Pro-Environmental Incentives and Loss Aversion: A Field Experiment on Electricity Saving Behavior*

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Abstract

This paper reports evidence from a field experiment investigating households' electricity saving behavior. We motivated households' efforts to save electricity via pro-environmental incentives that did not affect people's monetary utility but targeted their environmental preferences. The results show that such pro-environmental incentives can be effective, especially when framed as potential losses to the environment. Our loss-framed pro-environmental incentive led households to save 5% on their monthly electricity consumption compared to a control group.

Keywords: Pro-environmental behavior, Ecological incentives, Loss aversion, Loss framing, Randomized field experiments, Energy conservation
JEL Classification: D12, D91, Q48

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1 Introduction

The assumption that people respond to incentives is a cornerstone of economic analysis and policy. The most straightforward approach to behavior change is typically to alter people's incentives in a way that they find it optimal to adapt their behavior in the desired fashion.[1] Thus, if a policy maker would like consumers to reduce the consumption of a good that causes negative external effects, e.g., reduce electricity consumption, the first-best approach would be to tax the per-unit use according to its marginal social cost (Pigou, 1920). However, political processes often make it difficult to change financial incentives by levying environmental taxes (see, e.g., Engel et al., 2008; Felder and Schleiniger, 2002) or introducing marketable incentive schemes (see, e.g., Stephan and Paterson, 2012). To circumvent these obstacles in the political process, instruments from the behavioral economics toolkit have recently gained attention (see, e.g., Andor and Fels, 2018; Liebe et al., 2018; List and Price, 2016, for comprehensive reviews of the literature). The goal of such interventions is to curb externalities by steering behavior in the desired direction when the use of classical policy instruments, such as taxes, subsidies, or command-and-control regulation, is not feasible and policies instead need to rely on the voluntary contribution of actors (see, e.g., Allcott, 2011; Croson and Treich, 2014; Kesternich et al., 2017).

Contributing to this behavioral approach to public environmental policy, we report results from a framed field experiment[2] in collaboration with EnBW ODR AG, a German utility. We studied whether non-monetary incentives that target people's environmental preferences can be used to achieve reductions in households' electricity consumption. To this end, household customers were invited to participate in a campaign by the utility provider and were told that in return for four manually entered meter readings in four subsequent months, they were eligible for a prize (they were also explicitly told, that their electricity consumption did not impact on the winning probability). After a baseline period of one month, we randomly assigned households to four groups, one control group and three groups, which were subject to different experimental conditions. The experimental conditions were all concerned with non-monetary instruments. Our control group (*CONTROL*) received general electricity savings tips and encouragement to save electricity. Our first experimental group (*GOAL*) additionally received an exogenously determined savings goal of a 5% reduction in consumption, relative to their baseline level. In our second and third experimental group (the *GAIN* and *LOSS* group), participants

[1]Consider, for instance, empirical evidence on the effectiveness of performance pay for increasing worker effort (e.g., Lazear, 2000; Shearer, 2004) or how people can be motivated to save more for retirement via subsidies or tax benefits (Duflo et al., 2006).

[2]As defined by Harrison and List (2004), framed field experiments are experiments within a field context that target and measure real participant behavior (e.g., electricity consumption in our case). However, unlike in a natural field experiment, participants are usually aware that their (anonymized) behavior may be analyzed for research purposes and they give informed consent to participate in the study (see, e.g., Chen et al., 2017; Goette et al., 2019; Tiefenbeck et al., 2016; Schultz et al., 2016, for framed field experiments targeting environmentally-relevant behaviors).

received an exogenously determined savings goal of a 5% reduction in consumption but, in addition, they knew that a tree would be planted if they reached the pre-defined electricity saving goal. Thus, we matched a participant's pro-environmental deed (the voluntary reduction of electricity consumption) with another pro-environmental act (the planting of a tree). This intervention builds on previous literature showing that such matching of pro-social deeds can be an effective tool to increase pro-social behavior (e.g., Eckel and Grossman, 2003; Karlan et al., 2011; Meier, 2007; List and Lucking-Reiley, 2002).

Brandon et al. (2018) further show that combining behavioral economic instruments may increase electricity savings beyond the magnitude these instruments have in isolation. In this spirit, in order to make the pro-environmental incentive in our experiment more effective, we combined the incentive for some experimental groups with a differential framing manipulation. Specifically, we made use of the well-known effects of gain and loss framing that appeals to people's loss aversion (Kahneman and Tversky, 1979; Levin et al., 1998; Tversky and Kahneman, 1981).[3] Precisely, in our experiment, a gain frame of the pro-environmental incentive in one experimental group (GAIN) informed participants that a tree would be planted if they *reached* the electricity saving goal. Conversely, a loss frame in another experimental group (LOSS) informed participants that a tree was going to be planted as a recognition for their participation, but that this tree would *not* be planted if they *failed to reach* the electricity saving goal.

We find that pro-environmental incentives led to a significant reduction of electricity consumption compared to the control group. While our gain framed pro-environmental incentive reduced electricity consumption by about 2% compared to the control group, the subtle experimental variation between the gain and loss framed incentive magnified the effect. The loss-framed incentive led to a significant reduction of electricity consumption by about 5% with respect to the control group. In light of the findings of Andor et al. (2017) who claim that due to general low base-levels of electricity consumption in Germany[4] behavioral interventions such as feedback and social norms may be of limited use to curb electricity use, the significant reduction of electricity consumption by 2–5%

[3]Loss frames are powerful because losses seem to loom larger than gains in people's minds. This asymmetry is captured by prospect theory's value function that (i) evaluates outcomes with respect to a (potentially arbitrary) reference point, (ii) is convex for losses and concave for gains, and (iii) is steeper for losses than for gains in the domain near the reference point (Kahneman and Tversky, 1979). For a loss averse decision maker, the utility decrease in response to a loss is therefore larger than the utility increase in response to a gain of the same size, which motivates the decision maker to avoid losses with respect to the reference point (see, e.g., Abeler et al., 2011).

[4]The average yearly electricity consumption of a German household in 2013 was 3,304 kWh compared to 12,293 kWh of the average U.S. household (see WEC, 2016). The reason for this lower consumption in Germany compared to the U.S. lies in differences in household electricity usage. In Germany, households use electricity predominantly for plug-loads, lighting, cooking, and in part for hot water provision. In most households in Germany electricity is not used for space heating or cooling. In the U.S., in contrast, houses may, in some areas at least, be all electric (for all services), or a mix. This difference in demand for electricity results in a lower base rate consumption in Germany (and other European countries) which makes it more difficult for behavioral interventions to be effective in Germany compared to the U.S.

in our pro-environmental incentive groups seems noteworthy.

Our study contributes new insights to the literature as we specifically examine the effects of gain and loss-framed incentives on pro-environmental motivation.[5] The literature in behavioral economics on non-monetary incentives has so far mainly covered how the motivating power of loss aversion can be used to increase the effort of workers (or teachers) by applying loss frames to incentive contracts (e.g., Armantier and Boly, 2015; Fryer et al., 2012; Hossain and List, 2012). Even though the results of these studies are generally promising, loss-framed incentive contracts are not frequently observed in reality. A reason might be that individuals anticipate losses and therefore do not like to enter such contracts, which prevents employers from using them in the first place (Imas et al., 2016).[6]

The potential dislike of loss frames may also be the reason why surprisingly few behavioral policy interventions in the environmental domain make use of this tool. Policy makers may shy away from using loss frames because they fear negative reactions in response. Moreover, it may be difficult to quantify the cost of environmentally harmful behavior in many cases which may make the implementation of loss framed interventions more difficult.[7] The lack of policy interventions relying on loss frames when designing behavioral incentives, however, stands in contrast to popular initiatives, such as www.stickK.com, where people can define goals and commit to pay self-imposed fines for failing to reach a self-set target (Harvard Business School, 2014). The success and popularity of these on-line offers indicates that at least some people deliberately make use of loss contracts because the anticipated pain of losing helps them achieve their goals.

Nevertheless, it seems clear that loss framed interventions need to be designed very carefully. Our subtle version of a loss frame provides an example for a successful implementation that proved to be effective without triggering negative reactions from participants in our field experiment. In fact, we find no evidence in our data that participants tried to avoid the loss frame. Participants who were randomly assigned to the loss frame condition did not drop out more frequently from the study compared with participants who were assigned to the gain frame. Our results therefore show that easy to implement and low-cost changes in the framing of non-monetary pro-environmental incentives may thus be an effective tool to strengthen behavioral interventions that aim at triggering voluntary behavior change.

The remainder of the paper proceeds as follows. Section 2 describes the empirical

[5]Whereas loss framing of pro-environmental incentives has not been studied so-far, there are some studies investigating the effects of loss vs. gain framing on the processing of information about environmentally-relevant decision attributes (e.g. Avineri and Waygood, 2013; Chang and Wu, 2015).

[6]However, neither Imas et al. (2016) nor De Quidt (2017) find any empirical evidence that people avoid loss-framed incentive contracts.

[7]Some interventions from other policy areas, however, may be considered as similar to loss frames. Pictures on cigarette boxes, for example, suggest that smokers become seriously ill and may lose their lives if they continue smoking. The difference to loss-framed incentives, however, is that people are not explicitly given something (e.g., good health) which is taken form them if they do not stop smoking. This connection is rather implicit.

setting and the experimental design. Section 3 introduces a simple theoretical model to derive behavioral predictions. Section 4 reports the results from the experiment and Section 5 concludes.

2 Experimental Design

Our study investigates whether pro-environmental incentives, i.e., incentives that do not affect individual monetary utility but may impact people's behavior via pro-environmental preferences, can be used to motivate households to save electricity. Specifically, our experimental design has two aims in mind. First, we aim at exploring how pro-environmental incentives need to be designed in order to be most effective, thereby contrasting general encouragement and pre-determined reduction goals with gain and loss framed pro-environmental incentives. Second, we also target the question whether loss frames are a practical means in terms of actual policy making, assessing the participation rates among the different experimental groups throughout the trial.

2.1 Empirical setting and data collection

For our field experiment, we cooperated with EnBW ODR AG, which is a medium-sized electric utility from Southern Germany.[8] The utility invited a total of 35,000 households from their client base to partake in the study via e-mail.[9] Participation and completion of the trial was rewarded via a lottery in which all households who completed the study participated.[10]

The study consisted of three phases. In the baseline period (BASE), we measured base levels of electricity consumption during one month (mid-October to mid-November 2016). Participating households had to submit their electricity meter readings at the beginning

[8] The researchers designed the experiment, prepared the documents and arranged the randomization of households into the different experimental groups. The utility contacted the households via e-mail and provided a data collection platform for subjects to enter their electricity meter readings. The research team did not directly interact with the customers at any point during the experiment.

[9] The total client base of EnBW ODR AG consists of approximately 180,000 customers. However, households who operate heat-pumps or have electric heating and therefore have special tariffs and a much higher consumption than standard households were excluded from the study. Moreover, households without an e-mail address and households who had withdrawn consent to receive informational e-mails as well as utility employees were excluded from participation. The researchers could not personally identify households, but matched each household with an identification number. Upon registration for the study, informed consent on behalf of the households was obtained according to the rules and regulations of ETH Zürich's institutional review board.

[10] German data protection laws require that households provide consent when their data are being analyzed in a research study. Self-selection into the study was thus unavoidable. Given the sample, effects on the different experimental groups are unbiased, as we randomly allocated participating households to the experimental conditions. Moreover, as households who are more interested in the topic of electricity saving are likely to already undertake savings efforts to reduce consumption and are probably also more likely to sign up for the study than others.

and at the end of the baseline period via an online tool.[11] Households who successfully reported their electricity meter readings were randomly assigned to experimental groups. We stratified the random assignment by electricity consumption levels in 2015 and baseline period consumption levels between mid-October and mid-November 2016. We received valid meter readings from 1,636 households (4.7% of the households initially contacted) for the baseline period.

The treatment period (SAVE) was the period in which participants were encouraged to save electricity. We randomly assigned participants to four different experimental groups that were subject to different experimental conditions. We describe the experimental conditions in detail in Section 2.2 directly below. At the end of SAVE we administered a short questionnaire eliciting anecdotal evidence on how households tried to save electricity during the study, as well as collecting standard demographic variables. Additionally, households in the three experimental groups were informed whether they reached the goal.

To measure the persistence of our behavioral interventions, the trial ended with an untreated post intervention period (POST) from mid-December to mid-January 2017. Households who completed POST automatically took part in a lottery as a thank you for their participation. All participating households knew beforehand that the outcome of the lottery was independent of their saving effort and that every participating household was equally likely to win.

2.2 Experimental Treatments

We implemented four experimental groups including one control group. The goal of our experimental interventions was to strengthen participants' motivation to save electricity. The experimental interventions build on each other and increase in their behavioral strength (see Section 3 for a theoretical model that clarifies how our experimental conditions can have these effects). By comparing to a stringent control condition, we can identify how electricity saving goals and pro-environmental incentives need to be presented in order to make a significant difference for electricity saving behavior.

CONTROL Households in the control group received e-mails in which they were informed that they were part of a study on electricity saving and that they should try to reduce their electricity consumption. They also received weekly tips on how to save electricity.[12] Households in the control group and households in the experimental groups received the same electricity saving tips. Thus, we make sure that the effects we observe

[11] We specified a submission window for the readings to be submitted and sent out several reminders when readings were due. Roughly 85% of participating households provided the information in a range of +/- four days. In our analyses, we use mean electricity consumption per day as our dependent variable to account for potentially differing lengths of the experimental periods between participants.

[12] The texts of the e-mails and a complete list of the tips (each week three tips were provided for the total of four weeks in SAVE) can be found in section A2 and respectively section A3 in the Appendix.

are not driven by differences in information received by the households, but must be due to a stronger motivation to save electricity, triggered by our experimental manipulations.

GOAL Households in the *GOAL* group received the same information about the study and the same weekly tips on electricity saving behavior as households in CONTROL. In addition, they were given the goal to reduce their electricity consumption by 5% relative to their baseline level.[13] There were no consequences for households failing to reach that goal.

GAIN Households in the *GAIN* group received the same information and tips as in the previously described experimental group, and they were also given a 5% reduction goal as households in *GOAL*. In addition, the goal was coupled with a pro-environmental incentive in a gain frame format. Specifically, the utility promised to plant a tree if a household reached the 5% reduction goal.[14]

LOSS The *LOSS* group was identical to the *GAIN* group except that the pro-environmental incentive was presented in a loss frame. Households were informed that the utility would plant a tree because of their participation in the study but that the tree would *not* be planted if the household failed to reach the 5% saving goal during the SAVE period.

We illustrated the gain and the loss frame in the respective experimental groups with a small picture of a tree being planted (gain frame, see left-hand panel of Figure 1) and a tree being dug up (loss frame, see right-hand panel of Figure 1). The intention was to make the framing more salient and visually appealing in the e-mail received by the experimental group.

2.3 Discussion of the experimental design and empirical setting

We made several design choices which are specific to the empirical setting and warrant discussion.

First, we decided to invite households to participate in the study. This design choice was mainly motivated because of legal issues (German data protection laws require that households provide consent when their data are being analyzed). Thus, when policymakers

[13]We set the goal at 5% based on the results of previous studies (Abrahamse et al., 2007) and at a level that we hoped would be challenging but achievable (Beshears et al., 2016; Rogers et al., 2014).

[14]As electricity consumption generally increases due to seasonal effects from mid-October to mid-December (the time of the BASE and SAVE periods), we adjusted the 5% goal to these seasonal effects. Specifically, we used anonymized data from a group of households that did not partake in our trial, but had a smart meter transmitting daily electricity consumption levels. With the help of this data we determined that electricity consumption generally increased by 11.56% from BASE to SAVE. In order to attain the goal, households in the experimental conditions therefore needed to have an absolute excess consumption of less than +6.56% in SAVE compared to BASE. We did not inform the households about how the seasonal adjustment was carried out exactly, but only that the comparison would be “seasonally adjusted”.

Figure 1: Illustrations of gain (left) and loss (right) frame

Note.—Figure 1 shows the illustrations highlighting the framing of the gain and loss groups. The left-hand panel illustrates the gain frame (tree will be planted); the right-hand panel illustrates the loss frame (tree will *not* be planted).

in Germany and in the EU want to test and implement soft measures they often need to rely on voluntary participation of individuals. Our study therefore also samples self-selected households. However, many policy interventions can only be tested with such samples (see, e.g., Chen et al., 2017; Goette et al., 2019; Reichhuber et al., 2009; Tiefenbeck et al., 2016; Schultz et al., 2016; Zarghamee et al., 2017). In fact, in situations where informed consent for participation is required, some form of self-selection into the sample is inevitable. Nevertheless, when it comes to the analysis of the effectiveness of behavioral tools to internalize externalities, framed field experiments with such samples provide a meaningful testbed to provide first insights into behavior (List and Price, 2016). To account for the self-selection, however, we randomly allocated participating households to experimental conditions only after they signed up. Our experimental results are therefore unbiased given the sample population.

Our second design choice relates to the data collection. Only a limited number of customers of the utility have a smart meter installed in their homes. The majority of households at EnBW ODR self-report their annual meter data to the utility. Based on these self-reported meter readings, the annual electricity bills are calculated. Meters are only officially read by the utility when customers cancel their contracts or move house. In these cases, customers have to pay the difference/receive a refund in case these data were incorrectly reported. Thus, customers are used to self-reporting their electricity consumption data and have no incentive to under-report. Therefore, we decided to rely on self-reported data, too. This ensured that all households – those with a smart meter and those with conventional meters – were able to participate in the study. It further ensured that households were accustomed with the process of reporting the data and did not have to adjust to a new, unfamiliar procedure.

We thus faced a trade off between relying on the well established relationship between the utility provider and its customers and collecting the data by a third party. The latter, however, would have potentially jeopardized participation rates because meter readings are an intrusive procedure which requires additional effort for the participating households (e.g., appointments need to be made etc.). Yet, although self-reporting is common for customers of EnBW ODR, self-reported data are, however, not ideal. To account for this fact, we undertook several additional measures to i) incentivize honest reporting and, ii) verify correctness of the data entries.

To incentivize honest reporting, we told participating households that they are eligible for a prize if they provide four correct meter readings over the course of the study. The probability of winning did not depend on the consumption of a household and households with obviously incorrect meter readings were excluded from the raffle. Winning households were further likely to receive an official check by a representative of the utility at the end of the study. In addition, households needed to report their meter readings by use of the official tool of the utility provider. Households are accustomed to this tool because it is the same tool which they use to report their annual meter readings. Hence, under-reporting would not make sense from a monetary point of view because it would result in higher payments when re-adjusting for the annual bill. To verify correctness, we further checked whether the initial data submission was within a reasonable range of historically reported data, i.e., whether reported data are reasonable given the latest provided data to the utility. We excluded households with unreasonable baseline readings from the study. Nevertheless, although we have many reasons to believe that the data is reliable and represents true electricity consumption levels, we interpret our findings with caution.

As a third design choice we provided households with a savings goal of 5% relative to the baseline period in the *GOAL*, *GAIN* and *LOSS* group. We set the goal at 5% based on the results of previous studies (Abrahamse et al., 2007) and based on discussions with our field partner on what level would be challenging but achievable (Beshears et al., 2016; Rogers et al., 2014). However, we deliberately did not communicate the exact consumption level which resulted from a 5% saving on electricity and only informed households whether they reached their savings goal after the intervention phase. We did this for two reasons: First, we did not want to provide households with a concrete number which potentially creates some demand effects and may impact honest reporting of data. Second, as electricity consumption generally increases due to seasonal effects from mid-October to mid-December (the time of the BASE and SAVE periods), we adjusted the 5% goal to these seasonal effects. Specifically, we used anonymized data from a group of households that did not partake in our trial, but had a smart meter transmitting daily electricity consumption levels. With the help of this data we determined that electricity consumption generally increased by 11.56% from BASE to SAVE. In order to attain the goal, households in the experimental conditions therefore needed to have an absolute excess consumption of less than +6.56% in SAVE compared to BASE. As these seasonal effects are generally hard to predict, we did not inform the households about how the seasonal adjustment was carried out exactly, but only that the comparison would be "seasonally adjusted". Consequently, households did not know which level of reduction was necessary to achieve the savings goal. This, too, removed incentives to misreport consumption.

The fourth design choice we made was to provide an additional environmental incentive to save electricity rather than monetary incentives in the *GAIN* group and the *LOSS* group. We made this design choice because the effect of financial incentives on effort provision and motivation in goal setting environments and environments in which loss aversion plays a role has already been extensively studied (see, e.g., Fehr and Goette, 2007; Fryer Jr, 2011; Hossain and List, 2012; Imas et al., 2016; Karlan et al., 2016; Scott-Clayton, 2011). In addition, the literature on gift exchange documents that individuals also provide effort when they are rewarded with non-monetary incentives (see, e.g., Kube et al., 2012). Building on and combining existing findings from the literature in behavioral economics, our novel pro-environmental incentive intervention in the form of a tree to be planted advances the literature in environmental and behavioral economics in two ways. First, these pro-environmental incentives help curb emissions by motivating individuals to reduce electricity consumption. Second, trees as rewards for goal attainment benefit the environment by further contributing to reducing CO2 levels in the atmosphere. Given that a large share of people in the population is already motivated to reduce electricity consumption and do good for the environment, these incentives directly speak to individuals with pro-environmental preferences. While these people might also respond to financial incentives, testing how additional environmental incentives affect effort to reduce electricity consumption is important to know for researchers, policymakers and practitioners alike. This is especially the case since these additional pro-environmental incentives may be provided cost effectively (trees in our study were planted by EnBW and cost the utility provider about 5 Euro/tree).

3 Behavioral Predictions

We present a simple theoretical framework that allows deriving meaningful hypotheses for our experiment and that clarifies how our experimental interventions may affect the marginal utility from saving electricity. In the model utility depends on individuals' efforts to save electricity and on their reference points, which allows incorporating the differential framing of the pro-environmental incentives. Our framework is based on previous work on reference dependent preferences by Kőszegi and Rabin (2006) and Abeler et al. (2011). The model provides a simple extension in order to incorporate non-monetary, pro-environmental incentives.

In the control group participants neither receive an individual saving goal nor are they incentivized in any way to save electricity. Hence, subjects' utility in *CONTROL* solely depends on their own individually chosen effort to save electricity:

$$U^{CONTROL} = be - c(e) \tag{1}$$

where e is individual effort to save electricity and b describes the benefit from saving electricity (i.e., the amount of money saved by exerting electricity saving effort and the accompanying psychological benefits, such as feeling good when saving resources). For simplicity, we assume constant marginal utility of effort. $c(e)$ represent the cost of providing effort. We assume that the marginal cost of saving electricity is increasing and concave ($c'(e) > 0$ and $c''(e) < 0$).

In *GOAL* we provide households with an individual saving goal of 5%. In our model we assume that this goal constitutes a reference point. Subjects in this experimental group thus maximize:

$$U^{GOAL} = be + \Pi(e,x)\eta Y - (1 - \Pi(e,x))\eta\lambda Y - c(e) \tag{2}$$

where $\eta \geq 0$ is a parameter capturing the degree of reference dependence and $\lambda \geq 1$ captures loss aversion. $\Pi(e,x)$ is the probability of achieving the goal. This probability depends on effort provision e and the goal x. We introduce this element of uncertainty because, ex-ante, households do not know which effort level is required to reach the goal. With the counter-probability of $1 - \Pi(e,x)$ households do not reach the goal. We assume that the probability $\Pi(e,x)$ is increasing in e ($\frac{\partial\Pi}{\partial e} > 0$) and decreasing in x, the difficulty to reach the goal ($\frac{\partial\Pi}{\partial x} < 0$). Y is defined as the perceived utility gain (loss) from reaching (failing to reach) the saving goal ($Y \geq 0$).[15]

In *GAIN* households have an additional non-monetary, pro-environmental incentive in the form of a tree that is planted if they reach the saving goal. Importantly, the planting of the tree is framed in the gain domain, thus emphasizing that a tree will be planted if the household reaches the goal. The following utility function captures the potential additional utility from the pro-environmental incentive in this experimental group:

$$U^{GAIN} = be + \Pi(e,x)\eta(Y + T) - (1 - \Pi(e,x))\eta\lambda Y - c(e) \tag{3}$$

where parameter $T \geq 0$ captures the utility gain for the subject when the tree is planted as a consequence of reaching the saving goal.[16] As the pro-environmental incentive in form of the tree planting was framed in the gain domain in this experimental group, we assume that it enters only in the gain-related part of the reference-dependent utility function.

[15] If $Y = 0$, reaching the goal carries no value for the subject. We could also make Y depend on the extent of realized savings relative to the goal (e.g., $Y = x - e$ if $e < x$ and $Y = a(e - x)$ if $e > x$ with a capturing the importance of the goal for the subject). Predictions do not change and there are no additional insights gained by using this more complex version of the model, however.

[16] $T = 0$ captures the case in which the subject does not care about the tree.

In *LOSS* the pro-environmental incentive was framed in the loss domain and we made salient that failing to reach the saving goal would result in the loss of a tree that would have been planted otherwise. Therefore, following the same modeling approach as above, we incorporate the potential utility loss of the tree (T) in the loss-related part of the reference-dependent utility function in this experimental group. Participants thus maximize the following utility function:

$$U^{LOSS} = be + \Pi(e,x)\eta Y - (1-\Pi(e,x))\eta\lambda(Y+T) - c(e) \tag{4}$$

Deriving First Order Conditions (FOCs) for all experimental conditions yields:

$$\begin{aligned}
\frac{\partial U^{CONTROL}}{\partial e} &= b - c'(e) \\
\frac{\partial U^{GOAL}}{\partial e} &= b + \Pi'\eta Y + \Pi'\eta\lambda Y - c'(e) \\
\frac{\partial U^{GAIN}}{\partial e} &= b + \Pi'\eta(Y+T) + \Pi'\eta\lambda Y - c'(e) \\
\frac{\partial U^{LOSS}}{\partial e} &= b + \Pi'\eta Y + \Pi'\eta\lambda(Y+T) - c'(e)
\end{aligned} \tag{5}$$

Participants in the control group will provide effort to save electricity if the marginal benefit from saving is larger than the marginal cost of effort $\frac{\partial U^{CONTROL}}{\partial e} = b \geq c'(e)$. In *GOAL* the marginal benefit from providing electricity saving effort increases compared to the control group, if $Y > 0$, because of the potential additional satisfaction of reaching the goal, which figures as the reference point. Likewise, the fear of not meeting the savings goal and experiencing a loss with respect to the reference point motivates participants who place value on reaching the goal ($\frac{\partial U^{GOAL}}{\partial e} = b + \Pi'\eta(Y) + \Pi'\eta\lambda(Y) \geq c'(e)$). In *GAIN* the marginal benefit from providing effort increases further, if $T > 0$, because of the additional incentive that a tree will be planted if a household reaches the goal ($\frac{\partial U^{GAIN}}{\partial e} = b + \Pi'\eta(Y+T) + \Pi'\eta\lambda(Y) \geq c'(e)$). Finally, because of loss aversion, i.e., $\lambda > 1$, the marginal benefit from providing effort to save electricity increases even further in *LOSS* as $\frac{\partial U^{LOSS}}{\partial e} = b + \Pi'\eta(Y) + \Pi'\eta\lambda(Y+T) \geq c'(e)$.

This straightforward theoretical rationalization of our experiment thus allows deriving predictions for our experimental effects, as a function of the parameter values η (the degree of reference dependence), λ (the degree of loss aversion), Y (the value of reaching the saving goal), and T (the value of the pro-environmental incentive in the form of a tree). The FOCs show that in order to observe any differences between our experimental groups, we need at least some reference dependence ($\eta > 0$). In addition to that, assuming positive values for the other parameters (i.e., $Y > 0$, $T > 0$, $\lambda > 0$) leads us to predict the following ranking of our experimental groups in terms of achievement in electricity savings: $LOSS > GAIN > GOAL > CONTROL$.

4 Results

Table 1 presents summary statistics on the number of households and their mean electricity consumption (in kilowatt hours per day) in each of the four experimental conditions and across the three periods of the trial.[17]

Table 1: Summary statistics: Mean daily electricity consumption

		CONTROL	*GOAL*	*GAIN*	*LOSS*	Total
BASE	mean	10.707	10.758	10.912	10.773	10.787
	(sd)	(5.812)	(5.319)	(6.076)	(5.663)	(5.717)
	n	412	414	402	408	1636
SAVE	mean	12.072	11.893	11.813	11.471	11.815
	(sd)	(6.879)	(6.466)	(6.328)	(5.985)	(6.423)
	n	374	372	364	362	1472
POST	mean	12.785	12.867	12.622	12.230	12.630
	(sd)	(7.388)	(7.343)	(7.731)	(6.578)	(7.267)
	n	348	341	324	332	1345

Note: Mean electricity consumption per day across conditions and trial periods. Standard deviations in parentheses. BASE period went from mid-October to mid-November 2016 and was untreated. SAVE went from mid-November to mid-December 2016 and was the treatment period. POST went from mid-December 2016 to mid-January 2017 and was the untreated post-intervention period.

The row 'BASE mean' shows average daily electricity consumption in the baseline period *before* mailings were sent to households in the different experimental groups. The randomization process was successful, as households did not differ in electricity consumption between experimental groups in the baseline period ($p > .10$ for all group comparisons according to t-tests).[18] Across all conditions, average consumption in the BASE period amounted to 10.79 kWh/day (see rightmost column of Table 1).[19]

The data in Table 1 document an increase of electricity consumption between the three experimental phases across all experimental groups. Electricity consumption increased because of the seasonal change from autumn to winter between the first data collection in mid-October to the last data collection in mid-January.[20] The increase in electricity use

[17] Table 1 presents the final data set after data cleaning but before outlier correction. We base our main analysis on this dataset as well as on an outlier corrected dataset. In Appendix A1 we provide summary statistics for the outlier corrected dataset and detailed information on how we proceeded for data cleaning and outlier identification (using the procedure outlined by Bruffaerts et al., 2014).

[18] All p-values reported in this paper are for two-sided tests.

[19] For comparison, average consumption per household in Germany was 9.05 kWh/day in 2013 (WEC, 2016).

[20] Although in Germany (and most EU countries) only few households use electricity for heating purposes, electricity consumption increases during the autumn and winter months. Electricity is mainly used for lighting, plug-in devices, cooking and sometimes water heating. However, as households spend more time at home during the winter and because it gets dark earlier in the day, electricity consumption increases substantially.

was, however, more pronounced in the control group compared to our experimental groups with the saving goal and the pro-environmental incentives. In fact, as predicted in Section 3, the results in Table 1 indicate that the households in the *LOSS* group were the most motivated to save electricity and the households in the *CONTROL* group were the least motivated. The lowest increase in electricity consumption took place in the *LOSS* group and the highest in the *CONTROL* group. In the control group, households used 12.7% more electricity in the SAVE phase compared to the BASE phase. In *GOAL*, the increase in consumption was less pronounced. Households used 10.6% more electricity in the SAVE phase compared to the BASE phase. When moving to the *GAIN* group, we observe an even lower increase in electricity consumption. Households increased their consumption by 8.3% in the SAVE phase. In the *LOSS* group, the increase in consumption was the smallest. Households only used 6.5% more electricity in the SAVE phase compared to the BASE phase. When comparing the consumption during SAVE, the *LOSS* group led to an average electricity saving of 5.0% compared to the control group, and the *GAIN* group to a saving of 2.1%.

Table 2 presents statistical evidence in the form of regression results. The table presents results from random effects (models (1) and (2)) and household fixed effects (models (3) and (4)) panel GLS regressions with daily average electricity consumption as the dependent variable. Models (1) and (3) limit regressions to the outlier-corrected dataset. In regressions (2) and (4) we use the full dataset for our analysis. In all regression models standard errors are clustered at the household level.

The non-significant coefficients for the (non-interacted) dummies representing the experimental groups in models (1) and (2) show that electricity consumption did not differ between our experimental groups and the control group in the BASE period. Additional post-estimation Wald-tests indicate that there are also no significant differences in BASE consumption between any of the experimental groups ($p > .10$ for all possible comparisons). The regressions further highlight significant effects of our experimental interventions in the treatment phase (SAVE). We observe that participants in the *GOAL* group decreased electricity consumption in the SAVE period, however the effect was not statistically significant, as indicated by the non-significant coefficients for 'SAVE $\times$ *GOAL*'. Providing a gain-framed pro-environmental incentive linked to the goal as in *GAIN* led to an even stronger reduction in consumption, which was statistically significant (see coefficients for 'SAVE $\times$ *GAIN*'). Using a loss-framed pro-environmental incentive as in *LOSS* further reduced electricity consumption in SAVE (see coefficients for 'SAVE $\times$ *LOSS*'). On average, households in the *LOSS* group saved an additional 0.48 kWh per day (0.54 kWh/day when considering the full dataset in model (2)).[21] We summarize these findings

[21] For the SAVE phase, post-estimation Wald tests based on the regressions reported in Table 2 show that the contrast between *GOAL* and *LOSS* was marginally significant in models (2) and (4) ($p = .06$ in both models). All other differences between the experimental groups *GOAL*, *GAIN*, and *LOSS* were not statistically significant ($p > .10$).

in Result 1:

Result 1. *The combination of a saving goal and pro-environmental incentives led to a significant reduction of self-reported electricity consumption in the intervention period. The loss-framed pro-environmental incentive thereby led to the highest reduction.*

Directionally, the results from the SAVE phase are in line with the behavioral predictions of our theoretical model presented in Section 3. We found the highest savings in *LOSS*, followed by *GAIN*, and *GOAL*.

To shed some light on the persistence of the effects of our experimental interventions across time, we included a post-intervention period in our study. The Table 2 results show that the effect of the *GOAL* group seems to have disappeared fully in the POST period (see coefficients for 'POST × *GOAL*'), whereas we still see a directional effect in the *GAIN* group ('POST × *GAIN*') and a marginally significant effect for households in the *LOSS* group ('POST × *LOSS*') when compared to the control group and restricting observations to the outlier corrected dataset.[22] The (seasonally induced) increase in electricity consumption in the POST phase was smaller in *LOSS* compared to the other experimental groups (see Table 1). Relative to the BASE phase, consumption increased by only 13.5% (1.46 kWh/day) in *LOSS*. In the other experimental groups electricity use in the POST period (compared to BASE) increased by 15.7% (1.71 kWh; *GAIN*), 19.6% (2.11 kWh; *GOAL*), and 19.4% (2.08 kWh; *CONTROL*). Thus, the effects of a combination of saving goal and a loss-framed pro-environmental incentive persisted and also led to a reduction of electricity consumption in the non-targeted post-intervention period.[23]

Figure 2 summarizes our main findings by graphically presenting the difference-in-difference results from regression Table 2 for periods SAVE and POST relative to BASE. The figure shows the average increase in consumption compared to the BASE period for all of our experimental conditions and thus allows comparing the differences in consumption increases from BASE to SAVE and from BASE to POST between experimental groups. The left-hand side of the figure shows the change in electricity consumption levels in the SAVE period relative to BASE. The right-hand side of the figure shows the change in electricity consumption levels in the POST period relative to BASE. The figure again highlights that the households in *LOSS* had the lowest increase in consumption in the SAVE period, indicating that they were the most motivated to save electricity, and that this effect persisted into the POST period. However, the figure also illustrates that the difference between *GAIN* and *LOSS* that was marginally significant in the SAVE period,

[22] For the POST phase, post-estimation Wald tests based on the regressions reported in models (1) and (2) in Table 2 show that the following differences between the experimental groups *GOAL*, *GAIN*, and *LOSS* were at least marginally statistically significant: *GOAL* vs *GAIN* ($p = .07$ in models (1) and (3) and $p = .09$ in model (4)) and *GOAL* vs *LOSS* ($p = .03$ in model (1), $p = .02$ in model (2), $p = .04$ in model (3) and $p = .02$ in model (4)). All other contrasts were not statistically significant ($p > .10$).

[23] Note, however, that the post-intervention period is short and we are cautions when quantifying to what extend savings persisted.

Table 2: Panel Regression Results

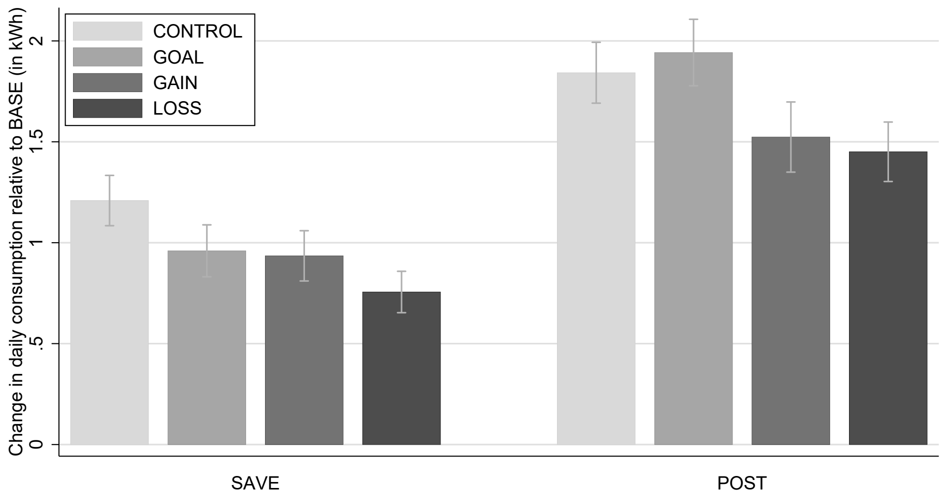
	(1) w/o outliers	(2) all data	(3) w/o outliers	(4) all data
SAVE	1.220***	1.254***	1.209***	1.237***
	(0.125)	(0.195)	(0.125)	(0.195)
POST	1.873***	1.945***	1.860***	1.927***
	(0.151)	(0.198)	(0.150)	(0.197)
GOAL	0.178	0.051		
	(0.333)	(0.388)		
GAIN	0.117	0.204		
	(0.334)	(0.417)		
LOSS	0.066	0.066		
	(0.336)	(0.401)		
SAVE × *GOAL*	−0.278	−0.149	−0.274	−0.135
	(0.179)	(0.251)	(0.178)	(0.251)
SAVE × *GAIN*	−0.354**	−0.450*	−0.347**	−0.447*
	(0.172)	(0.254)	(0.171)	(0.254)
SAVE × *LOSS*	−0.482***	−0.541**	−0.471***	−0.522**
	(0.161)	(0.237)	(0.160)	(0.237)
POST × *GOAL*	0.089	0.157	0.099	0.174
	(0.223)	(0.271)	(0.221)	(0.269)
POST × *GAIN*	−0.338	−0.322	−0.331	−0.317
	(0.228)	(0.304)	(0.227)	(0.302)
POST × *LOSS*	−0.384*	−0.403	−0.363*	−0.377
	(0.211)	(0.257)	(0.209)	(0.255)
Constant	10.363***	10.707***	10.483***	10.821***
	(0.238)	(0.286)	(0.039)	(0.051)
Household Fixed Effects	No	No	Yes	Yes
R^2	0.019	0.014	0.932	0.922
Observations	4341	4453	4341	4453
Households	1595	1636	1595	1636

Note: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the average daily electricity consumption in kWh. Standard errors, clustered by household, are in parentheses. Models (1) and (2) are random effects panel regressions, models (3) and (4) are fixed effects panel regressions. Models (1) and (3) exclude outliers (see Appendix A1 for details), models (2) and (4) include all available data.

disappeared in the POST period when the pro-environmental incentive in form of the tree was not present anymore.

Our second result concerns the attrition rates across experimental groups and periods. We discussed in the introduction that previous literature suggests that people dislike and

Figure 2: Difference-in-difference: change in electricity consumption relative to BASE



Note: The Figure shows the difference-in-difference results for the trial periods SAVE and POST relative to BASE. Results for period SAVE are in the left half of the figure. Results for period POST are in the right half of the figure. Note that absolute consumption levels increased during the time of the trial because of seasonal effects. Error-bars denote plus/minus one standard error of the mean.

therefore avoid loss frames (e.g. Imas et al., 2016), which may be a reason why they are so rarely used by policy makers. In order to test this conjecture, we examine the drop-out rates of households during our trial. Of the 1,636 households in our final data-set who entered their electricity consumption in mid-October and mid-November (BASE) and subsequently received a treatment mailing from the utility, 164 households (10.0%) dropped out in total during the experimental treatment phase (SAVE). In *CONTROL*, 9.2% (38 households) did not enter their electricity consumption a third time and dropped out during SAVE, compared to 10.1% (42) in *GOAL*, 9.5% (38) in *GAIN*, and 11.3% (46) in *LOSS*. Whereas the attrition was thus directionally largest in the *LOSS* group, none of the differences in attrition between the experimental conditions during the SAVE period are statistically significant ($p > .10$ for all comparisons according to proportion tests).[24] In the post-intervention period (POST), 6.3% (26 households) dropped out in *CONTROL*, compared to 7.5% (31) in *GOAL*, 10.0% (40) in *GAIN*, and 7.4% (30) in *LOSS*. Only the difference between *CONTROL* and *GAIN* is statistically significant (proportion test: $z = 2.02$, $p = .04$). None of the other differences between conditions are statistically significant ($p > .10$ according to proportion tests).[25]

Given existing evidence suggesting that people dislike and therefore avoid loss-frames,

[24]There are also no statistically significant differences between the conditions in terms of average BASE consumption of households who dropped out of the study during SAVE, and there are no significant differences between conditions when comparing BASE consumption only for households who remained in the study.

[25]The results comparing attrition rates between experimental conditions are qualitatively (i.e., in terms of statistical significance) the same when basing the tests on the uncleaned data-set, i.e., also including observations that had to be excluded from the main analysis for some reason (see Appendix A1).

it is noteworthy that we did not experience significant differences in attrition between the *GAIN* and the *LOSS* group ($z = 0.85$, $p = .40$) during the SAVE period, i.e., after the households had been assigned to the experimental groups. There was also no difference between these two experimental groups in terms of attrition in the final POST period of the study ($z = 1.30$, $p = .19$). There is thus no indication in our data that households avoided the loss-framed pro-environmental incentive by ending their participation in the study. We summarize this finding in Result 2:

Result 2. *Participants did not avoid the loss frame. The loss framed pro-environmental incentive did not lead to more attrition than the gain framed incentive.*

5 Conclusion and Policy Implications

This paper shows how non-monetary pro-environmental incentives can be used to motivate households' electricity saving behavior. We report findings from a framed field experiment in which we matched a household's pro-environmental deed (the reduction of electricity consumption) with a further pro-environmental act (the planting of a tree). The matched incentives were presented in different frames, thereby allowing us to investigate the effectiveness of gain and loss-framed pro-environmental incentives on electricity saving behavior. As theoretically predicted, we find that pro-environmental incentives are effective in reducing electricity consumption. The loss-framed pro-environmental incentive is thereby the most successful tool in reducing electricity consumption in our experiment when compared to a control group. The effects of the loss-framed pro-environmental incentive also carry over into a non-treated post-intervention period of one month after the experiment.

Our results add two noteworthy facets to the political discourse on using behavioral interventions to steer people's decision making in (energy) markets. First, despite the results of Andor et al. (2017) indicating that behavioral interventions may prove less effective when electricity consumption levels are already relatively low, our study illustrates that meaningful effect sizes can be achieved with the help of differentially framed non-monetary incentives. Second, currently policy makers seem to be cautious about using loss-framed incentives in the environmental domain due to the fear that people could dislike or avoid such interventions. Our results show that at least for subtle loss frames like the one used in our study this fear seems to be unsubstantiated, as we do not observe higher attrition rates from participants who were randomly assigned to the loss frame (see also Imas et al., 2016; De Quidt, 2017).

However, there are also moral and ethical considerations involved when applying behavioral interventions and especially when using loss frames. Behavioral interventions are delicate per-se as, by definition, they attempt to trigger behavioral change. Typical nudges, for instance, involve telling people how the majority does something (e.g., Goldstein et al., 2008), how their behavior compares to their peers' (e.g., Allcott, 2011; Costa and Kahn, 2013), what they should do according to moral standards (e.g., De Groot et al., 2013), or consist of a change of default rules (e.g., Johnson and Goldstein, 2003). Loss-framed incentives as behavioral interventions may be especially sensitive because they deliberately trigger people's loss aversion by giving the impression that people already achieved or possess something before they actually do. Telling some people that their efforts might not have been sufficient and therefore the incentive is "lost" may result in negative reactions that have personal psychological costs for individuals and that might trigger opposition to such incentive schemes. If, however, the majority of people can be motivated to change certain behaviors by loss framed incentives—and can be motivated to do so better than by other means—the question arises whether the overall positive effects outweigh backfiring of loss framed interventions for some individuals? From a purely economic perspective, this is subject to a simple cost-benefit analysis. From a moral perspective, this is not so clear.

In this paper we did not focus on the discussion about the ethics and the morality of loss-framed intervention nudges, and instead concentrated on demonstrating their effectiveness in a specific setting. We do believe that the loss-framed incentive we used in our study, while effective, did not trigger too severe psychological reactions, as it was a relatively low-powered incentive. We would also like to point out that in order to ensure that our experimental setup was appropriate from an ethical point of view, we obtained IRB ethics approval from the ETH Zurich ethics committee before conducting the study. However, there may be areas and specific implementations of loss-framed incentives that are indeed problematic. Thus, while our findings may encourage a more frequent use of behavioral interventions appealing to people's loss aversion, it is crucial that the potential costs and benefits of such interventions are carefully assessed before each implementation, and that moral and ethical considerations are taken into account.

Several important questions remain. First, the results from our post-intervention period indicate that the consumption reducing effect of the loss-framed pro-environmental incentive showed some temporal persistence, it is, however, unclear whether the effect would persist also in a longer time-horizon that we were not able to cover in our study. Moreover, it is also unclear how the effects would develop if our intervention was to be repeated. Would people stop caring about the pro-environmental incentive and would the effects thus wear off or would the effects grow stronger because of habituation? Although Allcott and Rogers (2014) show that effects from behavioral interventions may be persistent over long time periods and that individuals still respond to repeated experimental interventions, future research using smart-meter data may provide valuable insights whether these effects can be supported for the use of loss framed incentives, too.

Second, whereas we find no indication that the participants in our field experiment actively avoided the loss-frame, this may be due to the relatively subtle implementation of the framing and the low-powered incentive. When the framing or the incentives are stronger (which could be desirable to achieve even stronger effects of experimental interventions), it is unclear whether the same result would still hold or whether negative effects in terms of attrition would be the consequence. For instance, it would be interesting to couple the loss-frame with a monetary incentive in the environmental domain. In general, a better understanding of the boundary conditions of when loss frames should and should not be used would be desirable to further enable their application in public policy. Our study was just a first step in this direction.

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Appendix

A1 Data Cleaning and Outlier Detection

A1.1 Data cleaning

1,845 households reported their electricity meter reading a first time in mid-October and a second time in mid-November, thus allowing us to calculate a value for their BASE consumption. Of those, 1,675 entered their meter reading again in mid-December (allowing us to calculate a value for SAVE consumption) and 1'532 did so in mid-January (allowing us to calculate a value for POST consumption).

From these raw data we had to exclude a number of observations from the analysis because of four sources of bias:

- 11 observations in BASE, 5 in SAVE and 2 in POST showed negative consumption values, likely caused by erroneous meter reports.
- For 48 participants, there was an obvious system error in the electricity meter reading date in December. In the data we received on December 22, 2016, the date of their meter reading was recorded as December 31, 2016. Since it was not possible to recover the actual date of the meter reading, we had to exclude these participants because we were unable to determine the exact time span of their recorded consumption.
- For 158 participants, we discovered an anomaly indicating another system error in the recording of the electricity meter reading dates. All four dates of the meter readings were identical for these participants: 10/20/2016, 11/15/2016, 12/31/2016, and 01/23/2017. It is highly unlikely that this is due to chance, particularly because these dates have no systematic correlation with the dates on which requests and reminders to read the electricity meter were sent. Therefore, it seems likely that the recorded dates are incorrect, which would bias the calculation of daily electricity consumption in each period, which is why these participants were excluded.
- One study participant mentioned in the questionnaire that a severe case of water damage during the study period rendered all energy saving efforts moot.

In total, this led us to exclude 209 observations for BASE (leaving us with a final sample of 1,636 observations), 203 observations in SAVE (final sample of 1,472), and 187 observations in POST (final sample of 1,345).[26] Importantly, the number of excluded cases does not differ significantly by experimental condition ($p > .10$ for all comparisons).

A1.2 Outlier identification

Apart from the cases described above, which we needed to exclude from the analysis because of objectively identifiable and problematic exogenous events, our electricity consumption data are prone to measurement error because respondents entered the meter-readings themselves. Entry errors could lead to erroneously low or high electricity consumption values, which could bias the regression results. We therefore identified outliers in the consumption data and provided the main regression analyses (reported in Table 2)

[26] Note that some observations met several of the exclusion criteria.

also for an outlier-corrected data-set. Below we describe how we identified the outliers for this analysis.

As our consumption-data are right-skewed (see Figure A1), typical approaches like the standard boxplot method (i.e., identifying data points that are more than 1.5 interquartile ranges below the first or above the third quartile as outliers) would lead to too many outlier exclusions. We therefore relied on the method by Bruffaerts et al. (2014) who developed a generalized boxplot that is appropriate for skewed data. We used this method to identify outliers in the consumption data in all three periods separately. In the BASE period, we identify 29 outliers, in the SAVE period, we identify another 17 outliers, and in POST, we identify an additional 16 outliers. Observations that are identified as outliers in any of the three periods are excluded from the outlier-corrected analyses reported in models (1) and (3) of Table 2. Table A1 below presents the summary statistics table after outlier correction. Table 2 highlights that results are robust to including and excluding outliers. Figure A2 provides a graphical representation of effects after correcting for outliers.

Table A1: Raw data, mean daily electricity consumption without outliers

		CONTROL	*GOAL*	*GAIN*	*LOSS*	Total
BASE	mean	10.363	10.541	10.480	10.429	10.453
	(sd)	(4.790)	(4.692)	(4.613)	(4.709)	(4.698)
	n	405	405	390	395	1595
SAVE	mean	11.666	11.544	11.381	11.169	11.444
	(sd)	(5.703)	(5.573)	(5.336)	(4.965)	(5.405)
	n	367	364	353	350	1434
POST	mean	12.371	12.526	12.105	11.820	12.212
	(sd)	(6.174)	(6.426)	(5.864)	(5.424)	(5.991)
	n	342	334	316	320	1312

Note: Mean electricity consumption per day across conditions and trial periods. Standard deviations in parentheses. BASE period went from mid-October to mid-November 2016 and was untreated. SAVE went from mid-November to mid-December 2016 and was the treatment period. POST went from mid-December 2016 to mid-January 2017 and was the untreated post-intervention period.

Figure A1: Distribution of consumption in different periods

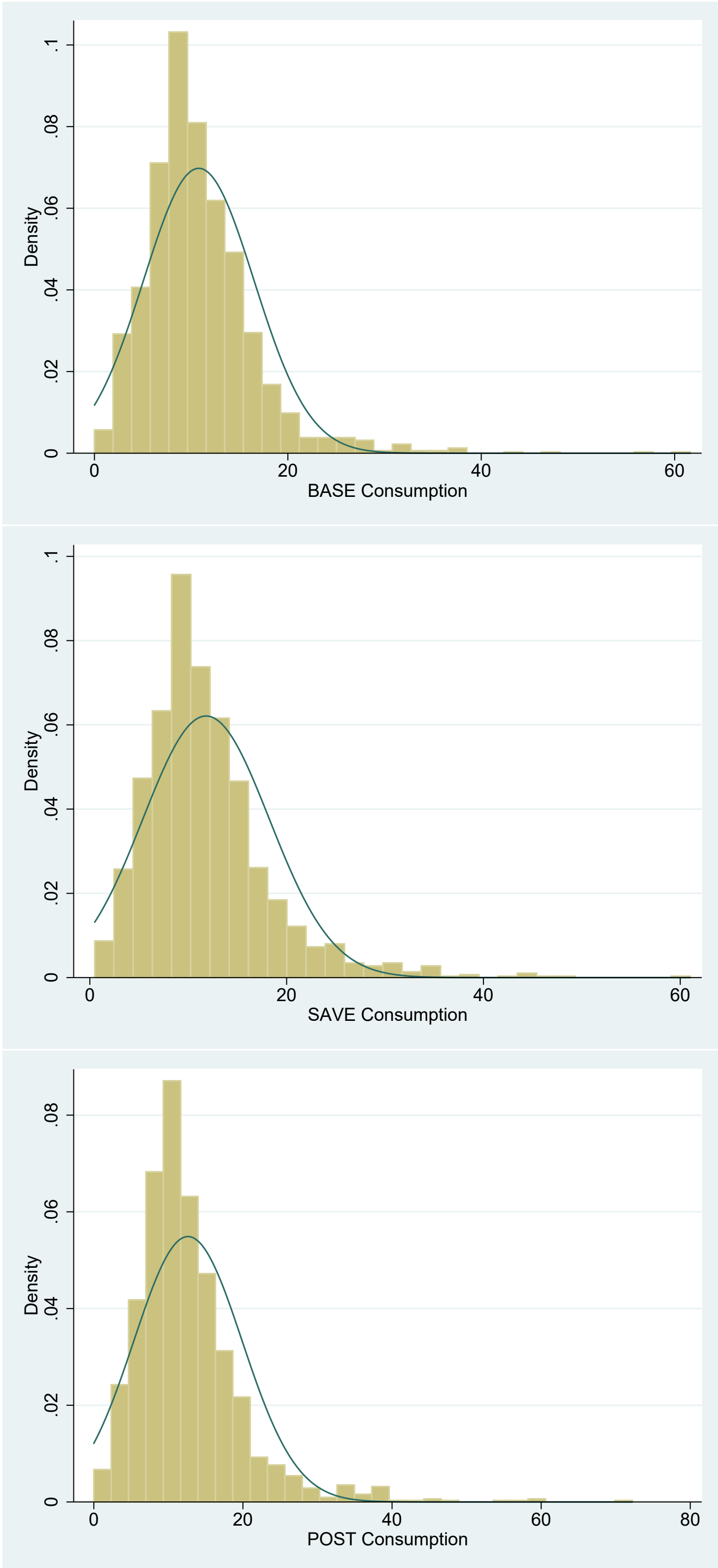
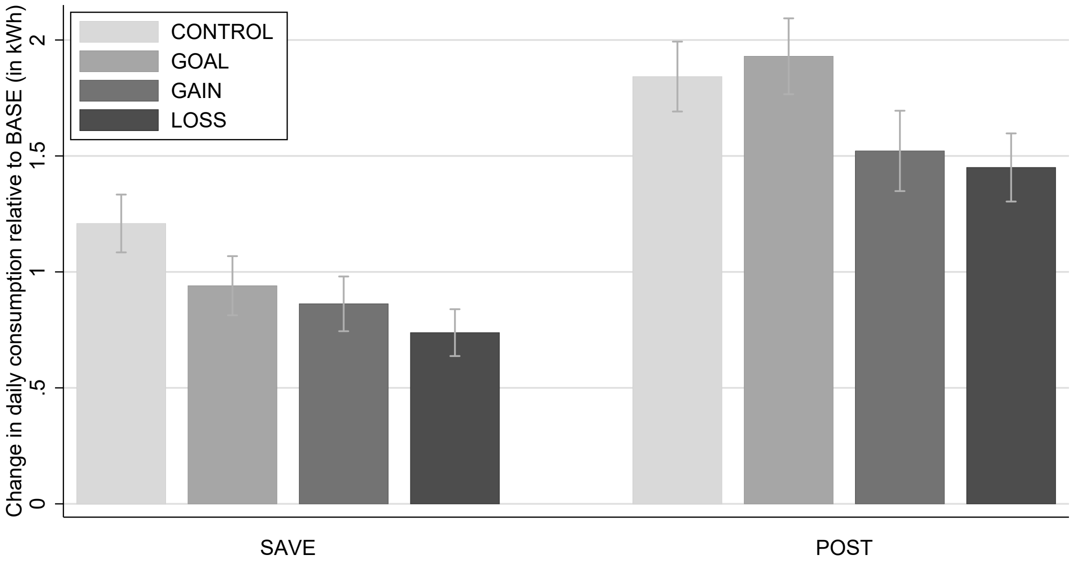


Figure A2: Difference-in-difference: change in electricity consumption relative to BASE



Note: The Figure shows the difference-in-difference results for the trial periods SAVE and POST relative to BASE. Note that absolute consumption levels increased during the trial from BASE to SAVE to POST because of seasonal effects (seasonal change from autumn to winter). Thus, the lower the bars, the lower the consumption increase relative to BASE and the higher the electricity saving. Error-bars denote plus/minus one standard error of the mean.

A2 E-Mail texts in SAVE

Dear customer,

Thank you for participating in our study.

We are very happy that you have provided your electricity meter readings once more. We would like to encourage you to reduce your electricity consumption in the following month. Reducing your consumption does not only save money, it is also beneficial for the environment. In order to support your efforts you will receive weekly tips on how to conserve electricity in your home.

GOAL *During the next 30 days you should try to lower your electricity consumption by 5% as compared to the previous month. At the end of the month you will be notified whether you have achieved this goal (the goal achievement will be seasonally corrected).*

GAIN *During the next 30 days you should try to lower your electricity consumption by 5% as compared to the previous month. At the end of the month you will be notified whether you have achieved this goal (the goal achievement will be seasonally corrected).*[27]

As a special thank you for your efforts towards regional climate protection, we will plant a tree on behalf of your name, if you reach the 5% saving target.

LOSS *During the next 30 days you should try to lower your electricity consumption by 5% as compared to the previous month. At the end of the month you will be notified whether you have achieved this goal (the goal achievement will be seasonally corrected).*[28]

As a special thank you for your efforts towards regional climate protection, we plan to plant a tree on behalf of your name. However, if you fail to reach the 5% saving target, the tree will not be planted.

Rest assured that your efforts do not influence your chances for the final prize draw of this study in the end of January.

Thank you!

[27] Treatment GAIN was supplemented by a graphical illustration, see Figure 1.
[28] Treatment LOSS was supplemented by a graphical illustration, see Figure 1.

A3 Electricity saving tips in SAVE

Three tips per week (a total of twelve tips) were distributed via e-mail to all participants of the study. Tips included a brief description on how to implement them in a household.

Tip 1: Save cash with LED
You are using a normal halogen lamp? If you replace 5 halogen lamps with LED lamps you cannot only save up to **163 kilowatt-hours** but also cash.

Tip 2: Proper use of Internet modem and router
Did you know that there are WLAN-router that use as much power as a refrigerator? With regard to energy saving it is therefore important that such devices are not overlooked. **Prevent wasting energy** by switching off your modem and router.

Tip 3: Maintain your refrigerators and freezers
The shorter the door remains open the less cold can escape. Check whether your **rubber seals** are intact and only put cooled and covered food in the refrigerator! Also, occasionally defrosting the refrigerator saves valuable energy.

Tip 4: Properly switch off your electronic devices!
Did you know that computers, TVs and coffee machines also use energy even if they are on stand-by or in sleep mode? To properly switch off your devices fast and easy you can **connect several devices to one socket strip**. All you need is a single button-press for all your devices.

Tip 5: Adjust your refrigerator correctly
The cooler the better? By no means. The cooler you set your devices the more energy they need to maintain this temperature. For efficient cooling we suggest a storage temperature of $5-7°C$ for refrigerators and a temperature of $-18°C$ for freezers. Increasing the temperature by one degree increases your electricity cost by 6% thereby costs you money.

Tip 6: Energy waster washing machine
The general principle: If you use the washing machine according to the program you wash efficiently. This means, for lightly soiled laundry 40°C instead of 60°C are sufficient. Washing the laundry at a lower wash cycle **reduces usage by 40%**. New wash cycles laundering at 20°C save up to 80% of energy compared to the 60°C-program.

Tip 7: Candle light instead of LED or luminaires
Especially during Advent time we like to illuminate our surroundings with many colorful lights. Whether chains of light or a shinning reindeer, **Christmas decoration is a hidden energy guzzler.** How about using candles instead of electric lamps? But be careful, a small flame can quickly grow larger. If you still want to save energy with luminaires, use **time switches for your chains of light**, to only activate them at the desired time.

Tip 8: LED-chains of light with certification mark
You like it cosy during Christmas time? If you do not want any unpleasant surprises you should use chains of light that have a GS-certification mark. Besides that, you can further reduce cost by using **chains of light with light-emitting diode (LED)** instead of light bulbs. The LED-alternative is not only more durable but also allows you to save energy and cash!

Tip 9: Baking cookies with savvy
Already baked the first cookies and preheated the oven for that? With most recipes you can save energy by forgoing the preheating. In this way **you can save up to 20 percent of electricity** and your cookies taste just as well. You can also **turn off the oven already five to ten minutes before the end of the baking time** and complete the bake with the residual heat. This applies not only to cookies but to all kind of oven dishes!

Tip 10: Showering smart
Using less warm water saves energy! So instead of taking a bath just take a quick shower. Thereby you can even save more energy by using a **low-flow showerhead**. Without sacrificing comfort you can save money. As various other products, you can also find low-flow showerheads in our online shop, to which the link can be found below.

Tip 11: Program-wonder dishwasher
Use your dishwasher, because fully loaded it uses less water than washing dishes by hand! In case of very dirty dishes you can set a higher temperature for the dishwasher and forgo the pre-rinsing. **In order to save energy in the long term you should rinse as often as possible at 50-55°C**. If you further don't pre-rinse and use **the short or saving program** you have still clean dishes in the end but you could save **10-15% of electricity.**

Tip 12: Each pot with lid saves energy
Always use a lid when cooking! In this way **you can save up to 60% of energy** and keep your energy cost low! Also the heat conductivity of your pans and pots influences your power consumption. The ideal pot matches the diameter of the hotplate and has an even ground. With that, optimal heat transfer is guaranteed. Also with the amount you are heating up in the pan you can save, because the less water is heated the less energy is needed.