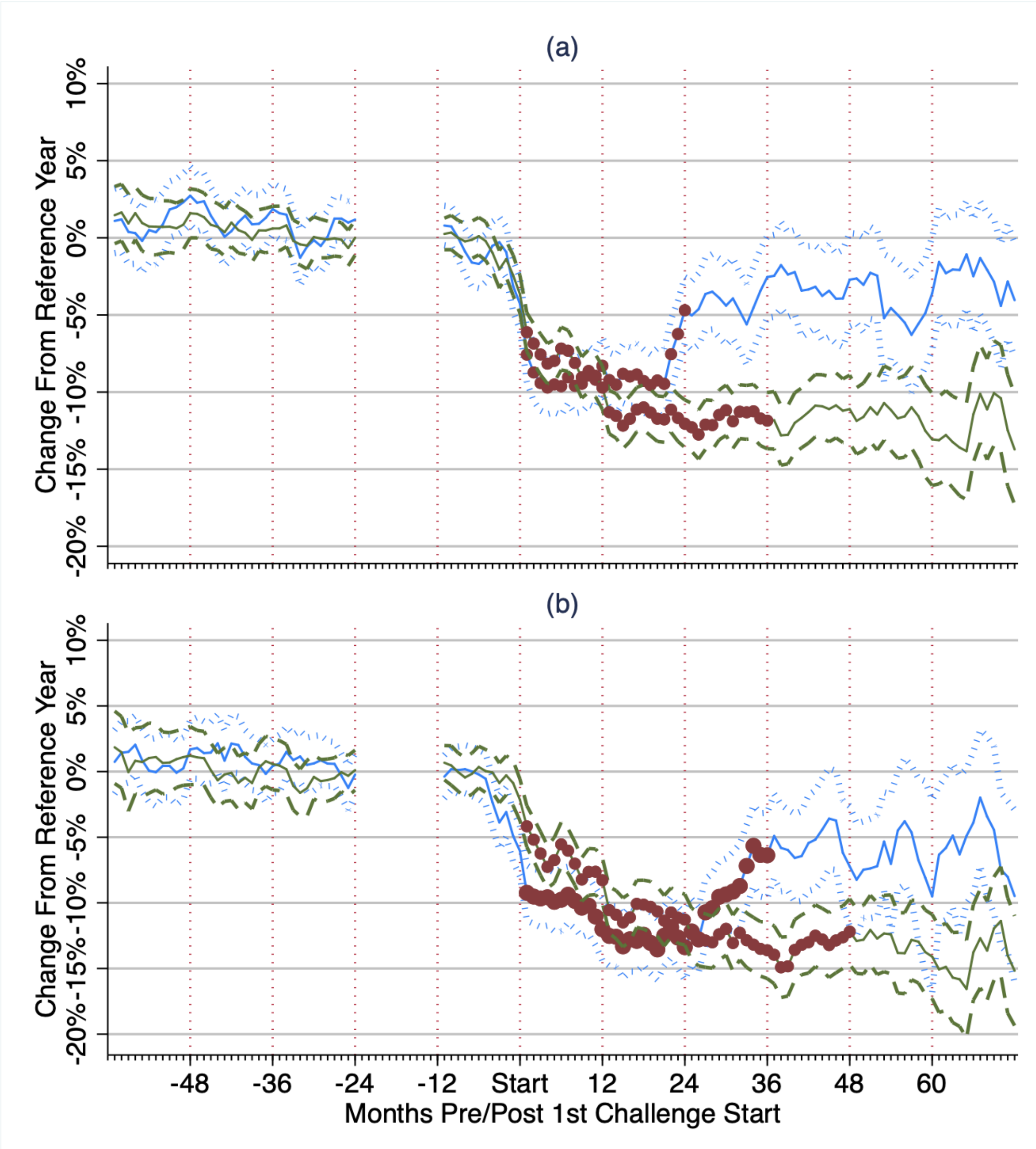
Electricity conservation may vary by season depending on how households reduce their electricity use. Improved insulation, smart thermostats, or reductions in the household temperature will produce larger energy savings in the winter and among households that heat primarily with electricity. More efficient dryers, lightbulbs, or other changes that affect primarily non-seasonal electricity use will generate energy savings year round for both household heating types. Comparing when during the year that reductions in electricity use occur, and between household heating types, sheds light on how households have responded to the conservation challenge.

I estimate the event study model (1) separately for each calendar month and household heating type, and use the year immediately before the initial conservation challenge as the reference baseline. In Figure 3, I plot the estimated program effects for each month of year during the initial challenge. Both household heating types have similar reductions in the summer months, while Electric Space Heating households have larger reductions in the winter months. To estimate the fraction of reductions due to heating, I use the reductions in electricity use over the four warmest summer months as a measure of changes

[24]To clarify the changes, Appendix Figure B.9 plots estimates from Figures 1 and 2 together in the same graph.

[25]This does not necessarily imply treatment effects were partially persistent. Idiosyncratic but persistent shocks to electricity use, combined with self-selection based on program success, can generate long-run lower electricity use among those that repeatedly re-enroll.

Figure 2: **Estimated Program Effects**: Two, Three, and Four Challenges



Notes: Estimated program effects $\hat{\beta}_\tau$ and 95% confidence intervals from equation (2) estimated for mutually exclusive groups of households. Estimates in blue are households that end their participation and estimates in green are households that re-enroll in an additional challenge. Estimation sample restricted to households that continue to subsequent challenges within 12 months. Not shown are estimates θ_g for electricity use during the gap between challenges. Estimates include individual and date fixed effects and I cluster standard errors at the household level. (a) compares households at the end of the second challenge, and (b) at the end of the third challenge.

in non-heating use. Comparing this to the reductions over the remainder of the year finds that 18% of electricity conservation among non-electric heating households is related to heating, while 52% of the conservation among electric heating households is due to changes that affect heating. That non-electric heating households have reductions due to heating is not unexpected; non-electric heating households may still use after-market baseboard heaters in addition to their non-electric primary heat source. Figure 3 also plots estimated program effects for Electric and non-Electric heating households, showing they have similar conservation trends.

To estimate the program effects over the initial challenge across household characteristics I use an event study model with annual event-time indicators,

$$y_{it} = \sum_{\varUpsilon=-4..-1,1..5} \theta_{\varUpsilon} D_{i,t,\varUpsilon} + \alpha_i + d_t + Pre_{it} + Post_{it} + \epsilon_{it} \tag{3}$$

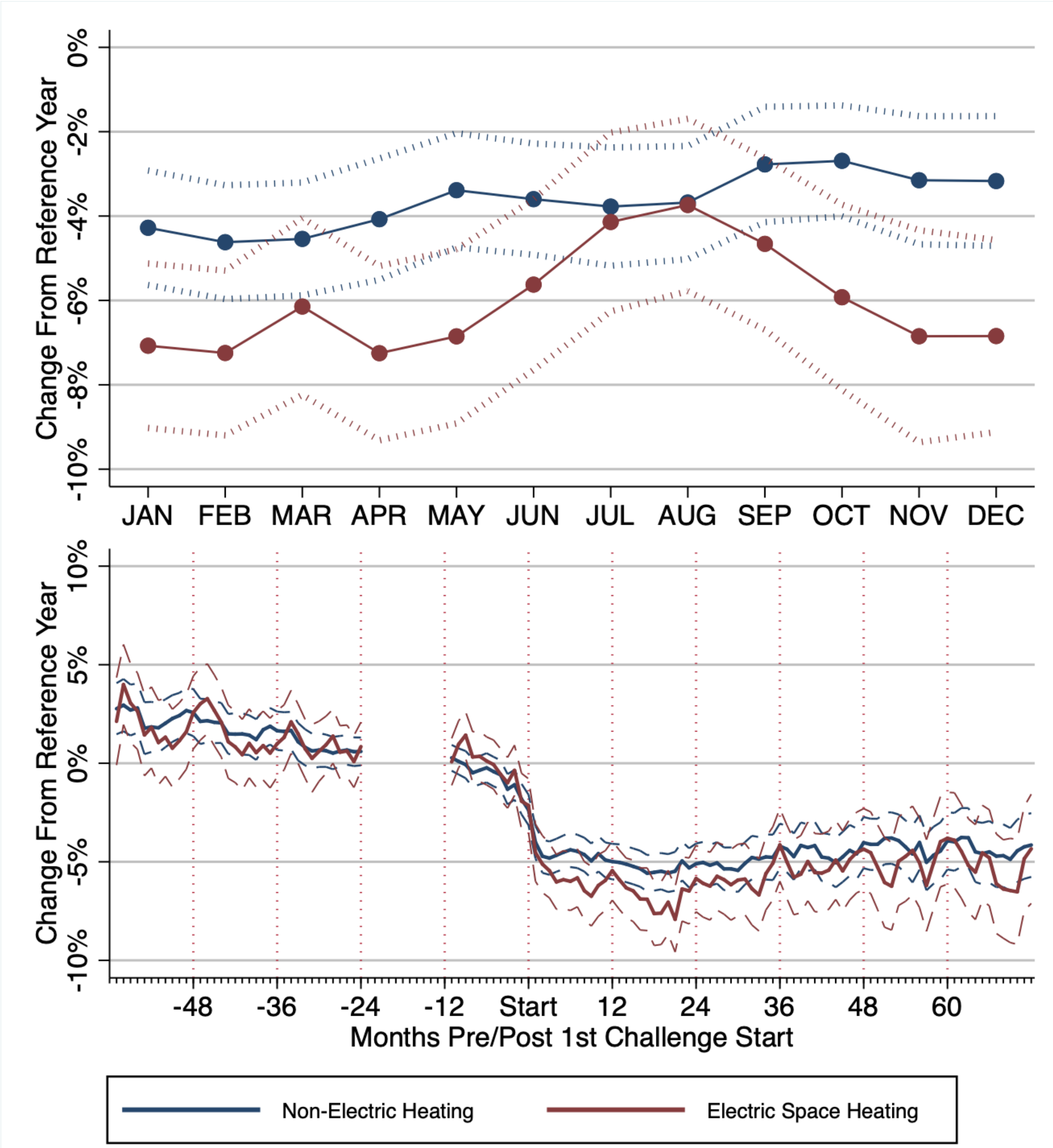
where y_{it} is log monthly electricity use for household i at monthly date t, and $D_{i,t,\varUpsilon}$ is an indicator for if household i in monthly date t is in year $\varUpsilon$ pre or post the challenge start date. α_i and d_t are household and monthly date indicators. Pre_{it} and $Post_{it}$ are indicators for households outside the ±5 year window. θ_1 is the average change during the initial conservation challenge, relative to the average during the reference year. I use the last pre-program year as the reference year, $\varUpsilon = 0$.

In Table 2 Panel A, I find that treatment effects in percentage terms are not statistically different across quartiles of pre-program use. This shows that reductions in absolute electricity use are larger for high-consumption household, but are not statistically different in percentage terms at a 5% level. In panel B, I find Apartments have larger, and statistically significant at a 5% level, reductions in electricity use compared to other dwelling types. In panel C I find no statistically significant differences, at the 5% level, in reductions by quartiles of assessed property values. If higher assessed value is taken as a proxy for income, the results imply that households respond similarly across quartiles of wealth. Panel D shows that dwellings in the top two quartiles of floor space have smaller reductions in percentage terms than smaller dwellings.

5 Re-enrollment In a Second Conservation Challenge

Section 4 documented how electricity use diverges between those who leave Team Power Smart and those that re-enroll in an additional conservation challenge. This suggests that energy use may rebound in the absence of program participation and that voluntary re-enrollment in additional conservation challenges could cause additional reductions in electricity use. This section uses the same Team Power Smart program to study two aspects

Figure 3: **Program Effects By Heating Type**



Notes: Non-Electric Heating are households expected by BC Hydro to heat primarily from sources other than electricity. *Electric Space Heating* are households whose primary source of heating is expected to be from electricity. The top panel plots estimated changes in electricity use during the initial conservation challenge relative to the same month in the year prior. Estimates are from equation (1) estimated separately for each month of the year. 95% confidence intervals shown by the dashed lines. The bottom pane plots estimates of $\hat{\beta}_\tau$ and 95% confidence intervals from specification (2) for all participant and non-participant households by heating type.

Table 2: **Program Effects by Pre-Determined Variables**

Panel A: Quartiles of Pre-Program Electricity Use				
	1st	2nd	3rd	4th
$\theta_1 : Initial\,Challenge$	-0.0612	-0.0559	-0.0497	-0.0604
	(0.00658)	(0.00533)	(0.00492)	(0.00482)
Avg. Use in 2006 (kWh)	370	648	917	1520
# of participants	1254	1253	1253	1253
# of non-participants	2220	2219	2219	2219
Panel B: Building Type				
	1 Sty SFD	2 Sty SFD	Apartment	Townhouse
$\theta_1 : Initial\,Challenge$	-0.0432	-0.0381	-0.0625	-0.0396
	(0.00306)	(0.00350)	(0.00676)	(0.00509)
# of participants	3426	2422	1282	1102
# of non-participants	3426	2422	1282	1102
Panel C: Quartiles of Assessed Value				
	1st	2nd	3rd	4th
$\theta_1 : Initial\,Challenge$	-0.0465	-0.0474	-0.0409	-0.0396
	(0.00421)	(0.00373)	(0.00381)	(0.00405)
Avg. Assessed Value ($1,000)	$288	$479	$686	$1,210
# of participants	2220	2222	2217	2218
# of non-participants	2221	2218	2219	2219
Panel D: Quartiles of Floor Area				
	1st	2nd	3rd	4th
$\theta_1 : Initial\,Challenge$	-0.0538	-0.0517	-0.0349	-0.0340
	(0.00467)	(0.00383)	(0.00362)	(0.00403)
Avg. Floor Area (sq. ft.)	984	1639	2196	3275
# of participants	2129	2133	2129	2124
# of non-participants	2145	2129	2137	2134

Notes: Estimated average change in electricity use from the year pre-program to year of the initial conservation challenge. Panel A: Quartiles of pre-program electricity use determined from households' average electricity use in the pre-program year, 2006. Quartiles are defined separately for the balanced set of participant and non-participant households. Estimates exclude households starting their initial challenge before 2009 to avoid biasing estimates with a reversion to the mean. Panel B: Building type includes the four principal housing types of single story single family dwellings, two story single family dwellings, apartments, and town homes. Panel C: quartiles of assessed value are from the 2010 BC Assessment for individual units and include both structure and land value. Panel D: quartiles of floor area are in square feet. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

of these households' extensive margin re-enrollment decisions; the decision to re-enroll itself, and causal reductions in electricity use from continuing to a second conservation challenge.

Weather affects households' electricity use. BC Hydro applies a weather-adjustment algorithm to avoid penalizing or unnecessarily rewarding households for changes in electricity use that are due to idiosyncratic changes in weather, and not their conservation effort.[26] This algorithm is applied to the changes in actual electricity use that customers are billed for; I refer to these changes, prior to their weather-adjustment, as billed changes. The weather-adjustment algorithm adjusts billed changes for year-to-year changes in heating degree days, and results in a second measure which is the electricity conservation houses receive credit for; I refer to these as credited changes. When households view their online progress towards their 10% reduction goal, or the reductions in electricity use they are credited with achieving during a challenge, they are shown the credited — not billed — changes.

Figure 4 plots the probability of re-enrolling in a second challenge against the credited changes in electricity use from a household's initial conservation challenge. This provides insights into households' decisions whether to re-enroll. Among households that fail their initial challenge, their probability of continuing to a second challenge is largely independent of their credited changes in electricity use. Figure 4 shows households with large increases in electricity use of around 20% have a similar probability of re-enrolling as households that had no change, and these households are approximately 7% less likely to re-enroll as those that nearly achieved their 10% target. A similar pattern repeats amongst households that passed their challenge; those that barely pass with reductions $\sim$10% are equally likely to continue as households that achieved reductions of $\sim$20% or more. In contrast, there is a sharp discontinuous jump in the probability of continuing to a subsequent challenge at the 9.5% threshold for success.[27] This pattern shows that in deciding whether to continue in the program, households are responsive to their success or failure in a conservation challenge but are largely insensitive to the level of reductions in electricity use they are credited with or achieve.[28]

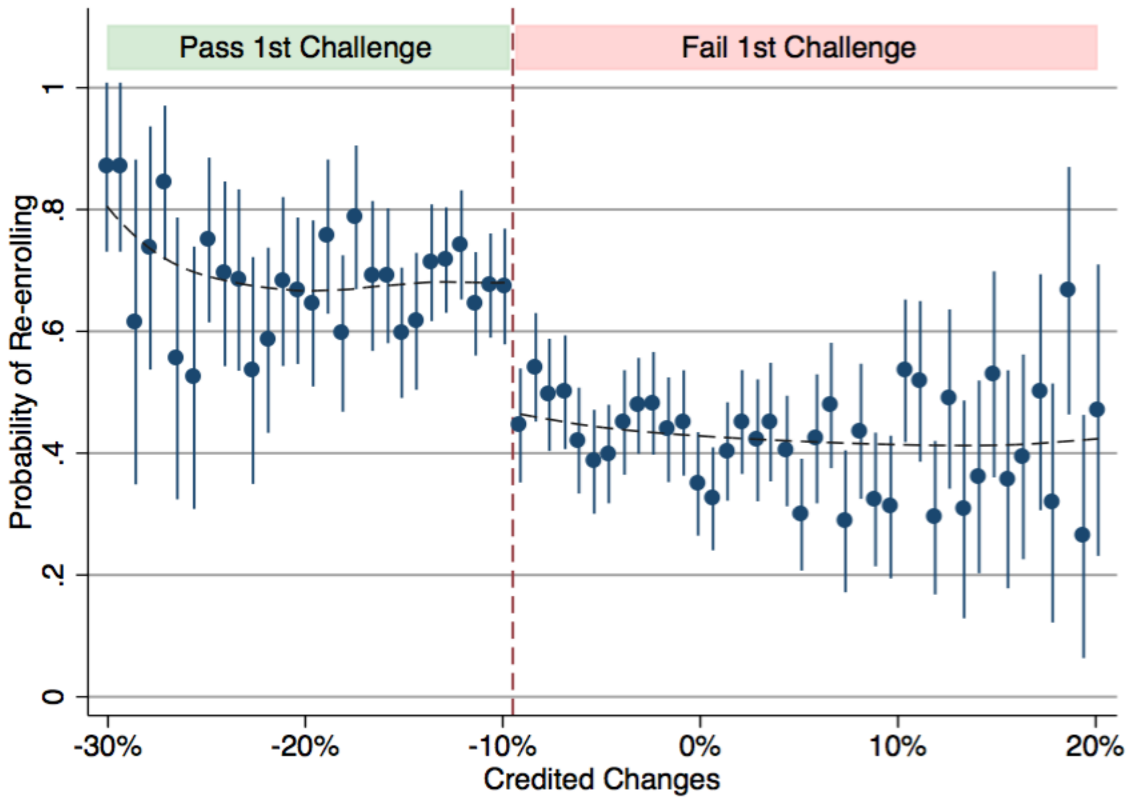
This insensitivity of re-enrollment to changes in electricity use is not consistent with the incentive structure of the program. Each conservation challenge is an additional 10% reduction compared to the year prior; this creates a greater incentive for households that had increases, or smaller reductions, in electricity use to re-enroll compared to if they had larger reductions. Households with large increases during a challenge establish a new higher baseline for their subsequent 10% conservation challenge, while those that decreased their electricity use establish a new lower baseline. Under the reasonable assumption that the marginal cost of electricity conservation is increasing, the greater the electricity conservation achieved in a challenge the more costly, in terms of effort or

[26] I discuss the algorithm in Appendix A.4.

[27] While similar discontinuities exist after the second and subsequent conservation challenges, the sample is too small for instrumental variable estimates.

[28] See Figure C.1 in the Appendix and Table 3 for the comparison to billed reductions.

Figure 4: **Probability of Re-Enrolling**



Notes: Credited changes are the annual changes in electricity consumption between the pre-program baseline year and the year of the first conservation challenge after processing by the BC Hydro weather adjustment algorithm. Credited changes are those changes shown to households during their conservation challenges and after their completion. Point estimates and 95% confidence intervals are the mean probability of re-enrolling among households within 0.75% width binds in credited changes. The dashed line is a first order local polynomial fit. The vertical dashed line indicates the 9.5% threshold. By definition, households to the left of the dashed line pass their conservation challenge while those to the right fail.

financial investment, the subsequent challenge will be.[29,30]

The magnitude of the discontinuity in Figure 4 is large compared to differences in re-

[29]This is supported by Figure C.1 in the Appendix, which shows the fraction of households that, conditional on re-enrolling, pass their second conservation challenge. Consistent with increasing marginal cost of electricity conservation described above, approximately half of households with billed increases in electricity use of 10-20% pass their second challenge; these households are twice as likely to pass as households that had reductions during the first challenge of 10% or larger. This finding also holds when using credited changes.

[30]A potential concern is that households may differ substantially depending on their credited changes in a way that affects their probability of re-enrolling, and this could offset the incentive to re-enroll caused by smaller reductions. The large weather adjustment makes such an offset unlikely to produce the observed insensitivity of re-enrollment to conservation. The weather adjustment significantly randomizes households achieving the same actual reduction in electricity use across the credited changes, increasing the comparability of households with different credited changes.

enrollment probability across household characteristics. To explore what correlates with households' re-enrollment decisions I estimate several Probit models—specification details are in Appendix C and results in Appendix Table C.1. These show that a households pre-determined characteristics have little direct, or in-direct through level of electricity conservation, correlation with the probability of re-enrolling. Taking the largest difference in point estimates finds Townhouses are 8.6% more likely to re-enroll than homes classified Other, and households with Pre-Program electricity use three standard deviations above the mean 2006 use are 5.9% more likely to re-enroll. In comparison, households that pass their conservation Challenge are 20.2% more likely to re-enroll.

5.1 Fuzzy Regression Discontinuity Empirical Strategy

I use the discontinuity in the probability of continuing to a second challenge at the 9.5% conservation threshold for success, shown in Figure 4, as the instrument for treatment in a second conservation challenge. The instrumental variable is a binary indicator for success in the initial conservation challenge. The first stage relationship is

$$C_i = \gamma_0 + \gamma_1 1\{R_i \leq \bar{R}\} + \gamma_2 R_i + \gamma_3 1\{R_i \leq \bar{R}\} \times R_i + \gamma_4 B_i + \gamma_5 X_i + \eta_i \tag{4}$$

where C_i is a binary indicator for whether a household continues to a second challenge, R_i are households' credited changes in electricity use from the first challenge, $\bar{R}$ is the threshold for success in the challenge and is -9.5%, $1\{R_i \leq \bar{R}\}$ is the dummy variable for success in the initial challenge, B_i are the billed changes from the initial challenge, and X_i is a vector of other controls. In my main specification I control for a linear trend in credited reductions and allow this trend to have different slopes on either side of the discontinuity. The instrument excluded from the second stage is $1\{R_i \leq \bar{R}\}$.

The second-stage relationship is

$$y_i = \beta_0 + \beta_1 C_i + \beta_2 R_i + \beta_3 1\{R_i \leq \bar{R}\} \times R_i + \beta_4 B_i + \beta_5 X_i + \epsilon_i \tag{5}$$

where y_i is the post-challenge percent change in electricity use. y_i is defined

$$y_i \equiv \frac{(u_{i,\tau=2} - u_{i,\tau=1})}{u_{i,\tau=0}} \tag{6}$$

where $u_{i,\tau}$ is household i's aggregate electricity use during the year indexed by event-time τ. For households that do not undertake a second challenge $u_{i,\tau=2}$ is the total electricity use in the 12 months immediately following the completion of their initial challenge, $u_{i,\tau=1}$ is the total electricity use during their initial challenge, and $u_{i,\tau=0}$ is the use during the pre-challenge year. For households that immediately undertake a second conservation challenge with no gap between challenges $u_{i,\tau=2}$ is the total electricity use during the second challenge and $u_{i,\tau=1}$ and $u_{i,\tau=0}$ are as before. For households that wait before beginning a second conservation challenge I define $u_{i,\tau=2}$ as the 12 months of electricity use during their second challenge and $u_{i,\tau=1}$ as the 12 months of electricity use immediately preceding that second challenge. This makes y_i a consistent measure of the reductions in electricity use a household is trying to achieve in its second challenge regardless of whether that household waited before undertaking a challenge or began it immediately. I center the billed and credited changes at the 9.5% threshold. β_0 is the post-challenge change in billed electricity use at this threshold for households that do not continue in the program. β_1 is the additional effect on post-challenge billed changes in electricity use relative to households that left the program.

5.2 Fuzzy Regression Discontinuity Identifying Assumptions

A regression discontinuity strategy requires that units on one side of the threshold that defines treatment are a suitable counterfactual for units on the other.(Lee and Lemieux, 2010) This assumption would be violated if households can precisely manipulate their assignment into treatment, and such manipulation results in households sorting at the discontinuity in any way that affects potential outcomes. Such sorting is a concern in this setup as households are explicitly trying to achieve a 10% conservation target. Sorting could occur if, for example, households are heterogenous in their attention to their progress and high-information type households exert additional effort in the last months of a conservation challenge and self-select into passing their challenge.

Fortunately, the separation of the 9.5% threshold for Challenge success from the 10% reduction target allows me to show that this RD strategy remains valid despite households manipulating the running variable of electricity conservation such that they exhibit bunching below the 10% reduction target. Sorting discontinuously at the 9.5% threshold for success is unlikely for several reasons. Most importantly, households were not aware that their success or failure would be evaluated against a 9.5% threshold instead of the advertised 10% target. This does not remove the potential problem of sorting; households sorting around the 10% target in a way that changed potential outcomes in the absence of treatment would invalidate the causal interpretation of regression discontinuity estimates at the 9.5% threshold.[31] In addition, the weather adjustment mechanically randomizes households near the threshold into and out of treatment based on the weather change that occurred. The cumulative nature of the challenge also makes precise manipulation of success difficult. A household at a 9% cumulative reduction entering the last month of their challenge would have to double their previous monthly reductions and reduce their use in the last month by 21% to achieve their 10% target.

Evidence on whether there is sorting on observables can be gained by testing the continuity of covariates and pre-determined variables across the 9.5% threshold and 10% target. I find no statistically significant change which supports that households are not sorting at

[31] In theory, a sufficiently large number of observations would allow estimation limited to bandwidths of $\pm 0.5\%$ around the 9.5% threshold, thus avoiding the problem of sorting around the 10% threshold but changing the subset of households that the LATE estimates are estimated for.

the 9.5% discontinuity or 10% threshold in a manner correlated with observables.[32] This is supported by a density test of McCrary (2008), which fails to reject the null hypothesis (one sided p-value 0.117) of no discontinuity in the density of the running variable at the 9.5% threshold.[33]

However, a McCrary (2008) density test rejects that there is no discontinuity in the density of electricity conservation at the 10% threshold—suggesting that households can manipulate their assignment around the 10% target.[34] Such manipulation only invalidates the RD strategy if causes discontinuous potential outcomes. While the continuity of covariates shown above suggests sorting on observables is not occurring, BC Hydro's separation of the 10% target from the 9.5% threshold allows the continuity of potential outcomes at the 10% target to be directly tested. If households were sorting around the 10% target in a manner that affected potential outcomes, their decision to re-enroll in a second challenge or their post-program electricity use would be expected to be discontinuous. This does not occur—see C.1 for details. As a result, the discontinuity in Figure 4 is due to households succeeding in their conservation challenge and is not due to manipulation of their electricity conservation that results in sorting around the 9.5% threshold.

The exclusion restriction requires that the instrument only affect the outcome, y_i, through the decision to continue to a second challenge (Angrist and Pischke, 2008). Conditional on credited and billed reductions, success in a challenge can have no direct effect on post-program changes in electricity use and therefore can be excluded from the second stage. This assumption could be violated if households receive a warm-glow effect from succeeding that affects their subsequent effort at reductions independent of continuing, or if the $75 rebate causes an income effect and alters post-program conservation. As $75 is small relative to household's incomes I assume there is no income effect that influences electricity use. Any warm-glow effect on subsequent conservation effort is likely to be short lived compared to the effect of the financial incentive, which remains throughout the twelve months of the challenge. In addition, if a warm glow effect was substantial it is likely to be particularly strong during the initial months of the next challenge while a household's success is still fresh in their minds. The event study estimates of Figure 1(b) indicate that additional program effects during the second challenge are consistent throughout the twelve months of challenge, suggesting that there is little warm-glow effect.

5.3 Fuzzy Regression Discontinuity Estimates

This section presents the fuzzy-RD estimates of the treatment effect of a second conservation challenge. Across a wide variety of specifications and robustness checks I find a

[32] See Appendix Table C.1 and Appendix Figure C.1.

[33] See Appendix Figure C.3.

[34] Appendix Figure C.2 shows a histogram of credited changes during households' first conservation challenges. The mass of observations just below the 10% threshold suggests that households may be bunching around the 10% target. This is tested in Appendix Figure C.2 which rejects the null of no discontinuity with a one sided p-value of 0.0017.

consistent pattern where re-enrolling in a second conservation challenge causes a large additional reduction in electricity use. These results are consistent with the event-study results and support that additional conservation challenges cause additional reductions in electricity use, and that electricity use rebounds as households leave the program.

Table 3 presents my preferred specification. Columns (2) through (6) show results estimated for different bandwidths from ±7% to ±3% around the threshold of a -9.5% change in credited electricity use.[35,36] Panel (A) shows the first-stage results for the probability of continuing to a second challenge, estimated from equation (4). For my preferred bandwidth of 5% I find that, conditional on failing the challenge, 53% of households continue to a second conservation challenge. At the 9.5% threshold for success, households that just succeed in their initial conservation challenge are 14.5% more likely to continue to an additional challenge than those which just failed. This pattern repeats across various estimation window widths. Approximately 50% of households re-enroll in a second conservation challenge if they fail their initial challenge, while success in a challenge causes an additional 14% to 20% of households to re-enroll.[37] Across bandwidths from ±7% to ±3% I find the F-statistic on the instrument decreases from 22 to 6.5. This indicates that the first-stage is reasonably strong for larger bandwidths (larger sample size), but the small sample size limits the strength of the 1st stage as the bandwidth narrows.

Table 3 panel (B) reports the OLS and second-stage instrumental variable estimates of equation (5). Specification (1) is the OLS result for all households. (1) shows that re-enrolling in an additional challenge is associated with a 1.6% decline in post-challenge electricity use, relative to households that do not re-enroll. Specifications (2) through (6) show the IV estimates for different estimation bandwidths. These estimates find a consistent pattern where, for households that comply with the instrument, continuing to a second conservation challenge causes a reduction in electricity use. For my preferred bandwidth of 5%, I estimate that continuing to a second conservation challenge causes a 23% reduction in electricity use ($\hat{\beta}_1 = -0.231$). This is a large effect. By definition, the treatment effect is the change in electricity use for complier households relative to what they would have had, had they not re-enrolled. An instrumental variables estimation strategy cannot identify the level of electricity use, in the absence of continuing, for these compliers. As a result, the treatment effect does not separately identify a potential rebound in electricity use among those that leave the program from additional reductions in electricity use beyond those achieved in the first conservation challenge.[38] Given the average reduction of 9.1% during the first challenge for households within the ±5% bandwidth, and the target of an additional 10% conservation, the estimated treatment effect

[35] I present estimates using a range of bandwidths and a uniform weighting instead of kernel estimates. See Imbens and Lemieux (2007) for a discussion on the practical similarities of varying the bandwidth to using different kernels. Results are robust across different bandwidths when using a triangular kernel.

[36] Plots of First Stage and Reduced Form with 7% bandwidths are presented in Appendix Figure C.4. 5% and 9% bandwidths are presented in Appendix Figure C.5.

[37] A weak instruments test by Moreira (2003) rejects (p-value 0.013, 5% bandwidth) that the binary indicator for Success (γ_1) is a weak instrument.

[38] $\hat{\beta}_0$ is not the average electricity use, conditional on other covariates, for complier households in the absence of a conservation challenge.

is likely comprised of both a significant rebound in electricity use among complier households that end participation and a large additional reduction among complier households that re-enroll in another challenge.[39]

In Appendix D I undertake a variety of robustness checks. These include: 1st and 2nd order bias-corrected estimates and optimal bandwidths selected using the variance-bias tradeoff method of Calonico et al. (2014), restricting the sample to households experiencing small weather shocks, using additional covariates, using an alternate challenge gap length of 6 months, and an alternative specification using the log of monthly electricity use. While some estimates lose significance, especially for small bandwidths, in all cases the point estimates stay a consistent sign and large magnitude. This suggests that while the magnitude of point estimates varies, the causal effect of an additional conservation challenge is a large additional reduction in electricity use for those households whose decision to re-enroll in Team Power Smart is affected by their success or failure in their prior conservation challenge.

6 Cost Effectiveness

The Team Power Smart program is designed to produce electricity generation capacity savings and reduce the expected future increase in demand for electricity. A full welfare analysis of the TPS program is beyond the scope of this work. Instead, I provide a lower bound on the cost of avoided electricity generation caused by the program. I estimate a lower bound for two reasons. First, because the costs of administering and advertising the Team Power Smart reward program are confidential to BC Hydro, I consider only the cost of the $75 rebates rewarded to households, and leave aside the costs of administering the program.[40] Second, I make the assumption that the estimated electricity conservation from the event-study model, Figure 1, are treatment effects; any overestimate of the true treatment effect will bias upward the cost of avoided generation. This is a reasonable assumption given the findings of Sections 4 and 5. From the estimated electricity conservation and the average electricity use among participants, Table A.1, I

[39] In comparing the OLS and fuzzy-RD estimates in Table 3 panel (B), it is important to note that the OLS estimates are for all households within the estimation window while the fuzzy-regression discontinuity design is a LATE for compliers. As a result, the difference in estimates may be partially due to compliers being responsive to the incentive as estimated, with always-taker's and never-taker's electricity conservation remaining largely unresponsive to the financial reward incentive.

[40] Because the program is administered online, variable costs excluding the rebate are likely to be negligible. Program fixed costs may not be insignificant relative to the cost of the rewarded rebates. One full time equivalent employee compensated at $70,000 per year for managing the program would add approximately $6 per challenge to the program. This is $20 per awarded rebate using the 30% success rate over the initial five challenges households undertake.

Table 3: **Fuzzy Regression Discontinuity Estimates of a Second Challenge**

	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A – First Stage					
Dependent variable: Continue to a Second Challenge C_i						
Window		±7 %	±6 %	±5 %	±4 %	±3 %
γ_1: Success Ind.		0.202	0.190	0.145	0.137	0.173
		(0.0432)	(0.0468)	(0.0516)	(0.0573)	(0.0677)
γ_2: Cred. Reduc.		-0.532	-1.391	-2.937	-1.290	2.090
		(0.778)	(0.971)	(1.244)	(1.760)	(2.765)
γ_3 : Success×		1.166	2.491	2.565	-0.778	-3.973
Cred. Reduc.		(1.104)	(1.383)	(1.809)	(2.447)	(3.906)
γ_4 : Billed Reduc.		-0.300	-0.368	-0.0474	-0.282	-0.511
		(0.324)	(0.344)	(0.366)	(0.404)	(0.491)
γ_0 : Constant		0.487	0.508	0.530	0.510	0.475
		(0.0303)	(0.0330)	(0.0364)	(0.0409)	(0.0479)
F-statistic		21.95	16.41	7.882	5.668	6.485
	Panel B – Second Stage					
Dependent variable: Percent change in post-challenge electricity use						
	OLS	Instrumental Variable Estimates				
Window		±7 %	±6 %	±5 %	±4 %	±3 %
β_1 : Re-Enroll	-0.0160	-0.125	-0.178	-0.231	-0.323	-0.183
	(0.00422)	(0.0605)	(0.0738)	(0.116)	(0.164)	(0.111)
β_2: Cred. Reduc.		-0.412	-0.643	-1.185	-1.108	0.785
		(0.241)	(0.354)	(0.654)	(0.828)	(0.654)
β_3 : Success×		0.303	0.375	0.867	-0.229	-1.732
Cred. Reduc.		(0.310)	(0.426)	(0.624)	(0.985)	(1.229)
β_4 : Billed Reduc.		-0.0461	-0.113	-0.101	-0.216	-0.222
		(0.0917)	(0.103)	(0.121)	(0.168)	(0.161)
β_0 : Constant	-0.00773	0.0712	0.103	0.138	0.184	0.0944
	(0.00289)	(0.0356)	(0.0447)	(0.0698)	(0.0950)	(0.0625)
N	5432	2050	1763	1475	1196	888

Notes: This table reports fuzzy-RD estimates corresponding to equations (4) and (5). Estimation sample is restricted to households that either start their next challenge within 12 months or do not undertake an additional challenge. Estimation window is restricted to ± the listed percent around the 9.5% threshold in credited changes. Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

find that the average aggregate reduction in electricity use over the first six years after an initial challenge is 2.7 MWh per household. This is the average across all households and accounts for their decisions whether to re-enroll after each challenge. Taking into account households' average success in their conservation challenges, and the number continuing to additional challenges, the average aggregate rebate payment over the six years is \$53 per household. This finds an average cost of avoided generation of \$20/MWh. In comparison, participants paid an average retail price of \$96/MWh in 2016, while a large hydroelectric dam under construction in the province is estimated to have a levelized cost of electricity of \$34-\$83/MWh (British Columbia Utilities Commission, 2017). This makes the Team Power Smart program a cost-effective way to reduce the demand for electricity in comparison to the cost of new generation.

What is the cost of avoided carbon emissions due to this energy conservation program? It is important to note that this energy conservation program was not designed to principally reduce carbon emissions and the cost of avoided emissions is not particularly relevant to BC Hydro. British Columbia generates over 90% of its electricity from hydroelectric dams and has a low emissions intensity of 9kgCO2e/MWh (BCH, 2016); \$20 per avoided MWh is a cost of avoided greenhouse gas emissions of \$2,222/tCO2eq. However, BC Hydro engages in large cross-border trade in electricity with the United States, primarily California. Lower electricity use in British Columbia allows BC Hydro to sell relatively low cost and low emissions power to California. Assuming all reductions in B.C. electricity use reduces generation in California finds, using the 2017 California average emissions intensity (EPA, 2017), a cost of emissions abatement of \$71/tCO2. At the 2017 U.S. average emission intensity, this falls to \$45/tCO2 (Schivley et al., 2018). These abatement costs are within the range of commonly discussed estimates of the SCC (EPA, 2016), and indicate that in some jurisdictions, repeated financial reward programs similar to the one studied in this work may be cost effective relative to the SCC. The results of this paper show that the continued incentive of repeated financial rewards is important for maintaining and causing additional reductions in electricity use. The continued incentive improves the program's cost-effectiveness, compared to a program offering a single annual conservation challenge. This improved cost-effectiveness occurs for two reasons. First, the program administration fixed costs are spread across additional conservation challenges. Second, the repeated incentive causes additional reductions, and keeps electricity use from rebounding back close to pre-program levels.

7 Conclusion

This paper investigates how households respond to repeated financial rewards offered in an electricity conservation program. Using a panel of monthly electricity use, I track households' decisions whether to re-enroll in the program along with changes in their electricity use. I use two separate empirical strategies to estimate changes in electricity use. These two strategies complement each other and allow me to address the potential for self-selection into the program, and into successive annual conservation challenges, which could bias estimated treatment effects. The ten years of the panel allow me to estimate the long-run persistence of electricity conservation as households leave the program, as well as the long-run changes among households that repeatedly re-enroll in the program. By comparing the electricity conservation across households I provide insights into what changes within the home households made to conserve electricity, and what information households use in their decision whether to re-enroll.

Two findings distinguish this work. First, this paper undertakes one of the longest studies of the persistence of electricity conservation. I do not find that financial rewards cause persistent changes; instead, electricity use rebounds close to pre-program levels as households leave the program, and continues to decline among households that re-enroll. This shows that the ongoing incentive of successive financial rewards is necessary in this setting to cause long-run lower electricity use. A potential implication of this is that programs in fields outside energy conservation, such as education, that aim to cause persistent effects should consider incentivizing specific changes rather than reward a general outcome. Second, the repeated financial rewards provide an opportunity to observe both extensive and intensive margin decisions. I find that in deciding whether to re-enroll, households are responsive to their success or failure in achieving their conservation target, and yet, conditional on this success or failure, are notably unresponsive to their degree of electricity conservation. This suggests that households use heuristics in making participation decisions, rather than accounting for the incentive structure of the program or their degree of conservation effort.

This paper's findings imply it is important for repeated incentive programs to explicitly consider self-selection into and out of re-enrollment. This is especially important when treatment effects fully dissipate after the incentive ends. Programs may be able to improve their cost effectiveness by encouraging re-enrollment, particularly when consumers fail to achieve their goal and self-select out of the program as a result. One way to address this could be to offer tiered incentives, such as offering an alternative challenge only to consumers that missed their target and are likely to self-select out of the program. Such an alternative incentive could be either transparently offered at the outset of participation, or revealed only to consumers that have not re-enrolled after some time period to target those likely to be ending participation. The responsiveness of households to their success or failure also opens up the possibility of modifying the reward structure to exploit this sensitivity; for example, by using unexpected consolation prizes instead of an all-or-nothing reward.

The use of information is central to how we model the decisions of agents across many questions within economics, from how tax rates affect labour market responses to how news affects voting behaviour. This paper showed how households use a heuristic in their extensive margin participation decisions, and the use of heuristics may be ubiquitous in intensive margin responses such as to information on price changes. Heuristics could result from rational inattention and allow households to avoid the cognitive and time costs of considering complex incentives. Alternatively, they could result from households responding to incentives and information based on models of behaviour beyond standard neoclassical models of decisions. Disentangling the ways in which consumers use information is an important avenue for future research. It is particularly important to the design of complex price schedules that are increasingly facilitated by the spread of smart meters, distributed electricity generation, and storage.

A further direction for inquiry are the consequences of repeating interventions. This paper showed how interventions that do not cause persistent effects can still produce cost-effective long-run changes by being repeatedly offered. These findings also suggest that consumers respond to each discrete reward in isolation, and do not consider the dynamic consequences of their effort on the future incentive structure. This raises the questions of whether this is a feature of the annual time scales of this program, or is a common feature of consumer responses to discrete rewards, and how consumers would respond as the time between potential rewards was reduced towards the limit of a continuous incentive.

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