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

Alastair Fraser*

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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 annual energy conservation challenges offered by a large electrical utility company. I find households' electricity use reduces over each year they participate yet rebounds when they stop participating. I also find that households' decisions whether to re-enroll in the program and attempt a subsequent goal are highly sensitive to their success or failure in achieving their energy conservation goal, but not to the financial incentive to continue participating or their level of past effort. This suggests that households are either responding to the emotional and normative aspects of success and failure, or are substantially inattentive to information that is provided directly to them.

Keywords: Goal-setting, Energy Conservation, Electricity, Financial Rewards
JEL Codes: DO4, Q40, Q50, D12, Q48

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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 technologies like smart meters and complement the growing deployment of renewable energy. Energy conservation and DSM policies fall into 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 behavioral 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] Third, programs that modify how energy is used by mandating or subsidizing changes in technologies.[4] Policies where participants can repeatedly attempt goals are widespread outside of energy conservation and contain several of these features: 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 behavioral nudges based on messages of success or failure. Compared to setting an individual goal and exerting effort towards it, which has been widely studied, relatively little is known about repeated goals and how consumers respond to success and failure.

In this paper, I study a long-running energy conservation program called Team Power Smart that repeatedly offered households financial rewards for achieving energy conservation goals. Team Power Smart offered households in the Canadian province of British Columbia the opportunity to attempt annual electricity conservation 'challenges' in which \$75 financial rewards — equivalent to 11% of the average annual electricity bill — would be provided 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 notified of their degree of energy conservation—their degree of success—directly alongside whether they passed or failed their conservation goal. All households, whether successful or not, were then given the same opportunity to attempt an identical goal of another 10% annual electricity conservation for another \$75 reward. This process repeated each year and I observe households' participation and energy use decisions over ten years and as they attempted up to nine separate goals.

This paper has two principle findings that are important to a variety of energy conservation and goal-setting programs. First, I find that in deciding whether to continue participating, households are highly responsive to their success or failure in achieving a previous goal but not to their degree of success, which affects their financial incentive to re-enroll. Second, I estimate the decline in energy use that

[1] See for example Delmas, Fischlein, and Asensio (2013), Kahn and Wolak (2013), Gans, Alberini, and Longo (2013), Sallee (2014), Delmas and Lessem (2014), Jessoe and Rapson (2014), Sexton (2015), Davis and Metcalf (2016), Schleich, Faure, and Klobasa (2017), Wichman (2017), Martin and Rivers (2018), and Stojanovski et al. (2020).

[2] Particularly relevant policies include Abrahamse et al. (2007), Allcott (2011b), Allcott and Rogers (2014), Allcott and Kessler (2019), Delmas and Lessem (2014), Dolan and Metcalfe (2015), Ito (2015), and Gillingham and Tsvetanov (2018).

[3] Wolak (2006), Borenstein (2009), Wolak (2011), Ito (2014), Ito (2015), Byrne, Nauze, and Martin (2018), and Shaffer (2020).

[4] Davis, Fuchs, and Gertler (2012), Boomhower and Davis (2014), Datta and Gulati (2014), and Fowlie, Greenstone, and Wolfram (2015).

occurs when households repeatedly re-enroll and the partial rebound in energy use as households end their participation, and estimate that re-enrolling causes large additional reductions in use.

That success or failure to achieve a goal affects subsequent decisions has been documented in several settings. These include hotel loyalty (Wang et al., 2016) and frequent flyer (Drèze and Nunes, 2011) programs, and the Math Olympiad where the decision to continue competing in response to success differs across genders (Buser and Yuan, 2019). However, understanding the underlying mechanisms is frequently complicated by the fact that those who succeed are typically different from those who fail, and succeeding typically creates different subsequent incentives compared to failing.

The Team Power Smart design studied here avoids both of these challenges. I take advantage of a natural experiment where households are as good as randomly assigned success or failure in achieving their energy conservation goal. I find that success increases the probability that a household re-enrolls by 27%. Conditional on success, their degree of energy conservation—which affects the difficulty of achieving the next goal—is uncorrelated with their decision to re-enroll. Importantly, the program design also provided the same incentive to re-enroll as well as provided the same information to both those who failed and succeeded; this substantially narrows the set of mechanisms that can cause such a large responsiveness to success and failure.

A largely experimental literature finds that emotional responses can affect decisions. This includes negative emotional reactions cause participants to avoid even options they expect will perform better (Ratner and Herbst, 2005), psychological responses to winning can induce additional effort, (Descamps, Ke, and Page, 2018), and an aversion to future regret and disappointment influences current decisions (Gill and Prowse, 2012). Previous work has also found people overweight positive relative to negative information about characteristics of importance to them, which has been called the good news-bad news effect (Eil and Rao, 2011) or asymmetric updating (Mobius, Niederle, and Niehaus, 2014). Similarly, people update their beliefs more in response to information that was better than that which was expected (Sharot, Korn, and Dolan, 2011) or confirms rather than refutes their priors (Rabin and Schrag, 1999). People also attribute past success to their ability and a lack of success to factors outside their control (Silver, 1995). The strong responsiveness to success vs failure suggested by this Team Power Smart program and the above literature could be important in a wide variety of settings as many, intentionally or not, have elements of achieving goals. For example, households may make energy consumption decisions based on prior 'success' in keeping their energy bill below a previous bill, or below an imagined target amount, rather than respond directly to the price, and in settings where customers can choose between electricity rates and providers their choices may be driven as much by emotional reactions to bill shock as to the price schedules offered.

An alternative mechanism for households participation decisions being strongly dependent on past success or failure, rather than on the future incentive offered, is that they may be inattentive to information (Gabaix, 2019). Because each energy conservation goal is an additional 10% compared to the previous year, a household's prior degree of success affects the incentive to re-enroll. Despite this, I find households largely take into account only their success or failure, and not their degree of success. This suggests households may use simple heuristics in making decisions, such as using the binary of success vs failure while being inattentive to the detailed information provided. Such inattention is consistent with previous findings. Within energy use, evidence suggesting inattention is that consumers temporarily use less electricity after receiving a bill (Gilbert and Graff Zivin, 2014) and use more after enrolling in automatic bill payments (Sexton, 2015). Consumers do not pay close attention to appliance energy efficiency labels (Sallee, 2014; Davis and Metcalf, 2016; Houde, 2018; Allcott and Taubinsky, 2015) or residential energy efficiency (Palmer and Walls, 2015), and may not be attentive to marginal electricity prices (Ito, 2014). Similar evidence of inattention comes from the closely related setting of water consumption where both billing frequency and perceived prices affect consumption (Wichman, 2014; Wichman, 2017).

While self-selection is inherent in decisions on repeating goals, it also creates concerns that energy conservation is credited to Team Power Smart and rewards are paid to participants for changes in energy use that would have happened even in the policy's absence. As Boomhower and Davis (2014) show, this non-additional conservation can be large in energy conservation programs but difficult to estimate. I address the identification challenge created by self-selection in Team Power Smart by employing both event study and fuzzy regression-discontinuity empirical strategies. Using an event study model I estimate the short-run reductions and long-run persistence of electricity use changes. I find that an initial conservation goal is associated with an immediate 4.3% 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 partially rebounds as households end their participation in the program. This rebound indicates that households tend to make short-run adjustments in addition to permanent investments or creating persistent habits, and that the ongoing incentive of additional goals is important for long-run lower electricity use. An important caveat is that as participation is voluntary, the event-study estimates are an upper bound on the causal treatment effect. To estimate the causal effect of a second conservation challenge I use a fuzzy-regression discontinuity design and exploit the discontinuity in re-enrolling created by success and failure. I find that attempting an additional conservation goal causes large additional reductions in energy use. Consistent with the descriptive event-study estimates, this supports the conclusion that the continued incentive of the goal and financial reward is important in causing long-run lower electricity use.

This rebound in energy use contrasts with the findings of Ito (2015) and Harding and Hsiaw (2014) who both study energy conservation goal-setting programs with features in common to this setting.[5,6] Ito (2015) investigates a mandatory program offering customers an additional 20% discount on their bill if they achieved a 20% reduction over 4 months, and Harding and Hsiaw (2014) studies a voluntary

[5] Several other papers have also considered financial rewards and energy conservation though with less overlap to the setting studied here. Dolan and Metcalfe (2015) undertake a randomized controlled trial (RCT) and find large financial rewards cause a large and persistent conservation over the two months of their treatment and two months post-treatment that 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, Rokkanen, and Rothe (2015) study a 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] A primarily psychology literature 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 Houwelingen and Raaij (1989).

program where customers voluntarily choose energy conservation goals without a financial reward. Ito (2015) does not find any long-run rebound over subsequent years, but does find results are driven by higher air conditioner use in warmer areas, whereas I find energy conservation occurs in both seasonal heating and base load electricity use. Harding and Hsiaw (2014) finds some evidence of an immediate rebound along with persistent reductions over the following 18 months for customers that choose realistic conservation goals. These differing results 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, or on the program design and context in which it is offered—for example a mandatory program during an electricity crisis as in Ito (2015), or a repeated voluntary program as part of routine electricity use as in this paper. 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 rebound and large effect of success on the decision to continue suggests an important trade-off in repeated goal-setting programs; more difficult goals can induce more effort (Gutt, Rechenberg, and Kundisch, 2020), but at the cost of higher failure rates and lower subsequent participation.

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 results. Section 5 explores households' decisions whether to leave the program and presents the fuzzy-regression discontinuity results. Section 6 concludes.

2 Institutional Setting, Program Design, and Data

BC Hydro is Canada's second largest integrated electrical utility company. It serves 1.7 million residential customers covering 95% of the population in British Columbia (BCH, 2014a). As part of government mandated improvements to energy efficiency, BC Hydro launched a voluntary program — Team Power Smart — summed up by the promotion: "*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]

Enrolling in the program requires only the minimal time cost of registering online. The program consists of energy conservation challenges which require households to reduce their aggregate electricity use over a 12-month period by 10%. Because it is the aggregate annual conservation that matters for success, households can miss their 10% goal in any given month and still pass their challenge. 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 baseline. All participants can view their progress towards their conservation goal 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 their electricity

[7]BC Hydro Team Power Smart website landing page, accessed June 2017.

use and monthly and cumulative progress towards their annual 10% conservation goal.[8]

Each household's annual 10% conservation goal is measured relative to their own annual electricity use over the preceding 12 months. To avoid unduly penalizing or rewarding households for changes in weather instead of their energy conservation effort, BC Hydro applies a weather adjustment algorithm. This algorithm adjusts the previous year's electricity use (from which the 10% reduction goal is measured) for annual changes in heating degree days.[9] As a result, the percent reduction in electricity use that is displayed to households — and that they receive credit for — differs from the actual percent change in kilowatt hours (kWh) they use and are billed for. I refer to changes in actual electricity use, prior to their weather-adjustment, as 'billed changes;' all event-study and fuzzy-RD estimates are of billed changes. I refer to the changes in electricity use that are displayed to households as 'credited changes.' Households would have had to calculate their own annual percent change in electricity use to notice that their credited percent reduction differed from their reduction in billed electricity use. I discuss the weather adjustment and differences between billed and credited changes in detail in Appendix A.4.

Upon completing the 12 months of the challenge 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 goal advertised to customers is 10%, BC Hydro evaluates final success or failure against a 9.5% conservation threshold. Households that reduced their credited electricity use by greater than or equal to 9.5% pass their challenge while the rest fail. As I discuss further in Section 5, the way BC Hydro notifies households is important: everyone was required to "*[l]og in to MyHydro to get your results. If you've reached your goal, choose how to receive your reward: a mailed cheque or a credit on a future bill.*"[10] As a result, both successful and unsuccessful households must log in to the online portal to learn whether they succeeded and they receive the same information on their degree of electricity conservation alongside their status of success or failure.

2.1 Structure of Additional Conservation Challenges

A novel feature of Team Power Smart is that households that both 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 can decide to undertake another 10% conservation goal based on their previous 12 months of (weather adjusted) electricity use. The new reduction goal is independent of whether the prior 12 months contained a challenge or not, and independent of whether the prior challenge ended in 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

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

[9] A heating degree day (HDD) is a common measure of the energy required to heat a home. HDDs are defined by BC Hydro as the sum over days of the number of degrees over a baseline temperature of 18°C.

[10] Participation in the program by the author and conversations with BC Hydro.

months of the gap prior to starting their next challenge. Because each additional 10% conservation challenge 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.

2.2 Data and Household Characteristics

Under a non-disclosure agreement with BC Hydro I obtained an anonymized sample of monthly electricity billing records and program participation histories from January 2006 to December 2015 for 10,000 Team Power Smart program participants and 20,000 non-participants. By comparison, 19,905 households, or 1.2% of all households served by BC Hydro, participated in an energy conservation challenge in 2012 alone (BCH, 2014b). I combined these records with detailed individual building characteristics from the property assessment corporation, BC Assessment.[11] The samples of participants and non-participants were randomly selected from the population of program participants and non-participants in the Greater Vancouver Area (an urban and suburban population of 2.4 million) who had not moved over the 10 years of the panel. As a result, both participant and non-participant samples represent more stable households than average in the population.[12] Comparing the non-participants in the sample to all residential units from the Greater Vancouver Area indicates that these relatively stable households are, unsurprisingly, more likely to live in single family dwellings and less likely to live in apartments or townhouses (Table 1). Comparing similarly stable participants and non-participants indicates that participants are more likely to live in apartments or townhouses compared to single family dwellings, and are more likely to use primarily non-electric heating. However, differences between participant and non-participant households cannot be attributed solely to different propensities of selecting into the program among household types. This is because BC Hydro engaged in a range of advertising for Team Power Smart, such as on bus shelters and online, that is unlikely to have been uniformly noticed by different household types and thus affects their likelihood of becoming participants.

The average monthly electricity bills among participant households is $58, meaning that the $75 reward is equivalent to 10.7% of the annual electricity bill in addition to their bill savings (Table 1). While participants have lower average electricity use than non-participants this is almost entirely a composition effect from different observable household types. After controlling for building and heating type the monthly 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.[13] This indicates that households selecting into Team Power Smart do not have substantially different electricity use from households in similar buildings who are not participating.

[11]BC Assessment data was provided by the University of British Columbia Centre for Urban Economics and Real Estate.

[12]On average 69% of people in this area moved in the previous 5 years — Statistics Canada 2016 Census.

[13]Specification and estimates are presented in Appendix A.2.

Table 1: **Household Characteristics**

	Participants		Non-Participants		All Residential	
	N	%	N	%		%
Building Type						
1 Story Single Family Dwelling	3,796	39	8,764	46		29
2 Story Single Family Dwelling	2,715	28	5,088	26		21
1.5 Story Single Family Dwelling	400	4	977	5		3
Apartment	1,414	14	1,813	9		29
Townhouse	1,202	12	1,677	9		13
Other	291	3	931	5		5
Heating Type						
Non-Electric	5,599	57	9,687	50		-
Electric	2,875	29	7,294	38		-
Unknown	1,344	14	2,269	12		-
Bedrooms						
0	12	0	12	0		0
1	505	5	703	4		10
2	1,599	16	2,745	14		23
3	3,625	37	6,709	35		28
4	2,263	23	4,569	24		17
5 or more	1,814	18	4,512	23		22
Total Households	9,818	100	19,250	100		
	Participants		All Non-Participants		All Residential	
	Mean	SD	Mean	SD	Mean	SD
Monthly kWh	884	568	971	636		-
Average Monthly Bill	$58		$64			-
Property Value ($1,000)	$662	$467	$721	$575	$638	$700
Floor Area (Square Feet)	2016	934	2123	997	1842	1104

Notes: This table shows the building characteristics and electricity use of participant and non-participant households in the sample, as well as those of all BC Residential units from the same geographic area (Greater Vancouver.) SD = standard deviation.

Table 2: **Probability of Challenge Outcomes**

Challenge number	Households in Challenge	Probability of Re-Enrolling: All	if Failed Challenge	if Passed Challenge	Probability of Passing: All	if Failed Previous. Chal.	if Passed Previous. 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: Households in Challenge 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] Prev. 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.

2.3 Outcomes During Multiple Conservation Challenges

Table 2 summarizes the decisions and outcomes of participant households across multiple conservation challenges. During the initial three challenges, 60-62% of households decided to re-enroll in an additional challenge; this probability mechanically declined among later challenges due in part to the limited panel length. Consistent with an increasing difficulty of achieving additional reductions in electricity use, the unconditional probability of passing a conservation challenge declines with additional challenges.[14] 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.[15] 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 goal.[16]

[14]This decline is statistically significant. Regressing an indicator for goal success 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).

[15]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.

[16]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 that 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. Second, 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 that 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 vacation. 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. Third, 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 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 (Angrist and Pischke, 2008). By estimating changes over time within households the event study model identifies treatment effects independent of self-selection into the program on observable and unobservable time invariant characteristics. However, like all difference-in-difference research designs this cannot identify causal treatment effects if self-selection is related to changes in electricity use; of particular concern here is self-selection due to energy efficiency investments or expectations of future consumption. As a result, this empirical approach provides an upper bound of the treatment effect given that estimates may be biased downward (more negative) if time-varying self-selection has occurred. I then use the estimated pattern of changes in electricity use before, during, and after participation to provide evidence on what forms of self-selection may have occurred.

To identify causal treatment effects, Section 5 exploits a discontinuity in the probability that households continue to a second conservation challenge in a fuzzy Regression Discontinuity (RD) design (Lee and Lemieux, 2010). By comparing households' decisions on whether to re-enroll in a subsequent challenge, Section 5 also provides insights into how households respond to information on their success and failure to achieve their conservation goal.

4 Event Study Empirical Strategy

I estimate two closely related event study models. The first is a standard event study model:

$$y_{it} = \sum_{\tau=-119}^{108} \beta_\tau D_{it,\tau} + \alpha_i + d_t + \epsilon_{it} \tag{1}$$

where y_{it} is the log of monthly electricity use for individual i in month-of-sample t, α_i is an individual fixed effect, and d_t is a month-of-sample fixed effect. $D_{it,\tau}$ is a dummy variable equaling 1 if individual i in month-of-sample t began treatment τ months previously, with $\tau = 1$ the month-of-sample a treated household undertakes its first conservation challenge. In my main specifications I include a control group of non-participant households who are never treated and therefore have $D_{it,\tau} = 0 \,\forall\, \tau$. I set the second year before each households initial conservation challenge as the baseline year by defining $D_{it,\tau} \equiv 0 \,\forall\, \tau \in [-12..23]$; this allows important trends in the 12 months preceding participation in the program to be estimated. β_τ are the non-parametric changes in electricity use relative to the baseline for τ months lag or lead of treatment and cover all periods in the panel.

Comparing estimated $\hat{\beta}_\tau$ 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. However, households were not required to immediately start a subsequent conservation challenge: some households undertaking multiple conservation challenges have a gap in time between when their previous challenge was completed and when they re-enrolled in a subsequent challenge.[17]

To account for this variable gap in time I modify the event study model in two ways. First, I include the indicator $G_{itg} = 1$ if household i in month-of-sample t has completed challenge g but has not yet re-enrolled in challenge $g + 1$. Second, I define $D_{it,\tau}$ equal to 1 if individual i in month-of-sample t began treatment τ months previously where any months with $G_{itg} = 1$ are excluded from counting τ.

$$y_{it} = \sum_{\tau=-119}^{108} \beta_\tau D_{it,\tau} + \sum_{g=1}^{8} \theta_g G_{itg} + \alpha_i + d_t + \epsilon_{it} \tag{2}$$

G_{itg} is not necessary for estimating the event study model of Equation 1; instead, it simplifies the interpretation of $\hat{\beta}_\tau$ across households with variable gaps between their challenges. For example, households ending their participation after a single challenge $\hat{\beta}_\tau, \tau = [13..24]$ are the estimated changes in electricity use for the first 12 months immediately following their initial challenge. For households re-enrolling in a second challenge $\hat{\beta}_\tau, \tau = [13..24]$ are the estimated changes in electricity use for the twelve months of the second conservation challenge regardless of the gap between conservation challenges. 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

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

excluding shorter and longer gap lengths.

4.1 Event Study Estimates—Initial Conservation Challenge

At the end of every challenge, households decide whether to end their participation, or re-enroll in a subsequent challenge. Event-study estimates of, for example, households that complete at least a 2nd challenge includes both households that ended their participation, and those that re-enrolled in a third challenge; this averages any potential rebound in electricity use among those that ended participation with any additional treatment effects from a 3rd or subsequent challenge undertaken by the re-enrolling households.

To separate the effects of ending participation compared to re-enrolling, Figure 1 presents event-study estimates for households depending on how many challenges they participate in.[18] In panel 1(a) I estimate equation (1) pooling all participant households regardless of how many times they re-enroll. Estimates $\hat{\beta}_\tau$, $\tau > 12$ in panel 1(a) are the average change in electricity use across both households that ended their participation after the initial challenge, and households that re-enrolled. As a result, in panels 1(b), (c), and (d) I estimate equation (2) separately for households that respectively decide whether or not to re-enroll after a first, second, and third conservation challenge. Each panel shows the estimates for two mutually exclusive groups (end participation vs. re-enroll), which combined are the re-enrolling households from the previous panel. Red circles denote point estimates for months households have undertaken a challenge prior to their re-enrollment decision; as mentioned, households in subsequent months may or may not be enrolled in additional challenges.

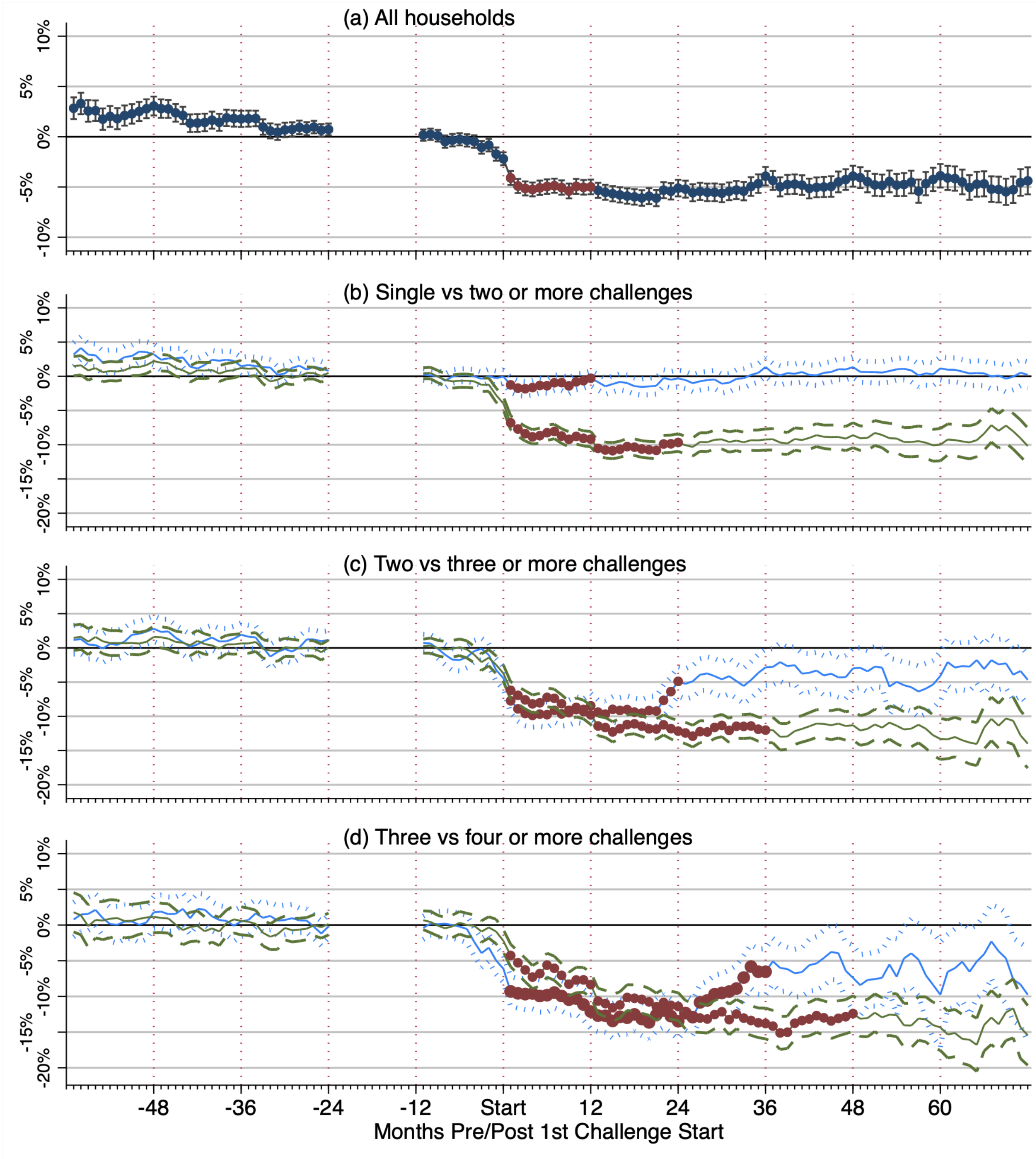
The self-selection decision

Several features in Figure 1 are informative of how households respond to the conservation challenge, the self-selection occurring, and ways in which estimates may be biased. In panel 1(a), the twelve months of the initial conservation challenge show a substantial reduction in electricity use of 4.9% relative to the second year pre-treatment baseline. The lack of a trend in monthly estimates over the 12 months of the 1st challenge shows that, on average, households are not significantly increasing or decreasing their electricity conservation during this 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 conservation also indicates 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 rebounding.

While panel 1(a) 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 an initial challenge from the effects of subsequent challenges. Panel 1(b) shows that households that end their program

[18] Table H.8 of the Appendix lists all point estimates. Results are robust to including all non-participant households, using other baseline periods, and excluding the never-treated control households; see Appendix B.2.

Figure 1: Event study estimates



Notes: Estimated $\hat{\beta}_\tau$ with 95% confidence intervals from equation (1) for panel (a) and equation (2) for panels (b)-(d). Treatment, the start of the first challenge, begins in the month labelled *Start*. The visual gap in estimates between months $\tau = -12$ and $\tau = -23$ is the excluded reference year; $\hat{\beta}_\tau$ are the percent change in electricity use relative to this reference. Panel (a) pools all households; estimates for the period after the initial challenge ends ($\tau > 12$) contain both households ending participation and those that re-enroll. Panel (b)-(d): Estimates in blue are households that end participation after the given challenge and estimates in green are for households that re-enroll. Red dots mark months in which a household has undertaken a challenge; subsequent months (in green) that may include additional challenges are not marked. The estimation samples are limited to households that, if they re-enroll, do so 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. This aligns months for households that end their participation with months for households that re-enroll. Estimates include individual and month-of-year fixed effects. I cluster standard errors at the household level. See Appendix H for all point estimates.

participation after their initial challenge have average reductions during the challenge of only 1.2%. In comparison, households that re-enroll in a second challenge reduce their use by 7.2% during their first challenge. 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.

The lack of large reductions in electricity use among single-challenge households is important to panel 1(a)'s declining trend in pre-treatment estimates over months -60 to -1. That these pre-treatment estimates are not zero suggests a potential violation of the parallel trends assumption. However, panel (b) shows that the non-zero pre-treatment estimates arise among households that participate in a single conservation challenge and do not re-enroll, and are not present in estimates for households that do re-enroll. As reductions in electricity in the pooled estimates of panel 1(a) come predominantly from households that re-enroll—and who have pre-treatment estimates largely indistinguishable from zero—a violation of the parallel trends assumption suggested by the single-challenge households of panel 1(b) does not bias the short or long-run estimated reductions in electricity use.

In Appendix B.1 I further investigate the non-zero pre-treatment estimates; I find no significant difference between pre-treatment trends for treated and control households. The pre-treatment estimates are robust to alternate specifications including exclusion of the non-participant control households. Estimates are highly similar to those presented in Figure 1(a) (but with larger errors), indicating that a violation of parallel trends between participant and non-participant households is unlikely to be the cause of the declining trend in event-study estimates. The pre-treatment decline could reflect heterogenous time trends between single-challenge and re-enrolling participant households that are not fully controlled for by month-of-sample fixed effects, or a violation of the event-study assumption of homogenous treatment effects (Borusyak and Jaravel, 2018).

Panels 1(c) and 1(d) show a similar pattern; both households that decide to end their participation after the challenge and those that decide to re-enroll have similar changes in electricity use leading up to and during the challenge. Households that end their participation show a rebound in their use over the final months of the challenge; those that re-enroll show continued reductions. These patterns suggest that households that don't re-enroll stopped making conservation efforts prior to the end of the challenge. This rebound does not return electricity use to pre-program levels; electricity use remains approximately 6.5% lower than pre-program levels.

4.2 Internal and external validity of event-study estimates

Self-selection into participation can bias estimates even when pre-treatment coefficients are zero. Of particular concern is if households make an energy efficiency investment like purchasing a new appliance and sign up for Team Power Smart as a result. Given the voluntary nature of the program this bias cannot be ruled out and the event study model estimates are thus an upper bound on the causal treatment effect. This is also one potential explanation for the pre-program decline in months $\tau = [-1, 0]$

in Figure 1(a), in which households reduce their use prior to the declared program start. However, this source of bias is unlikely to be large as making an energy efficiency investment would likely result in persistent reductions in electricity use; by contrast, electricity use rebounds as households leave the program. In addition, the pre-program decline can result mechanically from the billing process as BC Hydro did not record electricity bills on a fixed monthly basis.[19] Instead, BC Hydro used a rolling billing period where different houses are billed on different days and with up to two months between electricity meter readings. Electricity use was then calendarized to monthly consumption. As a result, reductions that occur after the start of a conservation goal 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.[20]

A further concern is that households may begin 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 being credited to the program. If this self-selection occurs it will manifest as positive pre-treatment effects in the months immediately prior to the initial conservation challenge. However, there is a decline over months $\tau = [-1, 0]$, suggesting that households do not self-select into the program based on past consumption shocks.

The relative stability of the households studied compared to the general population also raises questions about external validity. One concern is the difference in building types between participants and the general population in Table 1. In Appendix Table B.1 I estimate the reduction in electricity use separately across building types and other observables, and find similar reductions. Re-weighting separate estimates by the composition of building types in the population predicts an average reduction of 4.8%, compared to 4.3% estimated among participants. This shows that self-selection into the sample based on observable building characteristics is not a substantial threat to external validity. Unobservable differences between participants and the general population do remain a concern, though the similarity in estimates across different building characteristics and quartiles of pre-program use suggests that estimates are generalizable to a variety of building and household types. Compared to the average electricity consumer, relatively stable households do have a greater incentive to make energy efficiency investments that pay off over time. Similarly, stable participants may have more low-cost ways to reduce electricity use as a result of being more familiar with their home and its energy use. This is a further reason in addition to self-selection that the event-study estimates should be viewed as an upper bound when extrapolating to the average household.

[19] BC Hydro began a rolling installation of smart meters in 2011 which allowed collection of hourly electricity use data. 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.

[20] I discuss the rolling billing period and its effect on estimates in more detail in Appendix B.1. 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.

5 Success vs. Failure and Re-enrollment

Section 4 documented how electricity use diverges between those who decide to leave Team Power Smart, and those who decide to re-enroll in an additional conservation challenge. This suggests that the decision to re-enroll is important as energy use may rebound in the absence of subsequent program participation, and that additional conservation challenges could cause additional reductions in electricity use. In this section I study two aspects of households' re-enrollment decisions; the decision to attempt a subsequent goal in response to success vs. failure, and causal reductions in electricity use from continuing to a second conservation challenge.

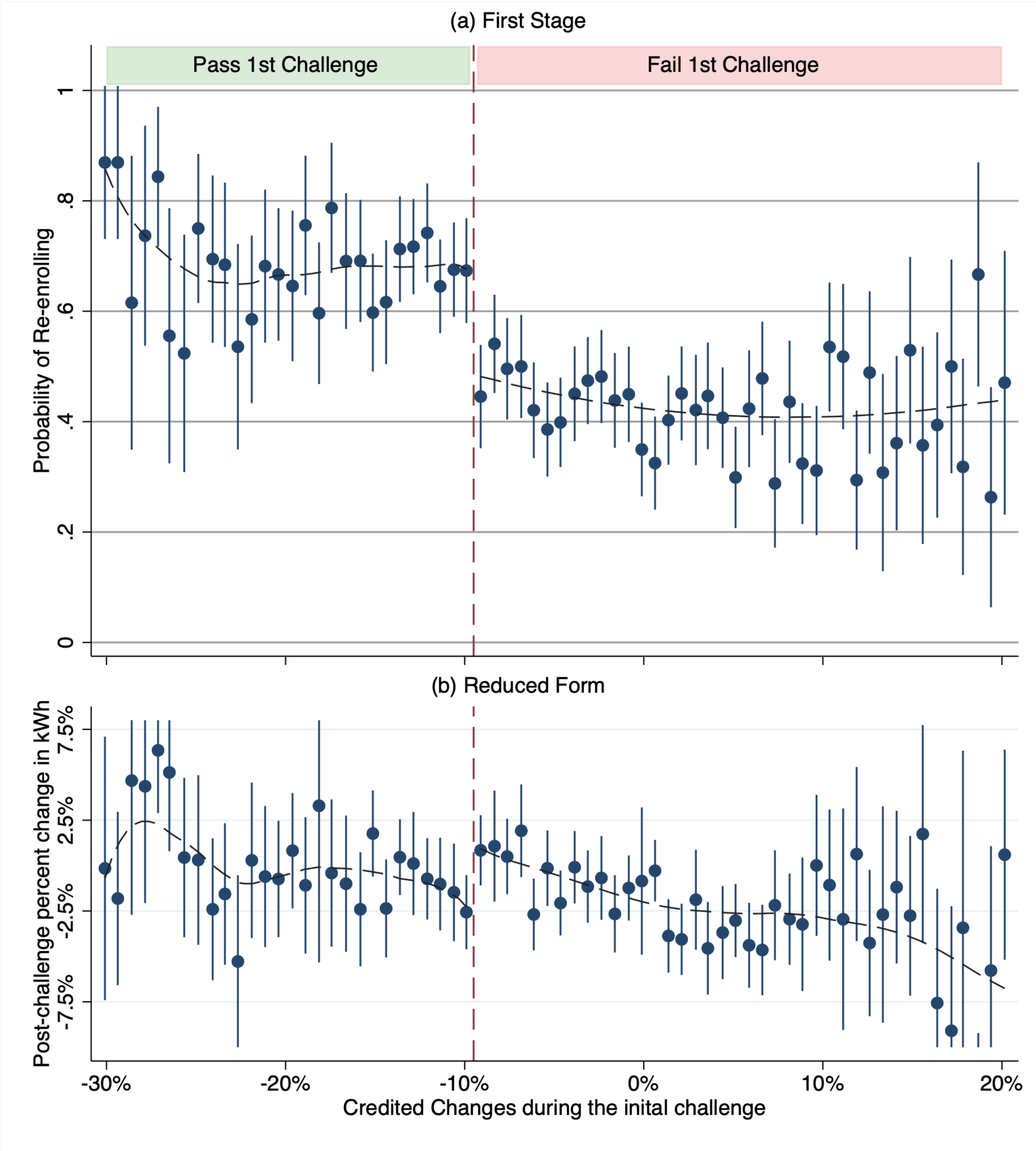
Before discussing mechanisms it is helpful to see the discontinuity. Figure 2 plots the probability of re-enrolling in a second challenge against the credited changes in electricity use from a household's initial conservation challenge. Among households that fail their initial challenge, their probability of continuing to a second challenge is largely independent of their credited changes in electricity use. Figure 2 shows that households with large increases in electricity use of around 20% have a similar probability of re-enrolling as households that nearly achieved their 10% reduction goal. A similar pattern is seen amongst households that passed their challenge; those that barely pass with reductions ∼10% are equally likely to continue as households that achieved reductions of ∼20%. In contrast, there is a sharp discontinuous jump in the probability of continuing to a subsequent challenge at the 9.5% threshold for success.[21] 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. Importantly, this discontinuity occurs only at the 9.5% threshold which households did not expect, and not at the 10% goal households expected and were trying to achieve.[22]

The magnitude of the discontinuity in Figure 2 is large compared to differences in re-enrollment probability across household characteristics. To explore what correlates with households' re-enrollment decisions I estimate several Probit models with details in Appendix D. 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 for the probability of re-enrolling across characteristics finds Townhouses are 8.6 percentage points 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 percentage points more likely to re-enroll. In comparison, households that just pass their conservation Challenge are 14.5 percentage points, or 27%, more likely to re-enroll than if they just fail.

[21] As expected there is no discontinuity in the billed electricity use: see Appendix Figure D.1. While similar discontinuities in credited changes exist after the second and subsequent conservation challenges, the sample is too small for instrumental variable estimates.

[22] See Appendix Figure E.1 for a narrow bandwidth of the discontinuity at 9.5% and the lack of discontinuity at 10%.

Figure 2: **Discontinuities in Re-Enrolling and Subsequent Electricity Use**



Notes: Credited changes are the annual changes in electricity consumption displayed to households. The vertical dashed line indicates the 9.5% threshold defining success; households to the left of the dashed line pass their conservation challenge while those to the right fail. Panel (a): The mean probability of re-enrolling with 95% confidence intervals among households within 0.75% width bins in credited changes. Panel (b): The mean change in billed electricity use in the first year after the initial conservation goal. The dashed line is a first order local polynomial fit; this is to clarify the local trends and is not the fuzzy RD fit.

5.1 The response to success vs. failure

Why does success or failure in achieving a goal have such a large persuasive/dissuasive effect on the decision to re-enroll? The specific design of the Team Power Smart energy conservation goals helps to clarify potential mechanisms. The inability to precisely manipulate energy conservation, the weather adjustment, and the ex-ante unexpected 9.5% threshold all contribute to households being as good as randomly assigned into success and failure. Comparing households that just failed to those that were slightly more successful and therefore just passed allows otherwise identical households to be compared; this rules out differences in re-enrollment being due to differences in characteristics like effort or ability. As discussed previously, the $75 reward offered for another 10% reduction challenge does not depend on prior success or failure. Conditional on their previous year's electricity conservation, households face the same incentives to re-enroll independent of past success or failure. Importantly, households were notified when their energy conservation challenge was completed, but were not informed of their success or failure until they logged into their online account. Once logged in, households received their final success or failure status directly alongside their degree of credited energy conservation; successful and unsuccessful households were equally aware of their degree of success. Combined, these features created a remarkably clean natural experiment; the large discontinuity in the decision to re-enroll from Figure 2(a) is not due to different households types, different incentives, or different information being provided to those who pass vs. fail. This suggests that *how* success vs. failure, and the degree of success, is interpreted is critical to their decision whether to re-enroll.

Mechanisms underlying responses to success vs. failure can be broadly separated into the use of information communicated by success and failure, and psychological responses to emotional and normative aspects. In deciding whether to re-enroll, the degree of success or failure in a previous goal is objective information from which people can update beliefs about their ability and the goal's difficulty. The large psychology literature on goal-setting finds that self-efficacy, which is the belief in one's ability to affect outcomes, can be altered by information on success and failure, and this in turn influences goal commitment and effort (Gutt, Rechenberg, and Kundisch, 2020; Soman and Cheema, 2004; Locke and Latham, 2002; Bandura and Locke, 2003). However, as households observe their degree of success—their energy conservation—the binary of success vs. failure does not itself convey additional information and a fully informed rational agent would not respond discontinuously. It is well known that consumers may be inattentive to information or find understanding its implications too mentally costly to consider—see Gabaix (2019) for a review of behavioral inattention. An inattentive household may update their beliefs about whether they'll succeed in another goal based on a simple heuristic that previous success implies future success, and past failure implies future failure. While inattention has been studied in a wide variety of contexts, including energy use (Allcott, 2011a; Sallee, 2014; Allcott and Taubinsky, 2015; Davis and Metcalf, 2016), inattention to information as an explanation for responses to success vs. failure does not appear to have been directly studied in any setting.

How people interpret information can also be affected by information's emotional and normative aspects. As Eil and Rao (2011); Peysakhovich and Karmarkar (2016); and Mobius, Niederle, and Niehaus (2014) show, 'good' news can have a larger effect on decisions than 'bad' news, which leads to asymmetrical responses. Such asymmetrical updating could cause the observed discontinuous response if households update their beliefs, such as a belief in their ability to reduce their energy use vis-à-vis the costs of doing so, more strongly in response to the good news of success than the bad news of failure. This is consistent with Eskreis-Winkler and Fishbach (2019) who find people answering simple binary-choice questions learn less when they are told of prior failure than prior success, even when it was more cognitively taxing to learn from their success. That success and failure are not viewed in purely objective terms is supported by Medvec, Madey, and Gilovich (1995) who show that Olympian bronze medal winners are happier than those winning silver; their explanation is that satisfaction is determined more by what the Olympian envisions as the counterfactual outcome rather than their objective place on the podium.

Psychological responses to the emotional and normative aspects of success and failure may also directly affect subsequent decisions. A discontinuous response to success vs. failure may arise if households face a higher emotional cost from a second failure than from an initial failure, leading to those that initially fail being less likely to re-enroll than those who initially succeed even if they have identical beliefs about their likelihood of subsequent success. Gill and Prowse (2012) consider disappointment aversion where agents are loss-averse around an endogenous expectations-based reference point that is determined through competition with a rival. If households are similarly disappointment averse they may adjust their reference point—separate from an updated belief about future success—in response to success and failure. This may lead them to avoid re-enrolling in order to avoid the cognitive cost of greater future disappointment. Work on regret aversion by Marcatto, Cosulich, and Ferrante (2015) finds that experiencing regret directly inhibits people from choosing the same option a second time "even when it is still objectively the best alternative." Extensions of prospect theory to dynamic decision making (Tymula et al., 2021) and a large literature on reinforcement learning theory (Rangel, Camerer, and Montague, 2008) offer alternative theoretical frameworks for describing how past outcomes like success and failure may affect the subsequent decisions.

A remaining potential explanation is that the $75 reward received has a direct effect on the decision to re-enroll by altering a household's budget constraint. However, if a $75 income shock made a material difference to re-enrollment then presumably re-enrollment would also increase with household income. I do not find that the probability of re-enrolling differs across property values or floorspace size which I assume to be correlated with household income (see Appendix D for details). While $75 is a small reward relative to the median provincial household income of $80,000, it may have a larger effect if households use mental accounting and consider the $75 as part of an energy category rather than aggregate income. While theorized, mental accounting in energy use has not been demonstrated (Hahnel et al., 2020). Households would also have to direct the $75 reward into an energy conservation mental category (like energy efficient lightbulbs), not energy expenditure, or else the reward would decrease the benefit of re-enrolling.

5.2 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 2, 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:

$$C_i = \gamma_0 + \gamma_1 1\{R_i \leq \bar{R}\} + \gamma_2 R_i + \gamma_3 1\{R_i \leq \bar{R}\} \times R_i + \gamma_4 B_i + \gamma_5 X_i + \eta_i \tag{3}$$

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 -9.5% threshold for success in the challenge, $1\{R_i \leq \bar{R}\}$ 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. B_i and X_i are not necessary for causal identification. 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:

$$y_i = \beta_0 + \beta_1 C_i + \beta_2 R_i + \beta_3 1\{R_i \leq \bar{R}\} \times R_i + \beta_4 B_i + \beta_5 X_i + \epsilon_i \tag{4}$$

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

$$y_i \equiv \frac{(u_{i,\tau=2} - u_{i,\tau=1})}{u_{i,\tau=0}} \tag{5}$$

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 their total electricity use in the 12 months immediately following the completion of their initial challenge, $u_{i,\tau=1}$ is their total electricity use during their initial challenge, and $u_{i,\tau=0}$ is their use during the pre-program 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. If (4) could be directly estimated without an instrument, β_0 would be 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.

Causal fuzzy-RD estimates require households to be as good as randomly assigned at the discontinuity and the exclusion restriction to be satisfied. Due to the Team Power Smart design, in particular that the threshold for success was not known in advance to households and the large ex-post weather correction, households were unable to precisely manipulate their assignment into treatment of success. This is supported by tests of the continuity of observables and a McCrary (2008) test of the density of observations which fail to reject continuity at the 9.5% treatment threshold — details and a further discussion of identification are in Appendix E.

The exclusion restriction requires that success in the initial challenge only affects subsequent electricity use through the decision to re-enroll. This assumption could be violated if households respond to their success or failure in a way that directly affects their subsequent conservation effort, or if the $75 rebate causes an income effect and alters post-program conservation. As $75 is small relative to households' incomes I assume there is no income effect that influences electricity use. Any emotional or behavior response to success and failure that directly affects subsequent conservation effort is likely to be particularly strong during the initial months of the next challenge while it is still fresh in a household's mind. By contrast, the event study estimates of Figure 1(b) indicate that additional energy conservation during the second challenge is consistent throughout the twelve months of the challenge, suggesting that there is no strong warm-glow or effect of disappointment on effort.

5.3 Fuzzy Regression Discontinuity Estimates

Across a wide variety of fuzzy regression discontinuity specifications and robustness checks I find a 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 the finding that additional conservation challenges cause additional reductions in electricity use, and that electricity use partially rebounds as households leave the program. Table 3 presents my preferred specification. Columns (3) through (7) show results estimated for different bandwidths from ±7% to ±3% around the threshold of a -9.5% change in credited electricity use.[23,24] Panel (A) shows the first-stage results for the probability of re-enrolling in a second challenge estimated from equation (3). For my preferred bandwidth of 5% I find that, conditional on failing the initial challenge, 53% of households at the 9.5% threshold re-enroll. Households that just succeed are 14.5 percentage points, or 27%, more likely to re-enroll than those which just failed. Across bandwidths from ±7% to ±3% I find the F-statistic on the instrument of success in the initial challenge decreases from 22 to 6.5.[25] This indicates that the first stage is reasonably strong for larger bandwidths, but the decreasing sample size limits the strength of the 1st stage as the bandwidth narrows.

Table 3 panel (B) reports the OLS, Reduced Form, and second-stage IV estimates of equation (4) for households who re-enroll within 12 months.[26] Specification (1) is the OLS estimate; this shows

[23]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.

[24]Plots of First Stage and Reduced Form with different bandwidths are presented in Appendix Section E.1.

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

[26]Results are robust to other re-enrollment windows and are kept at 12 months for consistency with other estimates.

that re-enrolling in a second challenge is associated with an on average 1.6% decline in post-challenge electricity use, relative to households that do not re-enroll. Specification (2) is the Reduced Form with a 5% bandwidth in Specification (5) and corresponds to the discontinuity in Figure 2(b). Specifications (3) through (7) show the IV estimates for different estimation bandwidths. These estimates are noisy yet find a consistent pattern where, for households that comply with the instrument, continuing to a second conservation challenge causes a reduction in electricity use. Part of the explanation for the noisy estimates can be seen from Figure 2(b); the height of the reduced form discontinuity is sensitive to the trend functional form, and bandwidth, around the threshold. For my preferred bandwidth of 5% and a linear specification I estimate that continuing to a second conservation challenge causes a 23% reduction in electricity use. This is a large effect.

In interpreting the IV estimate it is important to account for this being a local average treatment effect (LATE) for instrument compliers (Angrist and Pischke, 2008), and account for the definition of the outcome variable y_i being the post-challenge percent change in electricity use defined in Equation 5. y_i is the change in electricity use from a household's initial conservation challenge to the following year. 0% would mean a household maintains its initial challenge reductions (which were on average a 9.1% reduction compared to the pre-program year for a 5% bandwidth), a positive y_i is a rebound towards their pre-program use, and a negative y_i is an additional reduction in electricity use. The local IV estimate is for households achieving close to a 9.5% reduction in their initial challenge who comply with the instrument (success) by re-enrolling in a second challenge. This IV strategy cannot identify the level of electricity use for compliers who end their participation separately from compliers who re-enroll; it only identifies the difference — the treatment effect — caused by their re-enrollment. As a result, the 23% conservation caused by re-enrolling cannot be separated into the additional conservation (relative to the reduction achieved over the initial challenge) for those that re-enroll, and any rebound among those that do not re-enroll. With the OLS and event-study estimates being across all households — including compliers, always-takers, and never-takers — the relatively large IV estimate suggests that the average energy conservation is predominantly due to the instrument compliers, with the always-taker's and never-taker's contributing relatively little energy conservation as well as being unresponsive to their success or failure.

In Appendix G 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, Cattaneo, and Titiunik (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 maintain 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.

See Appendix G.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			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× Cred. Reduc.			1.166	2.491*	2.565	-0.778	-3.973
			(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	RF	Instrumental Variable Estimates				
Window		±5 %	±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.		-0.507*	-0.412*	-0.643*	-1.185*	-1.108	0.785
		(0.307)	(0.241)	(0.354)	(0.654)	(0.828)	(0.654)
β_3 : Success× Cred. Reduc.		0.274	0.303	0.375	0.867	-0.229	-1.732
		(0.442)	(0.310)	(0.426)	(0.624)	(0.985)	(1.229)
β_4 : Billed Reduc.		-0.0904	-0.0461	-0.113	-0.101	-0.216	-0.222
		(0.0986)	(0.0917)	(0.103)	(0.121)	(0.168)	(0.161)
Success Ind.		-0.0334**					
		(0.0132)					
β_0 : Constant	-0.00773***	0.0157*	0.0712**	0.103**	0.138**	0.184*	0.0944
	(0.00289)	(0.00912)	(0.0356)	(0.0447)	(0.0698)	(0.0950)	(0.0625)
N	5432	1475	2050	1763	1475	1196	888

Notes: Specification (1) is the OLS estimate across households. (2) is the Reduced Form corresponding to column (5). (3)-(7) are fuzzy-RD estimates corresponding to equations (3) and (4). Estimation sample for the OLS and IV is restricted to households that either start their next challenge within 12 months or do not undertake an additional challenge. The 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.

6 Conclusion

This paper investigates how households respond to repeated energy conservation goals. Using a 10-year panel of monthly electricity use, I study households decisions to re-enroll in subsequent energy conservation challenges, and estimate their long-run changes in electricity use. Those who succeed in achieving a goal typically differ from those who fail, and success typically results in different subsequent incentives. In contrast, the Team Power Smart program studied here randomized some households into success and failure, yet offered both successful and unsuccessful households the same information and same incentive to re-enroll. Using this natural experiment I find that households' decisions to continue participating are notably discontinuous based on their success or failure, yet largely unresponsive to their degree of success. As a result, they do not make their re-enrollment decision consistent with being a conventional well informed rational agent. I also use an event study model to estimate that electricity use continues to decline as households re-enroll in additional conservation challenges, but partially rebounds as households leave the program. Fuzzy regression discontinuity estimates find that re-enrolling causes large reductions in electricity use. The continued declines in electricity use as households participate, and rebounds if they do not, shows that the ongoing incentive of the goals and financial rewards are important for long run lower electricity use.

That peoples' decisions to continue participating are highly responsive to a prior goal's success or failure is an important finding to consider in the design of many energy conservation and repeated goal programs. Voluntary programs are ubiquitous, and by definition involve participation decisions. When participants do not respond solely to the marginal information and incentives provided, the impacts of past failure and success need to be accounted for. This makes the optimal goal-setting design dynamic; repeated goals should not be designed as repeats of single-attempt goals. For example, the continued reductions in electricity use as households continue to participate in Team Power Smart, and the rebound when they do not, demonstrates an important trade off; ambitious goals may incentivize effort within a goal yet discourage continued participation if they lead to higher failure rates. Compared to a goal offered only once, the optimal goals of a repeated goal-setting program may be less ambitious once the impacts on subsequent participation are accounted for. Relatedly, programs may wish to avoid clear messages of failure in order to keep participation rates higher. Consolation prizes may be one way to raise participation rates by reframing failure to achieve an initial goal as success in achieving an intermediate goal.

This paper's demonstration of a stark discontinuous response to success and failure is a promising direction for research. Many programs beyond energy use provide consumers with more detailed information so they can make more informed decisions. If participation and effort decisions are made based on emotional reactions or simple heuristics like past success implies future success and past failure implies future failure, then how people interpret the information they're provided in the context of achieving a goal may be an important factor to consider alongside the content of the information itself. Even decisions that do not explicitly involve goals may invoke aspects of success and failure with corresponding discontinuous responses. For example, a household's repeated decisions of how much energy to use or how much to spend on entertainment can both involve periodic revealing of 'success' or 'failure' to stay within a budget. Programs beyond energy conservation which are focused on changing incentives or providing information may not alter decisions as expected if emotional and psychological mechanisms like disappointment aversion, the 'good news-bad news' effect, or substantial inattention is widespread.

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The Online Appendix is available at www.alastairfraser.ca