Can Self-Set Goals Encourage Resource Conservation? Field Experimental Evidence from a Smartphone App

Andreas Löschel[†], Matthias Rodemeier[‡], Madeline Werthschulte[*]

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Abstract

This study leverages a large RCT to examine the potential of goal-setting nudges for resource conservation at scale. We randomize a feature that allows subjects to set themselves energy consumption targets in a popular smartphone app. We document negative effects of the nudge on app utilization and estimate null effects on energy consumption with confidence intervals that rule out estimates from observational studies. A complementary survey identifies the mechanisms underlying these behavioral responses. Using a structural model and random variation of the app's price, we estimate that the average user is willing to pay 7.41 EUR to *avoid* the nudge.

JEL Classification: C93, D91, Q4

Keywords: Nudging, goal setting, field experiments, scalability, energy, behavioral welfare economics, smartphones

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[†] Löschel: Ruhr-University Bochum, CESifo, and RWI; andreas.loeschel@ruhr-uni-bochum.de.

[‡] Rodemeier: Bocconi University, University of Chicago, and IZA; matthias.rodemeier@unibocconi.it.

[*] Werthschulte: ZEW - Leibniz Centre for European Economic Research Mannheim; werthschulte@zew.de.

1 Introduction

Policymakers increasingly rely on tools that leverage insights from psychology and behavioral economics to affect individual choices. These mostly non-pecuniary incentives, referred to as "nudges," simplify choice environments and help individuals to implement privately or socially desirable actions. One of the most important fields of policy intervention is resource conservation: Nudges are frequently used as complimentary tools to more classic interventions, such as Pigouvian taxes or legal mandates.

Our study is the first to test in a field experiment the effectiveness of a goal setting prompt —an ex-ante promising nudge—in encouraging resource conservation. In particular, we test whether self-set energy savings goals reduce household electricity consumption. The design of the nudge is motivated by a literature showing that prompting people to set themselves goals helps them follow through with desired behavior in other areas than environmental policy. It is therefore a natural step to analyze the role of these promising interventions in encouraging resource conservation and combating climate change.

To take the intervention to scale, we cooperate with a large public utility and a specialized IT company and develop an energy savings app accessible to the majority of the German population. Within this app, we randomize a goal-setting feature that prompts users to set themselves energy consumption targets for the upcoming month. In addition, we randomize a financial incentive that allows us to monetize the effect of the goal-setting prompt on consumer welfare. We then promote the rollout of the developed technology through a mass-marketing campaign and a set of sizable financial incentives. To maximize the chances of successful technology diffusion, we rely on established industry experts in creating and promoting the mobile app.

We observe behavior over a total period of seven months, with the following main results. First, we find a precisely estimated null effect of the goal-setting prompt on electricity consumption across all observed periods. The nudge does not affect behavior, although users set themselves meaningful goals that are highly predictive of future consumption. Poor targeting properties of the app might explain the null effect of the nudge. As a complementary survey shows, marginal consumers are characterized by an already low baseline energy consumption and high levels of energy-related knowledge. Further, the average marginal consumer is neither present-focused nor loss averse, two features commonly used in the theoretical literature to explain how goal setting affects behavior (Koch and Nafziger 2011, Hsiaw 2013).

Second, we find that the goal-setting nudge causes disutility to consumers, as it significantly reduces app utilization. In order to quantify the loss in welfare to consumers, we randomize a subsidy that incentivizes app utilization. We then leverage this exogenous price variation together with a simple structural model, and estimate that consumers are willing to give up 7.41 EUR to avoid the nudge. These results cast doubt on the prospects of mobile technologies as cost-effective scaling devices for behaviorally-motivated energy policies.

Our study makes three main contributions. It is the first study that uses randomly assigned goal-setting prompts to evaluate its causal effect on energy conservation. Based on the literature, prompting people to make plans and set goals has shown to help them reduce smoking (Armitage and Arden 2008), eat healthier (Achtziger, Gollwitzer, and Sheeran 2008), get vaccinated (Milkman et al. 2011), and vote during elections (Nickerson and Rogers 2010).[1] An overview article of the effectiveness of plan-making by Rogers et al. (2013) concludes that goal-setting prompts should play a larger role in public policy. We, therefore, investigate the potential of goal-setting prompts in the context of resource conservation policies.

Previous behavioral interventions have proven to be effective tools to reduce resource consumption, such as the widely used social norm comparisons (e.g., Allcott 2011, Ferraro, Miranda, and Price 2011, Ferraro and Price 2013, Dolan and Metcalfe 2015, Pellerano et al. 2017, Andor et al. 2020, and many others). In the area of goal-setting prompts, we are aware of only one related study on self-set energy consumption goals. Harding and Hsiaw (2014) use an event study design to evaluate the effects of an energy savings program in the United States that asked households to set themselves a target for their electricity consumption. The study finds that self-set goals reduced consumption by, on average, 8 percent in the short term, but identification hinges on the assumption that the timing of program take-up is quasi-random. Our estimated confidence intervals rule out these optimistic treatment effects. Interestingly, we estimate similar saving effects of our intervention if we use the same event study design for our treatment subjects. This implies that at least for our sample, an event study design fails to

[1] There are also few studies that find no effect of planning prompts. For instance, a recent study by (Carrera et al. 2018) asks gym visitors to make plans and estimates a precise null effect of plan-making on gym visits.

identify the causal effect of goals on consumption.

Second, we add to the emerging literature on the scalability of policy interventions by investigating the extensive margin decision of adopting an intervention that promotes resource conservation. Policymakers often need to base their decisions on studies from small-scale and highly selected samples, which may yield disappointing results when the intervention is brought to scale (Al-Ubaydli, List, and Suskind 2017, Al-Ubaydli, List, and Suskind 2019, Czibor, Jimenez-Gomez, and List 2019, DellaVigna and Linos 2020). Our intervention is rolled out using an easily accessible smartphone application that has the potential to be adopted by the majority of the population.

We also provide first evidence how smartphones perform in scaling up energy policies. The medical literature has identified nudges on smartphones as one of the most promising interventions to improve people's behavior at scale. Comprehensive overview articles by Vervloet et al. (2012) and Sarabi et al. (2016) conclude that smartphone applications that include reminders increase patient compliance with medication intake in the vast majority of the existing studies. The remarkable success of digital interventions in medicine has led economists to advocate for more studies that use smartphones for behavioral policy interventions (Al-Ubaydli et al. 2017). Our understanding of how these digital technologies can help people to follow through with plans is very limited despite the fact that there is substantial demand for goal-setting apps. Two examples are apps such as "Goal Meter" or "Goalmap," which have over 1.1 million downloads combined in the "Android Playstore."

Third, our experiment joins a small set of studies estimating the welfare effects of nudges and provides the first estimate of the welfare effects of goal-setting prompts. Understanding how nudges affect consumer surplus and social welfare is fundamental for the identification of optimal policy. In our setting, a complete cost-benefit calculation must not only consider the energy cost savings to consumers but also take into account how the nudge changes consumer utility. For example, the nudge may yield direct disutility to consumers by pressuring them to save energy. It may also cause consumers to give up energy service consumption they would otherwise enjoy. To capture the full effect on consumer well-being, it is therefore crucial to obtain an estimate of their valuation of the nudge.

For this purpose, we develop a simple model of technology adoption and goal-setting nudges to derive sufficient statistics to estimate consumers' willingness-to-pay for the nudge and the resulting welfare implications. In the experiment, we randomly offer users of the mobile app a lottery if they continue to use it. The lottery effectively creates exogenous variation in the opportunity cost of using the app. Comparing app usage between subjects who receive the lottery treatment and those who receive the goal-setting prompt allows us to approximate the average willingness-to-pay for the nudge.

We find that the average consumer is willing to give up a large amount of 7.41 EUR to *avoid* the nudge. This estimate compares negatively to the prominent social comparison nudges that show households their peers' energy consumption (Allcott 2011). Allcott and Kessler (2019) estimate an average willingness-to-pay of up to USD 4.36 (around 3.93 EUR) for a bundle of four comparison letters. The stark difference in willingness-to-pay in our study highlights the importance of estimating structural parameters of behavioral models to advance our understanding of how different nudges affect utility. Our study is the first to allow for a quantitative comparison between the welfare effects of underexplored energy conservation nudges to these well-established social comparison interventions that are widely employed by policymakers across the world. Only a handful of other studies have taken a structural approach to behavioral economics to natural field experiments: DellaVigna, List, and Malmendier (2012), DellaVigna et al. (2016), and Butera et al. (2019) who estimate social preferences; and Rodemeier and Löschel (2020) who estimate informational biases.[2]

We structure the presentation of our study as follows. Section 2 describes the experimental design, the energy app, and the technology diffusion strategy. Section 3 presents reduced-form results and underlying mechanisms. In Section 4, we estimate the welfare effects of the goal-setting nudge based on a simple theoretical model. Section 5 discusses our results in relation to previous evidence. Section 6 concludes.

[2]DellaVigna (2018) provides an overview of studies estimating structural behavioral parameters using lab, field, and observational data. Resulting policy implications of these models are also discussed in Bernheim and Taubinsky (2019).

2 Experimental Design

The experiment was conducted in cooperation with the utility provider of the municipality Münster, a German city with over 310,000 inhabitants. The utility is a subsidiary company of the municipality and is the default provider in the area, supplying about 80 percent of the households in the municipality. The experiment spans over a total period of seven months and was implemented in 2018. In the following, we first lay out the design of the mobile app and our treatment. We then elaborate on how the technology was diffused. The experimental design was pre-registered at the AEA RCT Registry.[3]

2.1 The Energy App

Our experiment intends to use a promising behavioral intervention and take it to a large scale. Mobile devices are often considered suitable scaling devices, as they are easily accessible by the majority of the population. For this reason, we developed a mobile app that integrated a goal-setting feature prompting users to set themselves energy consumption targets. To causally identify the effect of a goal-setting feature on energy consumption, the availability of the feature is randomly assigned among app users. The randomization of the treatment also implies that the app needs to include other desirable features such that users in the control group find it worthwhile to use. The app we developed therefore provided two useful features to every user irrespective of treatment assignment.

First, the app allowed households to scan their meter with their phone and to submit the meter reading to the utility electronically. This is a useful feature in the German context because the vast majority of German households do not have smart electricity meters. For meter readings, they are required to schedule an annual meeting with a representative of the utility who then reads the electricity meter manually. The app circumvents the hassle of manual meter readings by allowing for an electronic submission. As depicted in Figure 1a), the developed feature automatically recognizes and reads the electricity meter if the user points her phone's camera

[3] The trail number is AEARCTR-0003003. When we pre-registered the experiment, we intended to have one additional treatment arm, in which subjects could re-adjust their pre-set goal at any point in time. Since the first month of our experiment was the same for all treatments, we were able to adjust our design based on the surprisingly low sign-up rate. We decided to drop this additional treatment arm to have enough statistical power to identify the causal effect of goals on energy conservation.

toward the meter. The user then only needs to confirm the scan by clicking a button to upload the data to the utility provider's server. In case the user experiences technical issues with the scanning process, she can also manually enter the meter value. However, the app always takes a picture of the meter so that the self-reported meter value can be verified. Both the digital scans and the actual pictures are available to us.

Second, the app provides simple information on the electricity usage of various household appliances. Figure 1b) shows a screenshot of the information translated into English. This information is provided to all subjects irrespective of the experimental group assignment. Providing consumers with information becomes especially important for the treatment group because saving goals are likely to only affect consumption if subjects can set meaningful targets and know how to save energy in the first place. Of course, the information provided may also affect consumption without the goal-setting feature such that subjects in the control group alter their behavior. Our study therefore identifies the causal effect of a goal-setting nudge when consumers are informed about the energy consumption of their appliances. This also means that our design avoids the interpretation that goals may not affect behavior because people do not understand how choices map into outcomes.

Figure 2 gives an overview of the experimental timeline. For each app user, the experiment lasts for four experimental periods, where each period corresponds to 30 days. Upon signing up for the app, users get randomly assigned to a control and a treatment group with equal probabilities of one-half. Users need to enter their meter number, zip code, and meter type to exclude the possibility that the same household participates with multiple devices.[4] Participants then conduct a first digital meter reading with their phone. After subjects have done the first scan, they are informed that the next scan is due in 30 days. They are given the option to automatically save the due dates of all upcoming meter scans in the calendar on their phone. As we will discuss in more detail in the next section, subjects gathered a lottery ticket for every regular meter scan they submitted. The lottery assured that meter scans were incentivized for all subjects. Throughout the experiment, all participants received reminders to scan their meter one day before, one day

[4]The combination of meter number and zip code uniquely identifies a household. The meter type is either a regular meter or an HT-NT meter. An HT-NT meter records peak electricity consumption separately from off-peak consumption. This information is needed for the scanner functions, as with an HT-NT meter, two scans have to be made.

after, and exactly on the due date. We offered participants a grace period to scan their meter from two days before the due date to two days after the due date. If participants failed to scan the meter within this grace period, the scanning function was deactivated until another 26 days had passed. After this deactivation period, they could continue to use the scanning function.

We call the first experimental period our baseline period since these 30 days are identical for treatment and control. After having completed the second scan after 30 days, participants in the treatment and control groups receive the aforementioned information on the energy consumption of different appliances. Afterwards, participants in the treatment group are asked to set themselves an energy consumption goal for the upcoming 30 days. Figure 1c) shows the goal-setting screen. Participants enter their desired consumption in kilowatt-hours for the next 30 days. The app also tells the subject how the consumption goal translates into percentage savings relative to the baseline period, allowing them to try out different values and get a feeling for a realistic consumption goal.

After the third scan has been submitted, participants in the treatment group are informed of whether they have reached their goal. If they consumed less than or equal to the planned amount, they are congratulated and are shown a "thumbs up," as depicted in panel d) of Figure 1. If they fall short of their goal by consuming more than intended, they are shown a "thumbs down" (see panel e)). Subjects are always also told how many kilowatt-hours they consumed and how this compares to their target consumption. Afterwards, participants set a new goal for the next 30 days.

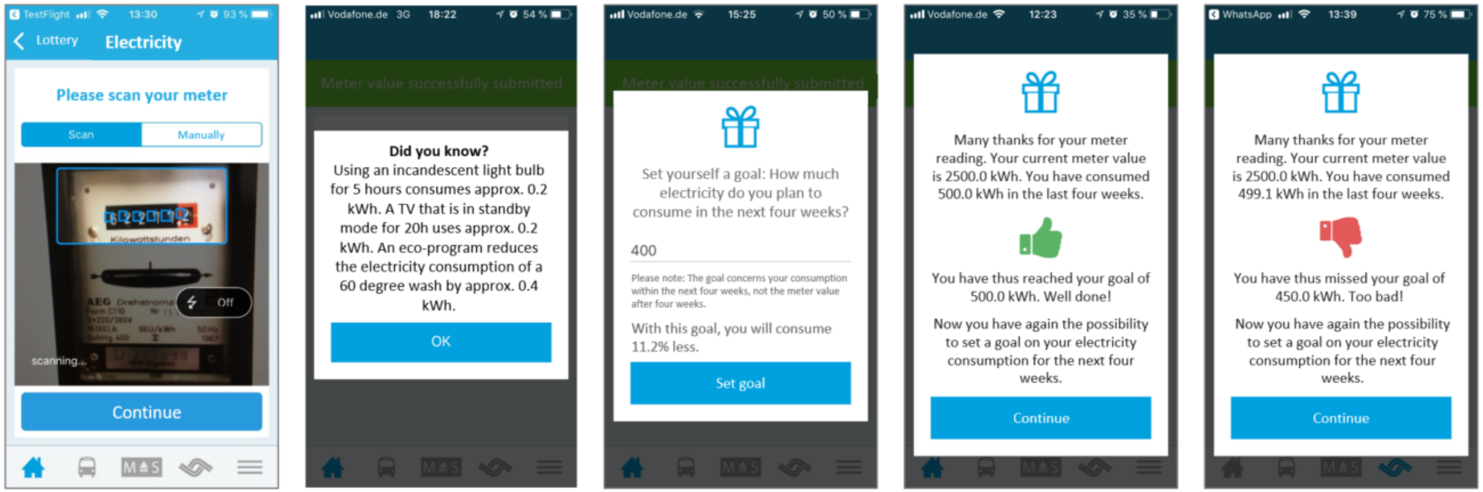
The control group does not have this goal-setting feature and just completes the third scan.

In experimental period 4 we randomize an additional treatment among all subjects that provides a financial incentive to save energy (see Figure A.5 for a screenshot). This "energy saving subsidy" treatment appears immediately after the fourth scan has been submitted and after a subject has potentially set a goal for the final period. Specifically, with a probability of one-half, the user is informed that she participates in a lottery. If she wins the lottery, she receives 1 EUR per kilowatt-hour saved in month 4 relative to her electricity consumption in month 3. The total amount a subject may receive from saving energy is limited to 100 EUR. Prizes are paid out in the form of vouchers for the online shop Amazon.com. The lottery draws 15 users with equal probability (and no replacement) from the pool of eligible users, which results in a winning probability of approximately 1.85 percent. The winning probability is the same for every eligible user and is communicated to the subject. With an average electricity price of 0.30 EUR per kWh, our savings subsidy corresponds to an increase in the expected electricity price of approximately 6.1 percent.

Thus, there are a total of four experimental groups in the last period: the control group with and without the savings subsidy and the goal-setting group with and without the savings subsidy. As we will show in Section 4, the savings subsidy enables us to estimate users' willingness-to-pay for the nudge.

Finally, participants are reminded of the fifth and last scan. After completing this last scan, all of them are invited to an online survey. The survey elicits individual characteristics, qualitative statements about goal-setting behavior, and measures of electricity price beliefs, loss aversion, and present-focus. We use this survey to investigate underlying behavioral mechanisms of the treatment effects.

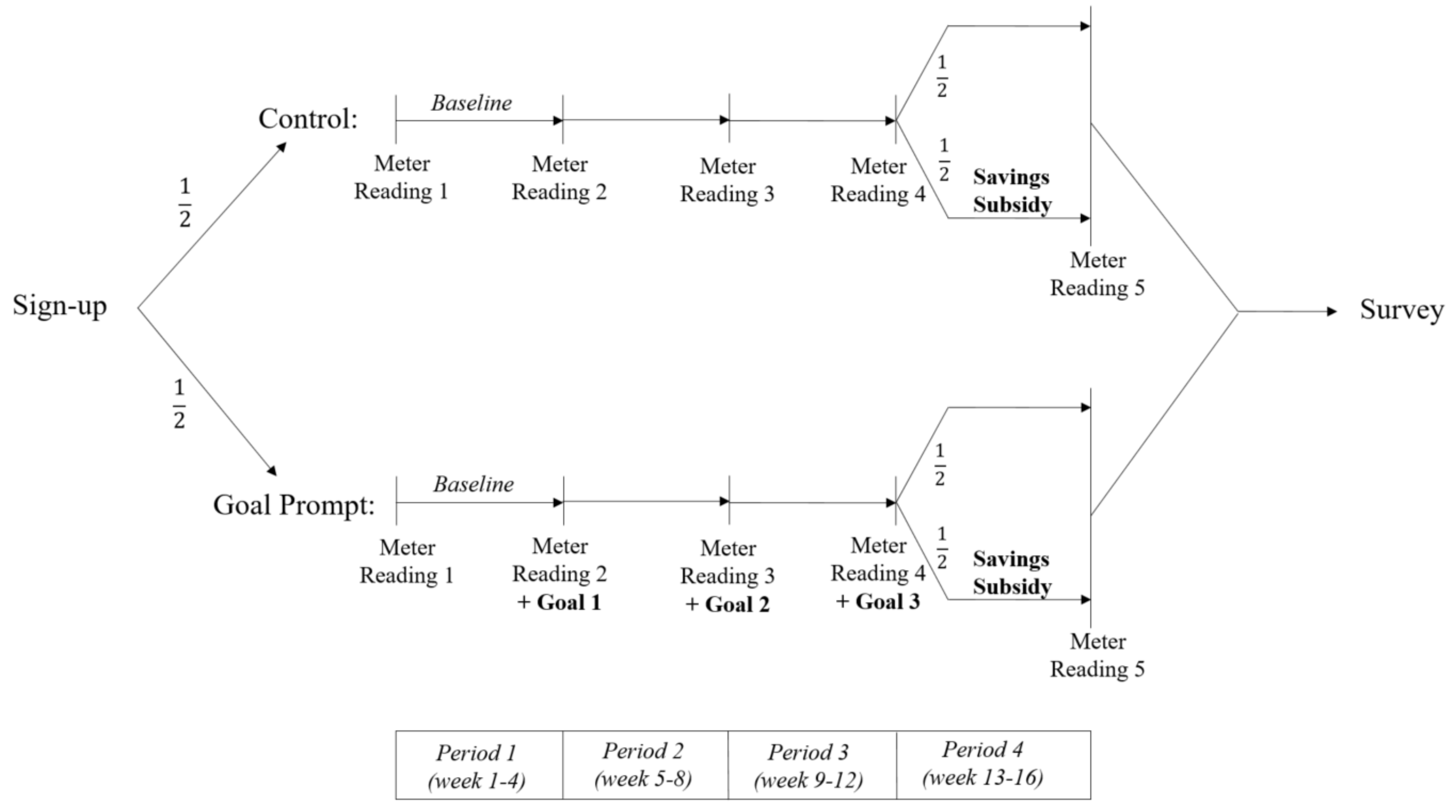
Figure 1: Screenshots of Energy App



a) Meter scan b) Appliance information c) Goal choice d) Achieved goal e) Failed goal

Note: The figure shows English translations of screenshots of the energy app. For the original version in German see Figure A.1.

Figure 2: Experimental Design and Timeline



2.2 Technology Diffusion

The energy app is easily accessible and intentionally simple to use for anyone capable of operating a smartphone, and it therefore has the potential to be used by the majority of the population. However, the diffusion of the technology may be slow if the app is not properly marketed. To maximize the chances of large-scale diffusion, we worked closely together with industry experts. Specifically, the design of the app was developed by an established IT company specialized for developing mobile apps. The app's promotion was designed by both an external marketing agency and marketing experts of the utility who have experience with the successful rollout and implementation of energy technologies.

We view the close cooperation with industry experts as a realistic simulation of how governments would ideally implement and scale up behavioral interventions: they partner with large players in the industry and leave much of the app's design and marketing strategy to industry experts. For our study, this approach also avoids the concern that the technology simply fails to deliver because its creation and promotion was entirely designed by researchers not trained for these purposes. Whenever we advertised the app, we were careful to only mention the features that were available to all users and not the randomized treatments.

The diffusion strategy consists of three main steps. First, the energy app is integrated into a larger and widely used mobile application. The "münster:app" has been downloaded by over 122,000 smartphone users and involves functions such as real-time information on changes in bus schedules, notifications of free parking spots downtown, and local news.[5] The integration of the energy app into the larger münster:app makes its usage particularly easy, as many households do not even have to download a new application. All of the münster:app users were notified about the new features by a push message on their phone and by a large banner displayed on the app's landing page.

Second, a promotion campaign targeted the entire municipality. Over 69,000 utility customers received direct and personalized mails promoting the app (see Figure A.3), and a popular local radio station played frequent advertisements. As shown in Figure A.2, 14,000 flyers were

[5]The münster:app is run by the local utility provider and is available both through the Google Play Store and the Apple App Store.

attached to annual electricity bills sent by mail. The same flyers were put into a print ad in a local newspaper, which was then distributed to 18,000 households. Another local newspaper with 48,000 prints announced the new app, and the social media outlets of the local utility advertised the app. The main student canteen with around 1,600 visitors per day displayed advertisement posters and flyers. An additional 4,000 flyers were handed out by research assistants either at public spaces or by going from door-to-door.

Third, we financially incentivized the use of the app (see Figure A.4 for a screenshot). *Every* app user receives a 45 EUR voucher for an online shop of household appliances if she completes all five meter scans and the online survey. In addition, all users (irrespective of how many scans they submit) participate in a lottery with various prizes such as holiday trips worth 1,000 EUR, Apple iPads, and 100 EUR vouchers for local activities. The total amount of the lottery prizes is 6,000 EUR. As previously mentioned, the chances to win in the lottery can be increased by conducting the digital meter readings: for every regular scan participants send in, they gather one additional ticket for the lottery. Subjects who submit all regular scans therefore gather five additional lottery tickets.[6]

2.3 Sample

Table 1 presents the summary statistics of our sample. A total of 1,627 participants signed up and sent in the first scan. Using information on the meter type, we know whether subjects have a double tariff involving different nighttime and daytime electricity prices. Around 3 percent of the sample have such a contract, and these subjects are balanced between the treatment and control groups. Meters also vary in how they record electricity consumption and may have different decimal places and numbers of digits before the decimal point. Both of these variables do not significantly differ between treatment and control.

The baseline electricity consumption is calculated as the difference between the second and first meter scan. Since not every subject submits a second meter scan, baseline consumption is not available for all subjects who signed up. Around 50 percent of those who signed up submitted two or more scans. Importantly, the probability to submit a second scan does not differ be-

[6] The lottery to encourage participation is independent of our randomly assigned Pigouvian lottery in period 4 that incentivizes energy savings.

tween treatment and control. In total, we have information on baseline electricity consumption for 844 subjects, who consume around 190 kWh, on average, in the baseline month. Baseline consumption also does not significantly differ between the treatment and control groups. Importantly, baseline consumption is far below the German average of 264 kWh for the respective month.[7] This suggests that the energy app is attracting consumers with an already low baseline energy consumption and might imply poor targeting—a point we turn to later in more detail.

Note also that the number of consumers taking up the app is relatively low given the substantial efforts we made in recruiting participants. Since every subject might have received a variety of advertisements, we cannot pin down the true response rate. However, we can create a very conservative upper bound on the response rate. Since at least 83,000 individual households were contacted through direct mailing or flyers with their energy bill, the most optimistic response rate is 1.96 percent. This rate indicates a very low demand for an app related to energy consumption, especially given our mass-marketing campaign and that participation was financially incentivized through 45 EUR vouchers and a lottery with sizable prizes.

To assure that our electricity data are reliable, our research assistants verified every meter scan. In particular, they compared the value reported by the digital scan (or the manual data entry by the user) with the pictures that the app made of the meter. Of a total of 3,610 meter scans, the research assistants were not able to verify 297 scans (e.g., because these scans did not match the pictures). In Table A.1 in the Appendix we present results from a regression of the probability to report non-verifiable meter scans on treatment and find no significant effect. We therefore drop these observations from our analysis. We also drop four users who have a missing first scan, which technically should not be possible and does not allow us to calculate baseline consumption.

[7] The annual electricity consumption of an average German household is 3,111 kWh (Federal Statistical Office of Germany 2019). To adjust for seasonal variation in consumption, we use the weights calculated for national load profiles for different months of the year in Fünfgeld and Tiedemann (2000). In the month of April, load profiles are about 8.5 percent of annual consumption, resulting in an average consumption of 264 kWh for the month of April.

Table 1: Summary Table

	Control		Treatment		Difference	
Double tariff (1 = yes)	0.036		0.029		–0.008	
	(0.187)		(0.167)		(0.009)	
Decimal places of meter	1.046		1.049		0.002	
	(0.342)		(0.398)		(0.018)	
Number of digits before decimal point	5.806		5.833		0.027	
	(0.605)		(0.615)		(0.030)	
Submitted at least two scans (1 = yes)	0.527		0.509		–0.017	
	(0.500)		(0.500)		(0.025)	
Baseline consumption (in kWh)		188.466		193.996		5.530
		(109.235)		(123.006)		(8.384)
N	824	434	803	410	1,627	844

Note: This table presents the mean of observable variables for the treatment and control group and the difference in means between these groups. Standard deviations for control and treatment are reported in parentheses. For the last two columns, standard errors of the differences are reported. Information on meter characteristics is available for everyone who signed up for the energy app. Information on baseline electricity consumption is available for anyone who scanned their meter at the beginning and end of period 1.

3 Reduced-Form Results

3.1 Extensive Margin Choices: Technology Adoption

We begin by analyzing subjects' utilization choice of the technology. In every experimental period $e \in \{1, 2, 3, 4\}$, the subject has the choice to actively use the app and submit a scan or to not use the app. We call the utilization choice the extensive margin. We code the outcome variable $Utilization_{ie}$ such that it equals one if subject i submitted a meter scan at the beginning and at the end of period e. If the subject did not submit the scan at the beginning or at the end of the period, the outcome equals zero. To investigate how the treatments affect utilization choice, we estimate the following linear probability model:

$$Utilization_{ie} = \alpha_e + \beta_e G_{ie} + \mathbb{1}_{e=4}(\gamma S_i + \delta \times S_i \times G_{ie}) + \epsilon_{ie}, \tag{1}$$

where G_{ie} equals one if subject i was in the treatment group in period e and zero otherwise. The average baseline probability to use the app in period e is given by α_e, and the error term is denoted by ϵ_{ie}. In period 4 we also randomized the savings subsidy, for which we assign the treatment dummy S_i. The coefficient γ is the treatment effect of the savings subsidy on utilization, and δ measures the interaction effect of the subsidy with the nudge.

Table 2 reports the results. Around 52 percent of subjects in the control group who signed up also submitted the second scan and therefore count as being part of period 1. For period 1, the difference in utilization between treatment and control is small and statistically insignificant, which is unsurprising given that period 1 was identical for subjects in both groups. The probability to use the app in the following periods becomes dramatically smaller for both treatment and control subjects. In the control group, only 29 and 23 percent use the app in periods 2 and 3, respectively. For these periods, the treatment coefficient of the goal-setting nudge is negative but remains economically small and statistically insignificant. This provides evidence of no selection on the extensive margin during the first two treatment periods and suggests we can causally identify the effect of the nudge on electricity consumption among users.

Columns 4 and 5 report the effects of the nudge and the subsidy on utilization in period

4. While column 4 shows pooled effects, column 5 interacts the subsidy with the goal-setting nudge. In the pooled model, the goal-setting nudge decreased the utilization probability by 4.2 percentage points. The effect is statistically significant at the 5 percent level. By contrast, the savings subsidy involves an increase in the utilization probability of 2.9 percentage points. While the effect is not statistically significant at conventional levels, the p-value of 0.148 is relatively small. An independent t-test shows that the treatment effect of the nudge and the subsidy are significantly different at the 1 percent level. Column 5 reports an even larger negative treatment effect of the nudge and a slightly smaller effect of the subsidy in isolation. When the nudge is combined with the subsidy, subjects are 1.6 percentage points more likely to use the app than when they are only treated with the nudge.

Overall, results from period 4 suggest that the goal-setting prompt sufficiently pressured or disturbed some of the subjects such that they stopped using the app.[8] Compensating consumers with the savings subsidy reduced this tendency.

Table 2: Effect on Extensive Margin: Probability of Using the App Over Time

	(1) Period 1	(2) Period 2	(3) Period 3	(4) Period 4	(5) Period 4
Goal treatment	–0.006	–0.013	–0.014	–0.042**	–0.050*
	(0.026)	(0.023)	(0.022)	(0.020)	(0.028)
Savings subsidy				0.029	0.021
				(0.020)	(0.029)
Goal × subsidy					0.016
					(0.040)
Constant	0.517***	0.293***	0.237***	0.190***	0.194***
	(0.018)	(0.017)	(0.016)	(0.018)	(0.020)
N	1,493	1,493	1,493	1,493	1,493

Note: The outcome variable is a dummy for whether a subject submitted the meter scan at the beginning and the end of the respective period. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are in parentheses.

[8]We do not find a significant correlation between failing to achieve the goal and dropping out of the app in the next period. While this correlation does not generally have a causal interpretation, it provides suggestive evidence consistent with the idea that subjects receive negative utility from the goal-setting prompt *directly* rather than from a failure to reach the goal.

3.2 Intensive Margin Choices: Goal Setting and Electricity Consumption

3.2.1 Goal Setting Behavior

Before we turn to the treatment effect of the goal-setting nudge on energy consumption, we briefly describe how subjects set their consumption targets. Specifically, we investigate whether subjects chose meaningful goals, as this indicates whether subjects engaged with the app and the goal-setting prompt. If consumption goals are meaningful, we would expect them to be correlated with actual consumption. By contrast, if subjects just set goals irrespective of their true consumption, the correlation would be zero. Figure 3 plots the consumption goal (planned consumption) for a month against the actual consumption of that same month. We also plot the 45-degree line indicating when planned and actual consumption are equal. To adjust for outliers, we exclude the top 5 percent of goals.

Visual inspection reveals a striking correlation between planned and actual consumption. Many subjects choose goals that are highly predictive of their consumption. The figure also shows that there is a non-negligible share of consumers who choose consumption goals equal to zero. In principle, a zero consumption goal is feasible to reach (e.g., when subjects go on vacation) but is more than unlikely to be a realistic goal for this many households. A more likely interpretation is that these subjects did not engage with the goal-setting feature and a value of zero is just a convenient mental default. Around 14 percent of all goals fall into this category.

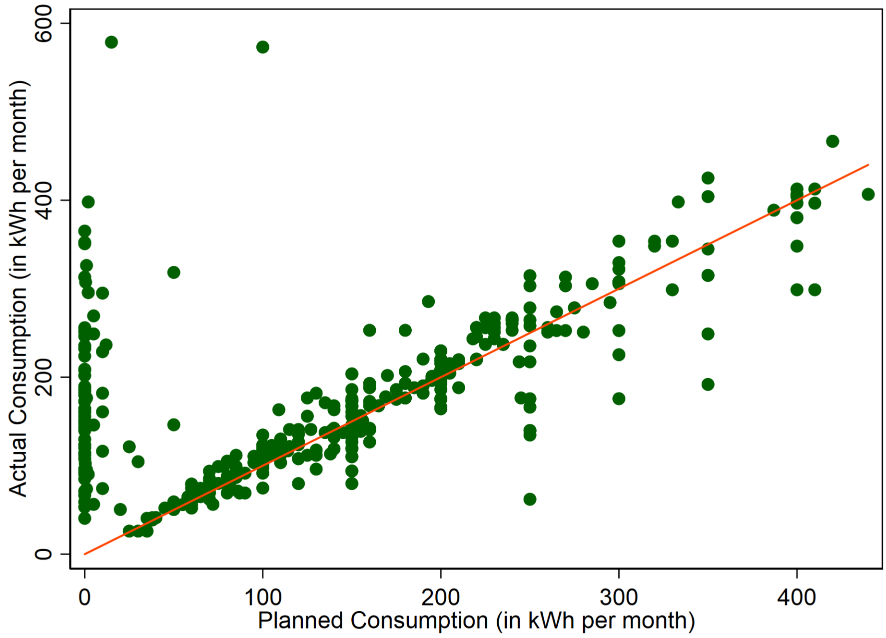
Among the remaining 86 percent of meaningful goals, we can further distinguish based on the ambitiousness of the goal. About 28 percent of all goals are *lenient*, meaning they are equal to or greater than baseline consumption. By contrast, the majority of 59 percent are *saving goals*, which are smaller than baseline consumption and reflect an intention to save energy. [9]

In a nutshell, there are two main groups of subjects in the treatment group. The first one, which includes the vast majority of the sample, sets meaningful consumption goals that are highly predictive of their actual future consumption. The second smaller, but non-negligible group, chooses meaningless goals of zero. Overall, there is a slight tendency for consumers to fall short of their goal, as most observations lie above the 45-degree line.[10]

[9] We further explore the association between goal achievement/failure in one period and the goal set for the subsequent period, but do not find a significant correlation.

[10] The median distance between planned and actual consumption is 7 kWh.

Figure 3: Actual Versus Planned Consumption



Note: The graph plots the consumption goals against the actual consumption for the treatment group. The orange line represents the 45-degree line. To account for outliers, we restrict the graph to the 95th percentile of consumption goals. Observations from all three treatment periods are included.

3.2.2 Effect on Electricity Consumption

The electricity consumption data are a panel data set with four experimental periods. When subjects stop using the app and do not scan their meters, electricity consumption is missing. We therefore have an unbalanced panel, which does not seem problematic for identification in the first three experimental periods because attrition rates do not systematically differ between treatment and control. Asymmetric attrition becomes a concern in period 4, in which treatment decreased the likelihood to use the app (see Section 3.1). Hence, we will use an unbalanced panel but run the analysis separately with and without the inclusion of period 4. Standard errors will be clustered at the subject level. Our empirical model is

$$log(kWh_{iet}) = A_i + E_e + T_t + \tau_e G_{ie} + \gamma_t S_{ie} + \epsilon_{iet},$$

where $log(kWh_{iet})$ is the natural logarithm of participant i's electricity consumption in experimental period e and calendar month t. We follow the literature in energy economics and logarithmize electricity consumption, but this does not change the qualitative interpretation of our results. Recall that subjects could submit their meter scan up to 2 days before and 2 days after the due date. We therefore normalize the outcome variable to resemble 30-days consumption.[11] The variables $G_{ie} \in \{0, 1\}$ and $S_{ie} \in \{0, 1\}$ indicate the nudge treatment and savings subsidy in experimental period e, respectively. Individual fixed effects and experimental period fixed effects are denoted by A_i and E_e, respectively. To control for seasonal variation, we also include months fixed effects, T_t. The consumption recorded in an experimental period e may belong to two calendar months because subjects do not necessarily start with the experiment on the first day of a calendar month. T_t therefore comprises a fixed effect for the calendar month in which consumption started and another fixed effect for the calendar month in which it ended.

The coefficient τ_e can be interpreted as the approximately average percentage change in electricity consumption in period e caused by the goal-setting prompt at the beginning of the same period. Table 3 reports the treatment effect coefficients for each period.

[11] Specifically, we divide the outcome variable by the number of days that lie between the first and second scan and then multiply the result by 30.

Table 3: Effect on Intensive Margin: Electricity Consumption

	(1) Log(kWh)	(2) Log(kWh)	(3) Log(kWh)
First goal	0.015	0.008	
	(0.026)	(0.024)	
Second goal	0.047	0.053	
	(0.036)	(0.037)	
Third goal	–0.034		
	(0.046)		
Savings subsidy	0.028		
	(0.040)		
Goals (pooled)			0.027
			(0.025)
Period 4 consumption included	Yes	No	No
N	1,813	1,538	1,538

Note: The outcome variable is the natural logarithm of electricity consumption measured in kWh. Month and user fixed effects are included in all regressions. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered on subject level are in parentheses.

The model used to produce results in column 1 includes observations from period 4 and must be interpreted with caution. We do, however, see that the exclusion of period 4 in column 2 does not substantially alter results. In both columns, we observe economically small treatment effects of the first goal-setting prompt of 1.5 and 0.8 percent on electricity consumption, respectively. Both coefficients are statistically indistinguishable from zero and involve tight confidence intervals. We can exclude treatment effects smaller than –3.6 percent and –3.9 percent with 95 percent confidence in columns 1 and 2, respectively. Results are similar for the second goal-setting prompt, but the lower bounds of the confidence intervals are even closer to zero. Columns 1 and 2 rule out treatment effects smaller than –2.4 percent and –2.0 with 95 percent confidence.

The third goal prompt involves an insignificant but negative coefficient. The confidence interval is still small but is larger than for the previous goals, which may be attributable to the fact that we have few observations for this period. Of course, it may also be a result of systematic selection on the extensive margin during this period. For the same reason, the coefficient of the savings subsidy may be imprecisely estimated and/or have no causal interpretation. The coefficient is small and statistically insignificant.

In column 3 we pool the goal-setting treatments to increase statistical power even further. We again observe a precisely estimated null effect and can rule out treatment effects smaller than –2.2 percent with 95 percent confidence.

Taken all findings together, our results provide considerable evidence that the goal-setting prompt failed to reduce electricity consumption but instead caused direct disutility to consumers as it reduced app utilization.

3.2.3 Heterogeneity

We next explore heterogeneity in treatment effects by baseline energy consumption. Table A.2 in the Appendix reports intensive margin treatment effects when the goal is interacted with an indicator for above-median baseline consumption. We do not find evidence of heterogeneous treatment effects as we estimate null effects for households with both above- and below-median baseline consumption. The lack of treatment effect heterogeneity is a particularly stark result against the intervention as it implies that even targeting of high consumption households would not induce energy savings.

The result complements previous findings in the literature showing that nudges that successfully manage to encourage resource conservation typically do so by inducing large treatment effects for high consumption households (see, e.g., Allcott 2011 and Andor et al. 2020).

Finally, we investigate the relationship between the ambitiousness of the goal and energy savings. Table A.3 shows the regression results for consumer subgroups that set themselves (1) a lenient goal (2) a goal equal to zero, and (3) a saving goals (as defined above). It is important to note that differences in energy consumption across these three groups do not necessarily have a causal interpretation since the goal is endogenously determined by subjects.

For the first goal, we do not find a statistically significant correlation between the goal and energy savings in any of the three subgroups. The same is true for the second goal among subjects who set themselves a relative savings goal equal to zero or a savings goal. By contrast, a lenient second goal is associated with a statistically significant *increase* in energy consumption compared to control. A potential explanation for this correlation is that subjects who intend to weakly increase consumption (i.e. set a lenient goal) in fact do so when prompted to set a goal.

3.3 Mechanisms

To investigate underlying mechanisms of the treatment effects, we first need to understand the theoretical argument of why goal setting would affect behavior. Economic models on goal setting typically argue that people set themselves goals to reduce overconsumption resulting from self-control problems (Koch and Nafziger 2011, Hsiaw 2013). Overconsumption is modeled as an implication of the well-established β-δ-model (Laibson 1997), in which consumers focus too much on the present when making choices. Goal setting may then be used as a commitment device for present-focused agents to mitigate overconsumption. The mechanism is that goals create reference points to which agents compare their behavior (Heath, Larrick, and Wu 1999). A crucial component of these models is loss aversion, meaning that consumers dislike falling short of a self-set goal by a certain distance more than they value achieving a goal by the same distance.

Our empirical setting features several intertemporal trade-offs that would result in overconsumption for present-focused consumers. Recall that in the German context, energy costs are only invoiced once a year. While there are exceptions to this billing cycle, 96 percent of our post-experimental survey respondents state that they receive energy bills annually. Even though every household pays a fixed monthly payment that should approximately cover monthly energy costs, this payment is completely independent of current consumption. This means that the costs of increasing current energy consumption is entirely delayed to the (far) future for the majority of our sample. In addition, externalities create another intertemporal trade-off, as discussed by Harding and Hsiaw (2014): the negative environmental consequences of excessive resource consumption only accrue in the future. Even if consumers have altruistic preferences, they may focus too little on the externalities from energy consumption.

Motivated by these theoretical arguments, we elicited several core model parameters in the post-experimental survey (see Appendix D). To measure present focus, we use the standard approach of two incentivized multiple price lists (see, e.g., Coller and Williams 1999 and Cohen et al. 2019). The first list asks participants to choose between receiving 100 EUR within the next 24 hours or an alternative amount in one month. The second list includes a trade-off of either receiving 100 EUR in one month or an alternative amount in two months. In both lists the alternative amount increases from 100 to 160 EUR across 14 decisions. Importantly, choices are incentivized since one choice from the two price lists is randomly picked as the actual payment. To ease research expense, we randomly chose a subset of subjects to be eligible for this actual payment. Randomizing eligibility has been shown to have no considerable effect on choices, as revealing true preferences remains optimal (Charness, Gneezy, and Halladay 2016).[12]

We infer discount rates, denoted δ, by assuming that utility, $u(z, \cdot)$, is linear in experimental payments, z, and indifference between the earlier and later payment at the midpoint, $\bar{z}$, between the payments at which the participant switches from preferring the earlier amount over the later amount.[13] Under these assumptions, we can calculate discount rates by $\delta = \frac{u_t(z,\cdot)}{u_{t+1}(\bar{z},\cdot)}$ for each participant and for both price lists. Here, t refers to the earlier date and $t+1$ to the later date of the respective list. The present focus parameter, denoted β, is then identified by the ratio of the discount rates inferred from the first and second multiple price list (Cohen et al. 2019). $\beta = 1$ implies time-consistent discounting, as both discount rates are equal, while $\beta < 1$ implies present focus.

Similarly, we measure loss aversion in our survey from multiple choices between either participating in a lottery, in which participants can win or lose 150 EUR with equal chances, or receiving a safe payment. The safe payment varied in 31 decisions from –150 to 150 EUR (Koch and Nafziger 2019). Different to the time preference elicitation, choices are hypothetical. As in Falk et al. (2016), we use the staircase method to reduce survey length. The staircase method condenses the 31 decisions of the multiple price list into five consecutive choices. This means that the first decision between lottery and safe payment determines the second choice,

[12]We communicated to participants a probability of payment, which was selected based on the number of app users such that in expectation, three participants will be paid. Participants were paid with vouchers for Amazon.com.

[13]If the participant switches multiple times between the earlier and later payments, we use the first switching point to determine the indifference amount. For participants always choosing the later payment, i.e., preferring 100 EUR later over 100 EUR earlier, we assume indifference at 99.5 EUR. Further, we assume participants always choosing the earlier payment to be indifferent at 165 EUR. However, whether imposing switching points or excluding never-switching participants, the average present focus estimate differs only slightly (1.030 when imposing versus 1.006 when excluding).

the second decision determines the third choice, etc. Although choices are hypothetical, Falk et al. (2018) show that preferences elicited from the staircase method correlate with a number of economic outcomes, such as education, savings and consumption patterns.

To elicit loss aversion, we follow the standard assumption that $u(z,\cdot) = z$ if $z \geq 0$ and $u(z,\cdot) = \lambda z$ if $z < 0$. Here, λ denotes the degree of loss aversion. If $\lambda = 1$, people value gains by the same amount they dislike losses of equal absolute size, while $\lambda > 1$ implies loss aversion. We assume the amount at which the participant is indifferent between lottery and safe payment, $\bar{z}$, to be the midpoint of the two safe payments at which the participant switches from preferring the lottery to preferring the safe payment.[14] With this assumption, we can solve the indifference equation between lottery and safe payment, i.e. $\mathbb{E}[u(z,\cdot)] = u(\bar{z},\cdot)$, for λ. Specifically, if $\bar{z} \geq 0$, $\lambda = \frac{\bar{z}-0.5\cdot150}{0.5\cdot(-150)}$, and if $\bar{z} < 0$, $\lambda = \frac{0.5\cdot150}{\bar{z}-0.5\cdot(-150)}$.

Table 4 shows distributional properties of present focus and loss aversion in our sample. The average subject features a present focus parameter of 1.03 and a loss-aversion parameter of 0.83. Both estimates are indistinguishable from the benchmarks of no present focus ($\beta = 1$) and no loss aversion ($\lambda = 1$). In the absence of measurement error, these values imply that the average participant is neither present-focused nor loss-averse. We can see that this is not only true for the average consumer but also for the majority of the survey sample, as most reported percentiles involve values close to 1. These values markedly differ from other studies. A meta-analysis by Imai, Rutter, and Camerer (2021) examines 220 estimates from the literature and estimates an average present focus parameter of 0.95 that is significantly different from one. For loss aversion, a meta-analysis by Walasek, Mullett, and Stewart (2018) estimates a median λ of 1.31 and excludes our finding of equal gain-loss weighting with 95 percent confidence. Other literature reviews regularly report higher loss-aversion parameters (e.g., the average λ of the studies summarized by Booij, Van Praag, and Van De Kuilen (2010) amounts to 2.069).

[14] If participants always preferred the lottery, we assume a switch to the safe payment when offered 160 EUR. Likewise, if they always preferred the safe payment, we assume a switch to the lottery when confronted with a safe loss of 160 EUR. Yet, when we instead exclude never-switching participants, the mean loss-aversion coefficient remains largely unchanged (0.826 when imposing switching points versus 0.868 when excluding participants).

Table 4: Behavioral Parameters: Present Focus, Loss Aversion, and Price Beliefs

	Sample Average (Std. error)	Percentile 10th	25th	50th	75th	90th	N	Representative Average for Comparison (Std. error)	Comparison Study
β	1.030 (0.007)	0.972	1	1	1.013	1.090	353	0.95 (0.02)	Imai, Rutter, and Camerer (2019) (Meta-analysis)
λ	0.826 (0.100)	–0.933	0	0.933	1.25	1.875	352	1.31 (0.11)	Walasek, Mullet, and Stewart (2018) (Meta-analysis)
$p_{max} - p_{min}$	6.658 (1.589)	0	2	4	9	15	193	12.10 (0.519)	Werthschulte and Löschel (2021) (German average)

Note: β denotes the present focus parameter, and λ is the loss-aversion coefficient. $p_{max} - p_{min}$ gives the differences between the maximum and minimum energy price participants believe to pay, measured in cents. Standard errors of the mean are in parentheses.

Our particular parameter estimates are supported by results from two survey questions, as depicted in Figures 4 and 5. As a measure of self-control, subjects were first asked how often they intend to save energy and then how often they fail to implement these intentions. Possible answers were "Never," "Sometimes," "Often," and "Always." We can see in Figure 4 that the distribution of answers to the first question is oppositely skewed to the distribution of answers to the second question. While the modal subject intends to save energy on a regular basis, she rarely fails to implement this intention. These results are also in line with the previously discussed finding that planned and actual consumption often coincide in the experiment (recall Figure 3).

We also find additional support for the estimated loss-aversion parameter. Specifically, we asked subjects how they feel when they either receive a refund of 100 EUR by the utility at the end of the year or when they have to pay an additional 100 EUR to the utility. Answers were ordered on a 7-point Likert scale from –3 (very bad) to +3 (very good). Figure 5 illustrates the distribution of responses, and we can see that self-reported emotions are almost perfectly symmetric about zero. Subjects do not systematically report stronger feelings of losing versus gaining 100 EUR. If anything, they value gains more than they dislike equal losses.

A theoretically driven explanation for the empirical null effect is therefore that the subject pool is not characterized by the behavioral anomalies that typically explain why goal setting affects behavior. This does, however, not mean that the general population does not feature these anomalies. Instead, subject features are likely to be a result of unfavorable self-selection into the pool of app users.[15] In fact, this form of systematic self-selection is evident by a number of additional results. For example, and as previously reported, the baseline consumption of app users is below the national average, which is important since previous research consistently finds larger energy savings effects for households with a larger baseline consumption (e.g., Allcott (2011), Andor et al. (2020)).

In addition, app users appear to have higher levels of "energy literacy" than the average German household. This becomes evident by another survey question eliciting subjects' confi-

[15]Note that a typical issue with small-scale studies is *favorable* self-selection of subjects into the pool of participants—a phenomenon Al-Ubaydli et al. (2017) label "adverse heterogeneity". Favorable self-selection implies that subjects select on gains such that study participants have larger treatment effects than the overall population.

dence about the energy price they pay. Subjects were asked to state the minimum and maximum price they think they pay for electricity. The last row in Table 4 reports the difference between the maximum and minimum as a measure of confidence. The average participant reports a relatively small interval of 0.07 EUR. To put this into perspective, we compare this estimate to the belief interval elicited in a nationally representative survey conducted by Werthschulte and Löschel (2021). In their sample the average deviation between maximum and minimum perceived electricity price is 0.12 EUR (i.e., almost twice as large as for our subject pool), suggesting consumers with an already high knowledge for energy-related topics use the app.

Additional evidence for systematic sample selection can be found in the sociodemographics, as documented in Table A.5 in the Appendix. App users are predominantly male (23 percent female versus 51 percent nationwide) and are better educated than the average German (76 percent with a high school degree versus 33 percent nationwide). The average participant is also slightly older (46 years versus 44 years) and earns a higher income (2,515 EUR per month versus 1,770 EUR per month). Participating households are even characterized by larger dwellings (107 square meters versus 98 square meters) and a larger household size than the national average (2.54 persons per household versus 1.98 persons per household). This means that energy consumption per capita is far below the average, since the total energy consumption per household was already relatively low. A plausible explanation is that sample participants consume energy more efficiently than a national representative household does, consistent with their high level of energy literacy.

In sum, the metrics point at very poor targeting properties of the app. Based on a theoretical argument, the ideal consumer to target would be loss averse and would have self-control problems and high levels of baseline energy consumption. Instead we find the opposite: subjects who decide to use the app are well-informed consumers that are neither loss-averse, nor present-focused, and that have low levels of baseline energy consumption. More generally, our results highlight the importance of carefully documenting selection into the pool of study participants in order to understand treatment effects—an approach advocated by List (2020).

Figure 4: Intentions and Self-Control

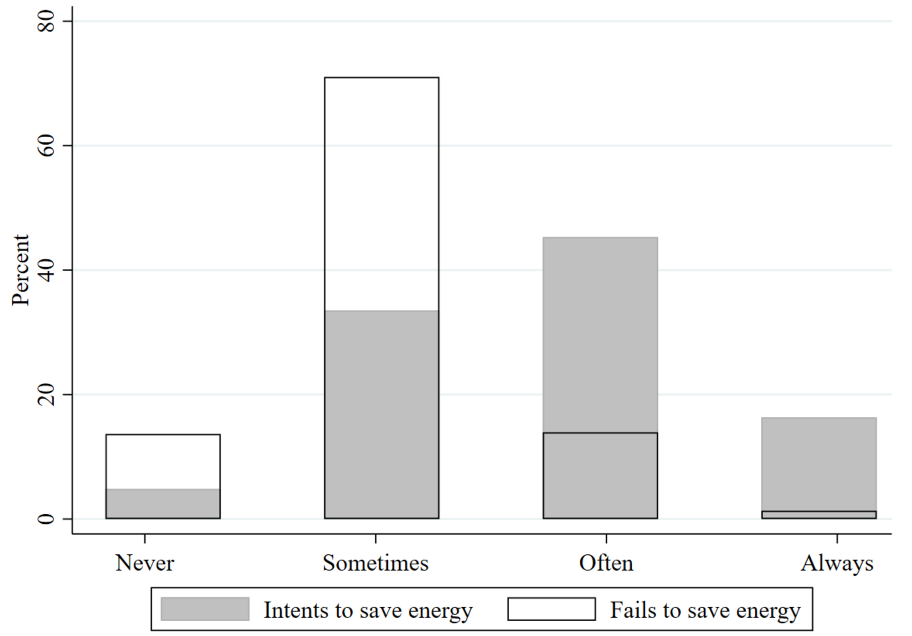
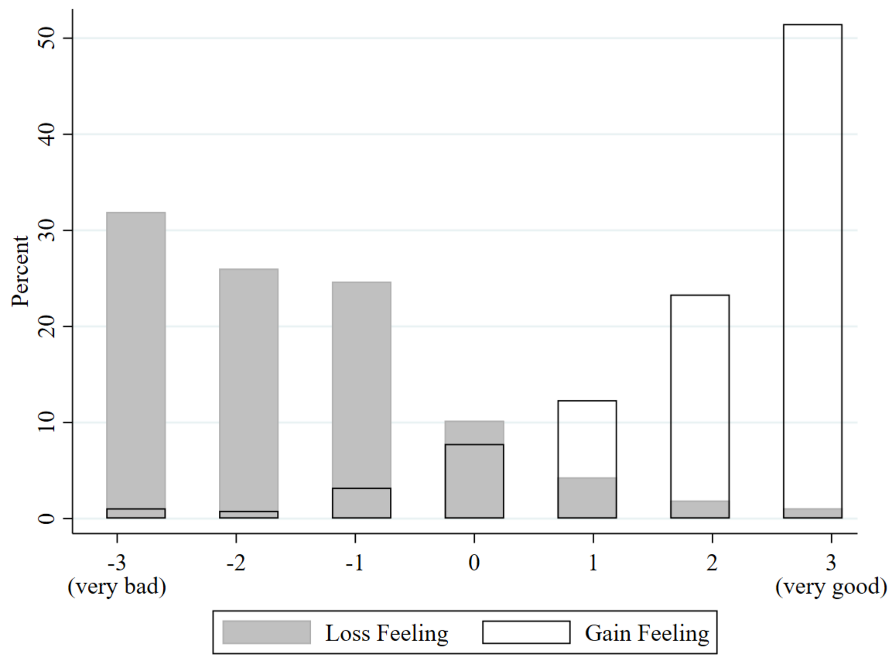


Figure 5: Gain and Loss Feelings



4 Welfare Analysis

4.1 A Simple Model of Technology Adoption

We develop a simple theoretical model that allows us to quantify the efficiency effects of the nudge. In our experiment, subjects can adjust their behavior on two margins. On the extensive margin, they choose app utilization $j \in \{a, o\}$, where a denotes the energy savings app and o the outside option. On the intensive margin, they potentially choose an energy savings goal (if the feature is available) and then choose their actual energy consumption. We first derive the optimal intensive margin choices conditional on a utilization choice and then characterize the optimal utilization choice on the extensive margin. Since an empirical welfare analysis requires exogenous price variation, we restrict our theoretical model to the last experimental period, in which we also varied the expected energy price. We therefore use a static model that applies to the fourth experimental period.

A consumer gets utility $v(x)$ from x units of energy. We make the standard assumptions about the properties of $v(x)$: $\frac{\partial v}{\partial x} > 0$ and $\frac{\partial^2 v}{\partial x^2} < 0$. The cost of energy consumption is given by $c_j(x, \cdot)$ and may depend on app utilization j and other factors, which we will explain below. We assume a quasi-linear utility function such that small variations in the energy price do not cause income effects. We consider this a reasonable assumption, as our experimentally induced price variation was relatively small in expectation. "Material utility" from consumption for a consumer with income Y is therefore given by $U = v(x) + Y - c_j(x, \cdot)$.

Consumers can typically not steer their energy consumption perfectly ex-ante due to exogenous factors such as varying weather conditions. They may also have uncertainty about how behavior maps into energy consumption.[16] We therefore assume that x is stochastic and the consumers can affect x through effort e. Formally, effort induces a draw from the conditional cumulative distribution function $H(x|e)$. We assume that $H(x|e)$ is differentiable in e.

To allow for the possibility that a goal-setting nudge affects choices, the consumer also receives "psychological utility," $B(\phi, x, g) = \phi + R(x, g)$, if she is treated with a goal-setting prompt $G_j \in \{0, 1\}$. Here, $G_j = 1$ indicates treatment and $G_j = 0$ no treatment. We assume

[16] While our experimental design involved information about the energy use of various activities, there may still be a remaining degree of uncertainty, and we therefore model it explicitly.

that utility from the nudge can be decomposed into two parts. First, it may cause direct utility unrelated to energy consumption, denoted ϕ. This term is positive if consumers enjoy being prompted to set a goal irrespective of its effect on energy consumption, and it is negative if they feel pressured by the prompt. The second term, $R(x, g)$, is the reference-dependence term, reflecting that the consumer receives utility from comparing her consumption to the self-set goal.

Since we use a simple static model, the consumer chooses the energy consumption goal and effort simultaneously. This is without loss of generality and can be easily translated to a dynamic model in which the consumer first chooses the goal and then chooses energy consumption in the next period. Denote the **optimal intensive margin choices** by the pair (e_j^*, g_j^*), which is simply given by

$$(e_j^*, g_j^*) = \arg\max_{e,g} \left\{ \mathbb{E}[U(x, c_j) + B(\phi, x, g)G_j|e] \right\}. \tag{2}$$

Next, we derive the extensive margin choice in which the consumer decides whether to use the app. Besides the utility she gets on the intensive margin, she may also like the energy app for other reasons. We define ϵ_j as the technology-related taste parameter. We call $\epsilon = \epsilon_a - \epsilon_o$ the relative taste parameter and let it follow an atomless distribution function $F(\epsilon)$. Furthermore, we let u denote the relative utility on the intensive margin of choosing the outside option: $u = \mathbb{E}[v(x) - c_o(x, \cdot) + B(\phi, x, g_o^*)G_o|e_o^*] - \mathbb{E}[v(x) - c_a(x, \cdot) + B(\phi, x, g_a^*)G_a|e_a^*]$. The consumer chooses the app if the sum of the utility on the intensive margin and the technology-specific taste parameter is larger for the app than for the outside option. Formally, the **optimal extensive margin choice** is to choose $j = a$ if and only if

$$\epsilon \geq u. \tag{3}$$

Demand for the app is then given by $D = \int_u dF(\epsilon)$.

We now show how this model can be used to empirically identify the effect of the goal setting nudge on consumer welfare. First, note that subjects in our experiment have $c_a(x, p, s, r, \pi) = px - \mathbb{1}_{r \geq x}(r - x)s\pi$, where p is the marginal energy price and s is a savings subsidy offered by technology a on every unit saved relative to a consumption benchmark r. r corresponds to the energy consumption in the previous month and $s = 1$ EUR. Since the subsidy was raffled among subjects, the subsidy is multiplied by the probability of winning the lottery, π.[17] Those who are not randomized into the subsidy group simply have $c_a = px$. Without loss of generality, we also set $c_o = px$.

Relating this model to our empirical results, we find that the goal-setting prompt had no statistically significant effect on energy consumption in the first two treatment periods but significantly reduced technology adoption in the third treatment period.[18] Proposition 1 establishes that when the effect of the nudge on energy consumption is negligible, we can identify the (dis)utility subjects get from the goal-setting nudge through knowledge of a small set of sufficient statistics: the treatment effect of the nudge on technology adoption, denoted $\Delta_G D$; the treatment effect of the savings subsidy on technology adoption, denoted $\Delta_s D$; and the first-order energy cost savings due to the subsidy. Knowledge of these statistics also enables us to approximate the effect of the nudge on consumer welfare. We denote consumer surplus by $CS(G_a, s)$.

Proposition 1. *If the effect of the goal setting prompt on energy consumption is negligible, then willingness-to-pay for the nudge is given by*

$$\phi \approx \underbrace{\frac{\Delta_G D}{\Delta_s D}}_{\substack{\textit{ratio of treatment} \\ \textit{effects on extensive} \\ \textit{margin}}} \underbrace{\mathbb{E}[(r-x)|e_a^*, r \geq x]\pi\Delta s}_{\substack{\textit{first-order cost savings} \\ \textit{on intensive margin} \\ \textit{due to subsidy}}}. \tag{4}$$

The effect of the nudge on consumer surplus is then

$$CS(1,s) - CS(0,s) \approx \frac{\Delta_G D}{\Delta_s D}\mathbb{E}[(r-x)|e_a^*, r \geq x]\pi\Delta s\left(D + \frac{\Delta_G D}{2}\right). \tag{5}$$

If the nudge does not affect energy consumption, the consumer's willingness-to-pay for the

[17]The assumption of a quasi-linear utility function also implies risk neutrality.

[18]While the coefficient of the nudge on energy consumption in the third treatment period is also insignificant, it may not have a causal interpretation because treated subjects were more likely to opt out of the app. Given the tightly estimated null effects in the previous periods, it is, however, unlikely that the effect on consumption was large in the third period. As an alternative, we could also impose additional structure to model the selection process and then estimate the effect on the intensive margin in the last period with this additionally imposed structure.

nudge simply equals the direct utility she gets from the nudge. As we prove in the Appendix, a first-order approximation of this term is simply the ratio of treatment effects of the nudge and the subsidy on demand for the app, multiplied by the first-order electricity cost savings due to the subsidy. These cost savings are identified in our experiment by averaging the difference $(r - x)$ for all control group subjects who have $r \geq x$ and then multiplying this average with the expected change in the subsidy.

Knowledge of willingness-to-pay for the nudge allows us to approximate its effect on consumer surplus. Equation 5 is obtained by a Taylor approximation up to second order and only involves one additional statistic compared to equation 4, namely control group demand for the app. Since this is obviously observed in our experiment, we have all of the ingredients to estimate the effect of the nudge on consumer surplus.

4.2 Structural Estimates

To calculate willingness-to-pay for the nudge, we use the first part of Proposition 1. Note that we must use the treatment effects on the probability to use the app in period 4, for the subsample of consumers who were still using the app in period 3. The reason is that subjects were offered the savings subsidy after submitting the scan at the end of period 3. Thus, those who dropped out earlier, e.g., at the end of period 2, were never offered the subsidy. Table A.4 shows the results of a regression of treatments on the probability to use the app conditional on having used the app in the previous period. Column 3 involves the relevant results for the identification of the structural parameters. The nudge decreased the probability to use the app in period 4 by 12.4 percentage points, while the savings subsidy increased the probability by 2 percentage points. These are the estimates of the relevant treatment effects, $\Delta_G D$ and $\Delta_s D$. We also calculate the average first-order savings of the subsidy as described in the previous section. We find that $\mathbb{E}[(r - x)|e_a^*, r \geq x] = 68.72$ kWh and multiply this result by the winning probability, $\pi = 1.85\%$, and $\Delta s = 1$ EUR. This yields expected first-order savings of 1.27 EUR per consumer.

Accordingly, column 1 in Table 5 shows that the average consumer's willingness-to-pay for the nudge is –7.41 EUR. Thus, the average consumer is willing to give up 7.41 EUR to *avoid* the nudge.[19] This amount is relatively large and compares negatively to other nudges that intend to encourage resource conservation. Allcott and Kessler (2019) estimate a positive willingness-to-pay for home energy reports that compare a household's energy consumption with that of other similar households. Willingness-to-pay estimates in their study range from USD 2.58 to USD 4.36 for a bundle of four home energy reports.

Column 2 presents the effect of the nudge on consumer surplus. The average consumer loses 4.32 EUR in utility due to the nudge. Obviously, this number is closer to zero than willingness-to-pay because consumers can avoid the full loss in utility by reducing their probability to use the app.[20]

To put the implications of the nudge for consumers into perspective, we run a back-of-the-envelope calculation to show how a nationwide rollout of the app would have affected consumer welfare. Münster has approximately 310,000 inhabitants of which 343 subjects still used the app at the end of the third month after the rollout (i.e., 0.1 percent of all residents). Assuming that this ratio is the same for a nationwide rollout of the app, we would expect 91,858 people out of a total of 83.02 million German citizens to use the app in the fourth month. The goal-setting nudge would then reduce consumer welfare by 396,827 EUR over a period of only four weeks. Additionally, one would have to subtract non-negligible costs for promoting the app nationwide.

Table 5: Structural Estimates and Consumer Welfare

Willingness-to-pay for Nudge (in EUR)	Effect on Consumer Welfare (in EUR per consumer)
–7.41	–4.32

Note: This table reports structural parameters calculated as described in Section 4.1. Specifically, we use the estimated treatment effects on the probability to use the app in period 4 conditional on pre-period utilization (see column 3 in in Table A.4). All numbers therefore apply to the fourth experimental period.

[19] If consumers are risk-averse, our estimate is a lower bound of (negative) willingness-to-pay. While we might therefore overestimate the disutility of the nudge, any reasonable degree of risk aversion over these small-stake lotteries can only change our results slightly. Also recall that the average consumer in our sample is *not* loss-averse according to our survey measures.

[20] The effect of the nudge on *social* welfare would be the effect on consumer surplus minus the nudge provision costs. The latter are costs of developing and promoting the energy app. The total costs resulting from programming and promoting the energy app in our case were approximately 60,000 EUR. Since these are one-time fixed costs, we do not include them in the cost-benefit analysis. In that respect, the reported loss in consumer surplus due to the nudge can be considered as lower bound of the total loss in social welfare.

5 Discussion

In this section we discuss how our results resonate with the existing literature on goal setting. Our reduced-form and structural metrics suggest that the goal-setting nudge has failed to deliver and is not a cost-effective policy tool to encourage resource conservation. We find substantial evidence that the lack of success of our intervention is at least partially driven by unfavorable selection of highly energy-literate and seemingly rational consumers into the subject pool.

Besides the poor targeting properties of the app, other factors may explain the null effect. One difference to other studies lies in the nature of energy consumption compared to other consumption dimensions. Much of the existing literature finds goal-setting prompts to be effective in domains where the goal targets one single action, such as getting vaccinated or going to the polls. By contrast, repetitive behavior, such as regularly going to the gym, has shown to be less affected by goal prompts (Carrera et al. 2018). Reducing electricity consumption often requires repetitive conservation actions and a high awareness while enjoying energy services. Each conservation activity, such as switching off a light bulb, typically only saves small amounts of electricity. This is particularly true for many European households that consume lower levels of electricity compared to US households due to using less air conditioning, electric heating, and other energy-intensive activities (Andor et al. 2020).

An alternative way to conserve energy with less repetitive effort would be to retrofit the house, e.g., by replacing energy-inefficient appliances. Yet, several other market frictions established in the literature may impede energy efficiency investments, such as credit constraints for low-income households (Berkouwer and Dean 2019) and false beliefs about how a product's energy efficiency level maps into actual savings (e.g., Attari et al. 2010).

There is, however, evidence from an event study by Harding and Hsiaw (2014) that a goal-setting prompt significantly reduces energy consumption. Their study investigates an energy savings program in the US that offered subjects the possibility to set themselves energy consumption goals in a utility's online system. Results of the study suggest that the goal-setting prompt significantly reduced electricity consumption by 8 percent in the first two months after program take-up and by 4.4 percent in the longer term. Different to our randomized controlled trial, their identification strategy crucially relies on the assumption that the timing of subjects' program adoption is quasi-random. Since our study directly randomizes the goal-setting feature, it also allows us to evaluate whether an event study would have identified the treatment effect in our sample. We therefore analyze whether our study would have yielded similar results to Harding and Hsiaw (2014) had we implemented the same identification strategy. To address this question, we run an event study regression for our subsample of treatment group subjects:

$$log(kWh_{iet}) = \gamma_i + \alpha_t + \delta Event_{ie} + \kappa S_{ie} + \xi_{iet}, \tag{6}$$

where $Event_{ie} \in \{0, 1\}$ is an indicator equal to one when consumption of subject i belongs to one of the treatment periods and zero otherwise. Calendar-month fixed effects are denoted by α_t and are coded as described in the Section 3.2. Individual fixed effects are given by γ_i, and the error term is denoted ξ_{iet}. The dummy S_{ie} controls for our additional financial reward that is not present in the study by Harding and Hsiaw (2014).

Results are presented in Table 6. Column 1 includes all treatment periods, while column 2 only includes the first two months after the treatment started. In both specifications we find statistically significant coefficients implying reductions in electricity consumption of 9.5 and 7.1 percent. Interestingly, these coefficients are close to the estimated treatment effect of 8 percent over the same time period (two months) in Harding and Hsiaw (2014). Recall that our estimated 95 percent confidence intervals of the identified treatment effects in the RCT exclude these values. Our results imply that the timing of treatment adoption is not quasi-random in our experiment and may cast doubt on event study design in these settings. Controlling for time and individual fixed effects does not eliminate the selection bias.

We stress that this is only suggestive evidence reconciling the difference to previous results and does not imply event studies are generally not identified in these settings. Our exploratory analysis simply suggests that methodological differences may explain the lack of congruence to previous findings.

Table 6: Effect on Electricity Consumption as Estimated from an Event Study

	(1) Log(kWh)	(2) Log(kWh)
Event	–0.095**	–0.071**
	(0.039)	(0.030)
Savings subsidy	–0.050	
	(0.062)	
Period 4 consumption included	Yes	No
N	872	751

Note: The outcome variable is the logarithm of electricity consumption measured in kWh. Calendar-month fixed effects are included. The variable "Event" equals one when the observation belongs to one of the treated periods: E2, E3, and E4. It equals zero if the observation belongs to the baseline period E1. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered on subject level are in parentheses.

6 Conclusion

Our study mimics a large-scale policy intervention that leverages insights from psychology to encourage resource conservation. We build on the promising results in the literature on goal setting and plan-making nudges and examine the role of goals for energy consumption. We scale the intervention through a newly developed energy app for mobile phones that is easy to use and is accessible for the majority of the population. We monetize the welfare effects of our intervention for consumers by randomly offering financial incentives that reward app utilization.

Despite substantial marketing efforts and financial incentives to participate, we find surprisingly little demand for the energy app. Those subjects adopting the energy app do not alter their actual energy consumption in response to the nudge even though they set themselves meaningful goals that are highly predictive of future consumption. Observable subject characteristics point to suboptimal targeting properties of the app as a likely mechanism for the null effect. The average subject who selected into the pool of app users has an already low baseline level of energy consumption. A complementary survey elicits behavioral parameters and shows that the average user is neither present-focused nor loss averse—the two core features in the theoretical literature that explain why self-set goals affect behavior.

Further, the nudge significantly reduced the probability to use the app over time, indicating that the goal-setting prompt caused direct disutility by pressuring subjects. Using random price variation, we estimate that the average user is willing to pay a relatively large amount of 7.41 EUR to *avoid* the nudge. Structural estimates imply that a goal-setting prompt could cause substantial welfare losses if implemented nationwide.

Our results are also helpful for the active policy debates on digital consumer technologies—referred to as "smart" devices—as potential measures to reduce energy consumption. Both the low demand for the energy app and the null effects of the nudge among those selecting into the app suggest a limited role of the use of mobile applications to scale up behaviorally-motivated energy policies.

Finally, it is important to keep in mind that our results may be specific to energy conservation. Previous studies in other fields show that goal-setting nudges can help people follow through with their plans. Our results do not stand in contrast to these studies but rather show the importance of distinguishing the fields in which goal-setting nudges can be effective. We encourage future research to identify the particular factors that can predict the success of nudges in different areas of public policy.

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Online Appendix (Not for Publication)

A Mathematical Proofs

Proof of Proposition 1

In the following, we write utility on the intensive margin as $u = u(G_a, s)$ to make explicit that it depends on our treatment variation. In particular, demand for the app is a function of the goal setting nudge and the savings subsidy: $D(G_a, s) = \int_{u(G_a,s)} dF(\epsilon)$. A very small change in the subsidy changes demand by:

$$\begin{aligned}\frac{\partial D}{\partial s} &= -\frac{\partial u(G_a, s)}{\partial s} f(u(G_a, s)) \\ &= \mathbb{E}[(r - x)|e_a^*, r \geq x]\pi f(u(G_a, s))\end{aligned}$$

where we have used the envelope theorem.

The demand response to a change in s by Δs can therefore be approximated to first order by:

$$\Delta_s D \approx \frac{\partial D}{\partial s} \Delta s \tag{7}$$

$$= \mathbb{E}[(r - x)|e_a^*, r \geq x]\pi \Delta s f(u(G_a, s)) \tag{8}$$

Next, we analyze the effect of the goal setting nudge on demand. In line with our empirical results, we make the assumption that the effect of the nudge on energy consumption is negligible. Formally, we assume $\mathbb{E}[R(x, g)|e_a^*] \approx 0$. Under this assumption the difference in relative utility on the intensive margin with and without the nudge is given by:

$$u(1, s) - u(0, s) \approx -\phi. \tag{9}$$

The effect of the goal setting nudge on demand for the app is given by:

$$\Delta_G D = D(1,s) - D(0,s) \tag{10}$$

$$= -\int_{u(0,s)}^{u(0,s)-\phi} dF(\epsilon) \tag{11}$$

$$\approx \phi f(u(0,s)) \tag{12}$$

where the approximation in the last line requires that the density $f(\epsilon)$ is roughly constant on the interval $[u(0,s), u(0,s) - \phi]$.

Now set $G_a = 0$ in equation 8, solve for $f(u(0, s_a))$, and substitute the result in equation 12 to get:

$$\phi \approx \frac{\Delta_s D}{\Delta_G D} \mathbb{E}[(r-x)|e_a^*, r \geq x] \pi \Delta s \tag{13}$$

which is the first statement in the proposition.

To prove the second statement in the proposition, first note that consumer surplus can be written as:

$$CS(G_a, s) = \int \left(\mathbb{E}\left[v(x) - c_o + B(\phi, x, g_o^*) G_o | e_o\right] + \epsilon_0\right) dF(\epsilon) - \int_{u(G_a,s)} \left(u(G_a, s) - \epsilon\right) dF(\epsilon)$$

The change in consumer surplus due to the goal setting nudge is therefore given by:

$$
\begin{aligned}
CS(1,s) - CS(0,s) &= \int_{u(0,s)} (u(0,s) - \epsilon)\, dF - \int_{u(1,s)} (u(1,s) - \epsilon)\, dF \\
&= \int_{u(0,s)} (u(0,s) - \epsilon)\, dF - \int_{u(0,s)-\phi} (u(0,s) - \phi - \epsilon)\, dF \\
&= \int_{u(0,s)} (u(0,s) - \epsilon)\, dF \\
&- \left[\int_{u(0,s)} (u(0,s) - \phi - \epsilon)\, dF - \int_{u(0,s)}^{u(0,s)-\phi} (u(0,s) - \phi - \epsilon)\, dF \right] \\
&= \phi D + (u(0,s) - \phi) \int_{u(0,s)}^{u(0,s)-\phi} dF - \int_{u(0,s)}^{u(0,s)-\phi} \epsilon dF \\
&\approx \phi D - (u(0,s) - \phi)\, f(u(0,s))\phi - f(u(0,s) \int_{u(0,s)}^{u(0,s)-\phi} \epsilon d\epsilon \\
&= \phi D - (u(0,s) - \phi)\, f(u(0,s))\phi - f(u(0,s) \left(u(0,s)(-\phi) + \frac{\phi^2}{2} \right) \\
&= \phi D + f(u(0,s))\phi^2 - f(u(0,s) \frac{\phi^2}{2} \\
&= \phi D + f(u(0,s) \frac{\phi^2}{2} \\
&\approx \phi \left(D + \frac{\Delta_G D}{2} \right)
\end{aligned}
$$

The approximations, again, require that f is roughly constant on $[u(0,s), u(0,s) - \phi]$. Substitute ϕ with the expression in equation 4 to arrive at the second statement in the proposition. This completes the proof. □

B Tables

Table A.1: Probability of Submitting a Non-Verifiable Scan

	(2) First Scan	(3) Second Scan	(4) Third Scan	(5) Fourth Scan	(6) Fifth Scan
Goal Treatment	-0.012 (0.014)	-0.017 (0.022)	0.004 (0.025)	0.010 (0.026)	0.012 (0.044)
Savings Subsidy					-0.009 (0.039)
Goal x Subsidy					-0.049 (0.055)
Constant	0.087*** (0.010)	0.094*** (0.016)	0.081*** (0.017)	0.070*** (0.017)	0.099*** (0.028)
N	1,628	632	484	427	435

Note: The outcome variable is a dummy that equals 1 if the subject submitted a scan that could not be verified by the picture, and zero otherwise. We run this regression for each of the five scans separately. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table A.2: Effect on Intensive Margin by Median Baseline Usage

	(1) Log(kWh)	(2) Log(kWh)
First Goal	0.047	
	(0.054)	
Second Goal	0.076	
	(0.078)	
Above Median	0.916***	0.916***
	(0.032)	(0.032)
First Goal x Above Median	-0.024	
	(0.058)	
Second Goal x Above Median	-0.028	
	(0.079)	
Goals (pooled)		0.060
		(0.058)
Goals (pooled) x Above Median		-0.025
		(0.059)
Constant	4.627***	4.627***
	(0.027)	(0.027)
N	1,538	1,538

Note: The outcome variable is the natural logarithm of electricity consumption measured in kWh. Month and user fixed effects are included in all regressions. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered on subject level are in parentheses.

Table A.3: Subgroup Analysis of Intensive Margin Behavior

	(1) Log(kWh)	(2) Log(kWh)	(3) Log(kWh)
First Goal	-0.009	0.008	-0.006
	(0.044)	(0.026)	(0.040)
Second Goal	0.130***	0.035	0.059
	(0.047)	(0.041)	(0.052)
Treatment Subgroup	Lenient Goal	Saving Goal	Zero Goal
N	1,317	1,467	1,288

Note: The outcome variable is the natural logarithm of electricity consumption measured in kWh. Month and user fixed effects are included in all regressions. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered on subject level are in parentheses.

Table A.4: Probability of Using the App Conditional on Pre-Period Utilization

	(1) Period 2	(2) Period 3	(3) Period 4	(4) Period 4
Goal Treatment	-0.019	-0.000	-0.124**	-0.151*
	(0.036)	(0.047)	(0.053)	(0.080)
Savings Subsidy			0.021	-0.002
			(0.053)	(0.072)
Goal × Subsidy				0.048
				(0.107)
Constant	0.566***	0.614***	0.645***	0.658***
	(0.025)	(0.033)	(0.046)	(0.054)
Observations	768	427	343	343

Note: The outcome variable is a dummy for whether a subject used the app in the respective period. We define the outcome variable such that a subject is said to use the app if she submitted the meter scan at the beginning and the end of a period. In this analysis we condition on subjects who have used the app in the previous period. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

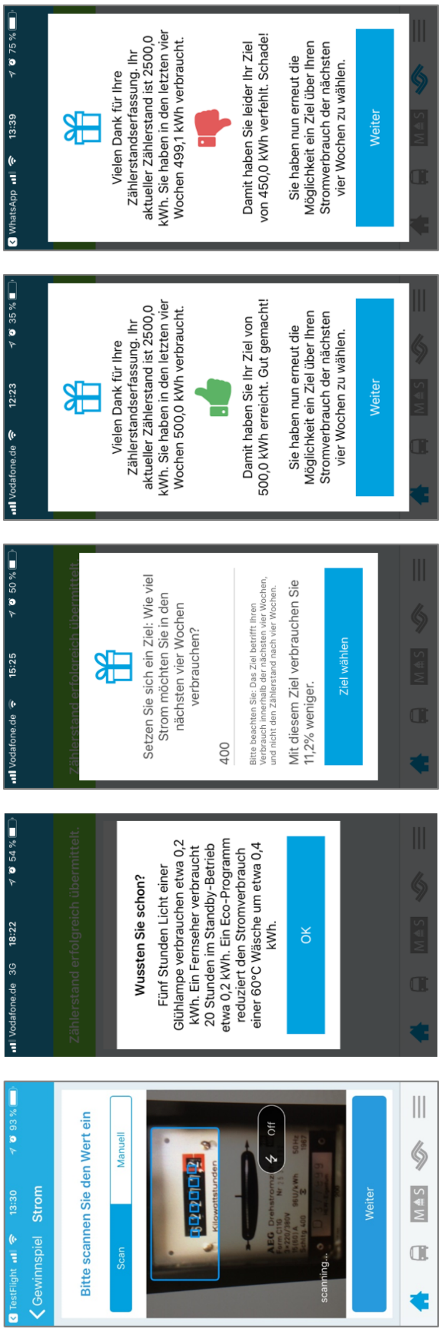
Table A.5: Socio-demographic information

	Sample Average (Std. dev.)	Min	Max	N	Country Average (Std. dev.)	Two-sided t-test
Female (1 = Yes)	0.23 (0.42)	0	1	360	0.51 (0.50)	$p < 0.00$
Age	45.76 (14.76)	20	82	369	43.83 (23.29)	$p = 0.01$
High School Degree (1 = Yes)	0.76 (0.43)	0	1	350	0.33 (0.47)	$p < 0.00$
Working (1 = Yes)	0.78 (0.42)	0	1	338	0.76 (0.43)	$p = 0.40$
Personal net income (in Euros)	2514.74 (1113.20)	249.50	4500	283	1770.38 (1671.66)	$p < 0.00$
Dwelling size (in square meters)	107.31 (44.18)	16	260	371	98.29 (45.94)	$p < 0.00$
Household size (count)	2.54 (2.54)	1	6	379	1.98 (1.10)	$p < 0.00$

Note: “High school degree” is an indicator for having a degree which qualifies to go to university (e.g., the German “Abitur”). “Working” is an indicator for the German job categories “Auszubildende/-r”, “Arbeiter/-in”, “Angestellte/-r”, “Beamter/Beamtin” or “Selbstständiger”. Participants were given ten income categories to indicate their monthly net income: 1 “0-499 EUR.” 2 “500-899 EUR.” 3 “900-1.299 EUR.” 4 “1.300-1.499 EUR.” 5 “1.500-1.699 EUR.” 6 “1.700-1.999 EUR.” 7 “2.000-2.599 EUR.” 8 “2.600-3.199 EUR.” 9 “3.200-4.499 EUR.” 10 “ at least 4.500 EUR.” They could also decide not to answer the income question. The variable ‘personal net income’ is calculated as the midpoint of the selected income bracket. “Dwelling size” gives the square meters of the dwelling the participant lives in. “Household size” is the number of persons in the household, including the participant. National averages are obtained from Federal Statistical Office of Germany (2020) and SOEP (2020).

C Figures

Figure A.1: Original Screenshots of Energy App in German



a) Meter scan

b) Appliance information

c) Goal choice

d) Achieved goal

e) Failed goal

Figure A.2: Flyer

Figure A.3: Letter from Direct Mailing Campaign

Stadtwerke Münster

Stadtwerke Münster GmbH . Postfach 76 09 . 48041 Münster

Bestandsadressen

JETZT MITMACHEN UND GEWINNEN:
- 3 Reisegutscheine im Wert von je 1.000 €
- 8 Apple iPads
- 10 Münster-Gutscheine im Wert von je 100 €

Strom sparen und gewinnen –
mit der neuen Zählerstandserfassung in der münster:app!

Sehr geehrter Herr Mustermann,

mit der beliebten App für Münster haben Sie viele Services rund um unsere Stadt buchstäblich in der Tasche. Von der Fahrplanauskunft über den Parkplatzfinder bis zum Terminplaner der Müllabfuhr.

Ab sofort macht der neue **Bereich Mein Stadtwerk** die münster:app für Sie noch besser! Die Funktion „Zählerstandserfassung" ist besonders interessant für alle, die ihren Stromverbrauch im Blick behalten und sparen möchten. Wie sieht mein Stromverbrauch aus? Wie kann ich ihn steuern? Wie werden meine Stromkosten berechenbarer?

Testen Sie jetzt als einer der Ersten die neue Funktion und nutzen Sie die große Chance auf tolle Gewinne! Sie müssen dafür lediglich die App einmal aktualisieren. Sie nutzen die münster:app noch nicht? Dann laden Sie die **App kostenlos im App Store oder bei Google Play** herunter!

Viel Spaß beim Stromsparen und viel Glück beim Gewinnspiel.

Mit freundlichen Grüßen

[redacted]

[redacted]
Marketing

PS: Ihre Teilnahme als einer der ersten Nutzer lohnt sich garantiert, denn Sie können sich zusätzlich zu Ihren Gewinnchancen weitere Gutscheine für den Grünspar-Online-Shop im Wert von 45 € sichern.

Stadtwerke Münster GmbH
Hafenplatz 1
48155 Münster

Fon 02 51.6 94 - 12 34
Fax 02 51.6 94 - 11 11
info@stadtwerke-muenster.de
www.stadtwerke-muenster.de

SERVICE VOR ORT
ServiceCenter, Hafenplatz 1
48155 Münster
Mo bis Fr 9 – 18 Uhr

CityShop, Salzstraße 21
48143 Münster
Mo bis Fr 9 – 19 Uhr
Sa 10 – 18 Uhr

mobilé, Berliner Platz 22
48143 Münster
Mo bis Fr 9 – 19 Uhr
Sa 9 – 14 Uhr

BANKVERBINDUNG
[redacted]

VORSITZENDER DES AUFSICHTSRATES
[redacted]

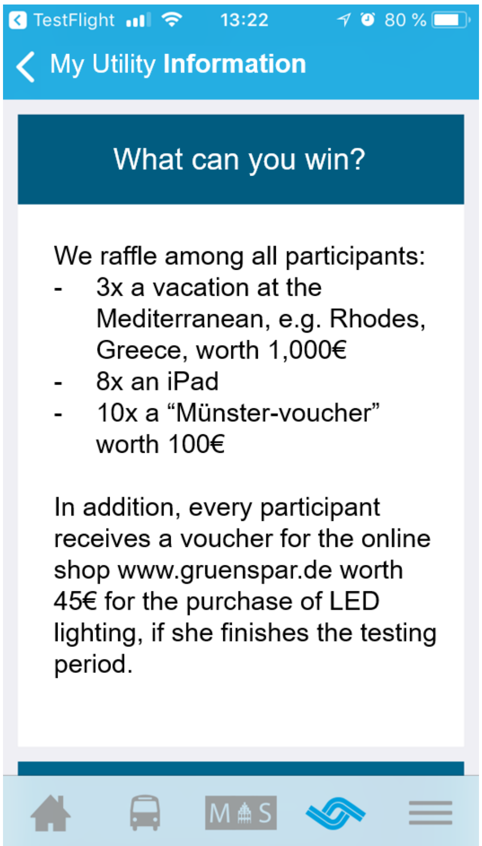
GESCHÄFTSFÜHRUNG
[redacted]

HANDELSREGISTER
[redacted]

Einfach. Näher. Dran.

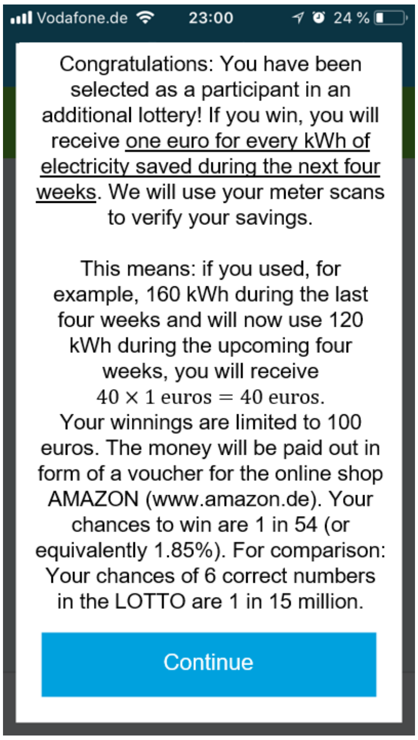
Note: This figure shows the letter that was sent to utility customers during the marketing campaign. Black censor bars hide the CEO's signature and the bank account information of the utility.

Figure A.4: Participation Incentives



Note: This figure is a translated version of the screenshot showing the participation incentives. For the original version in German see Figure A.6.

Figure A.5: Energy Savings Lottery



Note: This figure is a translated version of the screenshot showing the raffle of the energy savings subsidy. For the original version in German see Figure A.7.

Figure A.6: Original Version of Participation Incentives in German

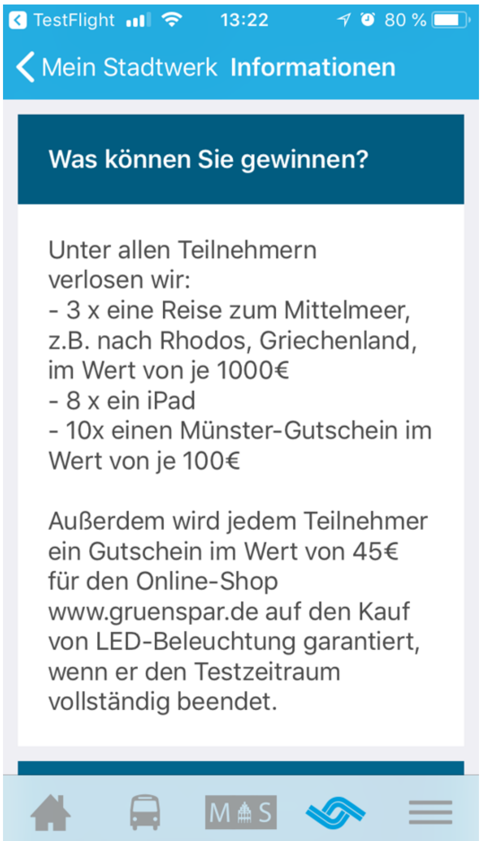
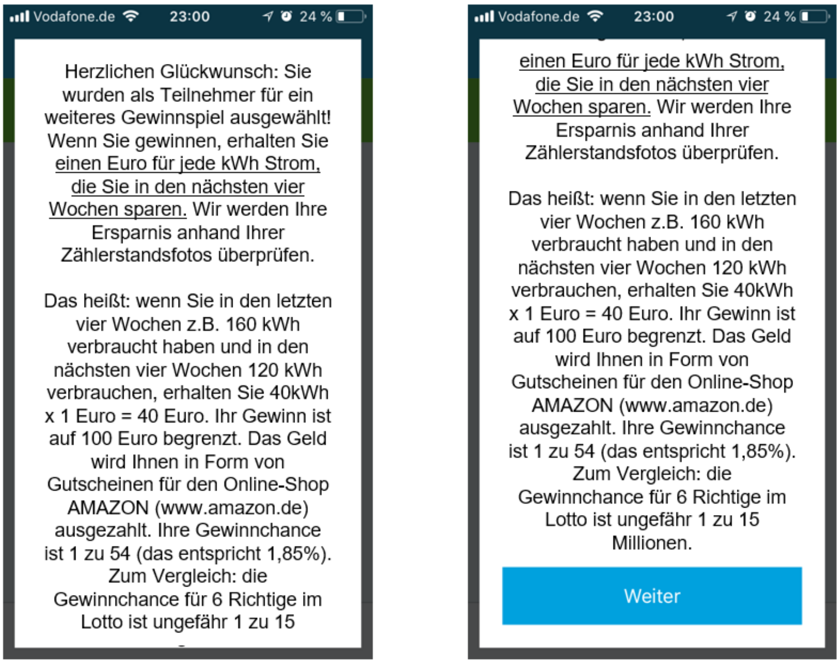


Figure A.7: Original Version of Energy Savings Lottery in German



D Translated Version of Post-Experimental Survey

The following pages show a translated version of the post-experimental survey. The original version in German is permanently stored here: Web Link to German Survey.

Thank you for participating in our survey

Dear participant,

This survey was developed by the University of Münster as part of the EU research project "PENNY" investigating energy use in European households.

You will need about 10 minutes to complete this survey. Upon completion of the survey, you will receive a **voucher of 45€ for the online shop www.gruenspar.de as well as a ticket for the lottery of three trips to the Mediterranean Sea worth 1,000€ each, eight iPads and ten vouchers for local shops worth 100€ each**.

Please complete the survey without pausing. Unfortunately, it is not possible to save the answers temporarily and finish the survey later.

Thank you for participating in this survey!
The project team of the University of Münster

* 1. To continue, please agree to the conditions of participation.

- ◯ I agree with the conditions of participation.

Your apartment/house

* 2. With how many people, including yourself, do you share your apartment/house?

[Drop-down list with answers: 1, 2, 3, 4, 5, 6, 7 or more, No answer.]

* 3. Among these, how many are younger than 14 years?

[Drop-down list with answers: 0, 1, 2, 3, 4, 5, 6, 7 or more, No answer.]

* 4. Has the number of people you share your apartment/house with changed in the last four months?

- ○ Yes, there are more people now.
- ○ Yes, there are less people now.
- ○ No, there are the same number of people.
- ○ No answer.

* 5. How many square meters does your apartment/house approximately have?

- ○ No answer.
- ○ In m²:

* 6. Do you use electricity to generate hot water for showering/bathing or for heating your living areas?

Multiple answers are possible.

- ☐ Yes, for showering/bathing.
- ☐ Yes, for heating.
- ☐ No, for neither of them.
- ☐ No answer.

* 7. How long have you been living in your apartment/house?

- For less than a year.
- For one year.
- For two years.
- For three years
- For four years.
- For more than four years.
- No answer.

Your electricity bill

* 8. How often do you receive your electricity bill?

(Based on the transmission of your electricity meter reading: If you transmit your meter reading to your electricity provider once a year, your bill is also due annually).

[Drop-down list with answers: Monthly, Quarterly, Biannually, Annually, Do not receive an electricity bill.]

* 9. When did you receive your last electricity bill?

[Drop-down list with answers: January, February, March, April, May, June, July, August, September, October, November, December, Do not know., Have not received an electricity bill (yet).]

* 10. Do you regularly make plans regarding the amount of your electricity bill?

- Yes. I usually plan to receive money back from my electricity provider.
- Yes. I usually plan to pay additional money to my electricity provider.
- Yes. I usually plan neither to receive money from nor to pay additional money to my electricity provider.
- No, I do not make plans on the amount of my electricity bill.
- Other:

Your electricity consumption

* 11. Please indicate to what extent the following statements apply to you.

	Never	Rarely	Frequently	Always
I regularly intend to use less electricity.	○	○	○	○
But then, I frequently consume more electricity than I had intended.	○	○	○	○
I try to use less electricity for climate protection reasons.	○	○	○	○

* 12. On a scale from 1 to 7, where 1 means "very bad" and 7 means "very good", how do you feel in the following situation:

	1 (very bad)	2	3	4	5	6	7 (very good)
My electricity costs are 100€ higher than expected.	○	○	○	○	○	○	○

* 13. On a scale from 1 to 7, where 1 means "very bad" and 7 means "very good", how do you feel in the following situation:

	1 (very bad)	2	3	4	5	6	7 (very good)
My electricity costs are 100€ lower than expected.	○	○	○	○	○	○	○

Your electricity consumption

* 14. Have you set yourself a goal on your electricity costs or your electricity consumption in the last three months?

- ◯ Yes.
- ◯ No.
- ◯ I cannot remember.

Your electricity goal

* 15. Why have you set yourself a goal on your electricity consumption/electricity costs?

	Completely disagree	Disagree	Agree	Completely agree
Because my high electricity consumption bothers me.	◯	◯	◯	◯
Because I wanted to challenge myself.	◯	◯	◯	◯

Other reasons:

* 16. On a scale from 1 to 4, how committed did you feel to this goal?

1 (Not committed at all)	2	3	4 (Very committed)
◯	◯	◯	◯

* 17. On a scale from 1 to 5, how much are you bothered if you do not achieve this goal?

1 (Not bothered at all)	2	3	4	5 (Very bothered)
◯	◯	◯	◯	◯

About yourself

* 18. Please indicate how much you agree or disagree with the following statements.

	Completely disagree	Disagree	Neutral	Agree	Completely agree
I often behave as others expect me to.	◯	◯	◯	◯	◯
I am good at seeing the intention of others.	◯	◯	◯	◯	◯

* 19. Please indicate your gender.

◯ Female.

◯ Male.

◯ No answer.

* 20. How old are you?

About yourself

* 21. What is your personal monthly net income approximately?

(*incl. public transfer payments, less taxes and social security contributions).*

- No answer.
- 0–499 euros.
- 500–899 euros.
- 900–1,299 euros.
- 1,300–1,499 euros.
- 1,500–1,699 euros.
- 1,700–1,999 euros.
- 2,000–2,599 euros.
- 2,600–3,199 euros.
- 3,200–4,499 euros.
- 4,500 euros or more.

* 22. What is your highest general schooling degree?

Please assign degrees obtained abroad to an equivalent German degree.

- Graduated after a maximum of 7 years of school attendance.
- Graduated from a Haupt- or Volksschule.
- Graduated from a polytechnic secondary school.
- Graduated from a Realschule or otherwise obtained Mittlere Reife.
- Graduated from high school or otherwise obtained a university qualification.
- No schooling degree.
- Still in education.
- No answer.

* 23. What is your highest vocational training or university degree?

Please assign degrees obtained abroad to an equivalent German degree.

- Vocational preparation year or internship.
- Apprenticeship/vocational training in a dual system *(incl. graduation from a vocational school, preparatory classes for the mid-level service in a public administration, and degree from a 1-year school for health and social professions).*
- Vocational school qualification *(incl. master craftsman/-woman, technician, degree from a 2- or 3-year school for health and social professions, and vocational school for pre-school teachers).*
- Vocational school of the GDR.
- Bachelors.
- Masters.
- Diploma, completed teachers or state examination, artistic degree or comparable degrees.
- Doctorate or PhD.
- No vocational training.
- Still in education/vocational training.
- No answer.

* 24. Considering your current situation, which of the below applies most to you?

If you have interrupted your activity, e.g., due to parental leave, your answers refer to the interrupted activity.

- Unpaid family member helping on the family's holding.
- Self-employed or freelancer.
- Civil servant, judge, professional soldier, pastor, or priest *(incl. in vocational training to become civil servant, temporary soldier, person in voluntary military service).*
- Employee (white collar) *(incl. marginally employed, e.g., 450 euro job, 1 euro job, short-term employment, person in federal voluntary service).*
- Worker (blue collar).
- Apprentice with training remuneration *(incl. volunteer, trainee, person in paid internship).*
- Unemployed.
- Student without (marginal) employment or in vocational training without remuneration.
- Housewife/husband without (marginal) employment.
- Pensioner without (marginal) employment.
- Permanently unable to work.
- No answer.

Your electricity costs

Your electricity contract consists of two elements: A fixed basic price and a consumption price that you pay for each kilowatt hour of electricity. **This question is about your consumption price.**

* 25. We would like to ask you to estimate: How many cents do you pay for each kilowatt hour of electricity?

- ◯ I do not know.
- ◯ In cents: ________

* 26. If you are not completely sure, how much do you think you pay **at least** for each kilowatt hour of electricity?

- ◯ I do not know.
- ◯ In cents: ________

* 27. If you are not completely sure, how much do you think you pay **at maximum** for each kilowatt hour of electricity?

- ◯ I do not know.
- ◯ In cents: ________

Your decision

Please imagine the following situation: You could choose between a safe payment OR a lottery. In the lottery, you would have a **50/50 chance either of losing 150 euros or winning 150 euros**.

We will now show you five different choices between safe payment and lottery. **For each of the decisions, please choose the option that you would find best in such a situation!** There is no right or wrong decision.

Your decision

* 28. What would you prefer?: Would you prefer a lottery with a **50% chance of losing 150 euros** and the same **50% chance of winning 150 euros?** OR would you prefer a safe gain of **0 euros?**

- ◯ 50/50 chance of either winning 150 euros or losing 150 euros. [go to 29]
- ◯ Safe gain of 0 euros. [go to 30]

Your decision

* 29. Would you prefer the 50/50 chance or a safe gain of **80 euros**?

- ◯ 50/50 chance of either winning 150 euros or losing 150 euros. [go to 31]
- ◯ Safe gain of 80 euros. [go to 32]

Your decision

* 30. Would you prefer the 50/50 chance or a safe loss of **80 euros**?

- ◯ 50/50 chance of either winning 150 euros or losing 150 euros. [go to 33]
- ◯ Safe loss of 80 euros. [go to 34]

Your decision

* 31. Would you prefer the 50/50 chance or a safe gain of **120 euros**?

- ◯ 50/50 chance of either winning 150 euros or losing 150 euros. [go to 35]
- ◯ Safe gain of 120 euros. [go to 36]

Your decision

* 32. Would you prefer the 50/50 chance or a safe gain of **40 euros**?

- 50/50 chance of either winning 150 euros or losing 150 euros. [go to 37]
- Safe gain of 40 euros. [go to 38]

Your decision

* 33. Would you prefer the 50/50 chance or a safe loss of **40 euros**?

- 50/50 chance of either winning 150 euros or losing 150 euros. [go to 39]
- Safe loss of 40 euros. [go to 40]

Your decision

* 34. Would you prefer the 50/50 chance or a safe loss of **120 euros**?

- 50/50 chance of either winning 150 euros or losing 150 euros. [go to 41]
- Safe loss of 120 euros. [go to 42]

Your decision

* 35. Would you prefer the 50/50 chance or a safe gain of **140 euros**?

- 50/50 chance of either winning 150 euros or losing 150 euros. [go to 43]
- Safe gain of 140 euros. [go to 44]

Your decision

* 36. Would you prefer the 50/50 chance or a safe gain of **100 euros**?

- 50/50 chance of either winning 150 euros or losing 150 euros. [go to 45]
- Safe gain of 100 euros. [go to 46]

Your decision

* 37. Would you prefer the 50/50 chance or a safe gain of **60 euros**?

- 50/50 chance of either winning 150 euros or losing 150 euros. [go to 47]
- Safe gain of 60 euros. [go to 48]

Your decision

* 38. Would you prefer the 50/50 chance or a safe gain of **20 euros**?

- 50/50 chance of either winning 150 euros or losing 150 euros. [go to 49]
- Safe gain of 20 euros. [go to 50]

Your decision

* 39. Would you prefer the 50/50 chance or a safe loss of **20 euros?**

- 50/50 chance of either winning 150 euros or losing 150 euros. [go to 51]
- Safe loss of 20 euros. [go to 52]

Your decision

* 40. Would you prefer the 50/50 chance or a safe loss of **60 euros**?

- 50/50 chance of either winning 150 euros or losing 150 euros. [go to 53]
- Safe loss of 60 euros. [go to 54]

Your decision

* 41. Would you prefer the 50/50 chance or a safe loss of **100 euros**?

- 50/50 chance of either winning 150 euros or losing 150 euros. [go to 55]
- Safe loss of 100 euros. [go to 56]

Your decision

* 42. Would you prefer the 50/50 chance or a safe loss of **140 euros**?

- 50/50 chance of either winning 150 euros or losing 150 euros. [go to 57]
- Safe loss of 140 euros. [go to 58]

Your decision

* 43. Would you prefer the 50/50 chance or a safe gain of **150 euros**?

- 50/50 chance of either winning 150 euros or losing 150 euros. [end]
- Safe gain of 150 euros. [end]

Your decision

* 44. Would you prefer the 50/50 chance or a safe gain of **130 euros**?

- 50/50 chance of either winning 150 euros or losing 150 euros. [end]
- Safe gain of 130 euros. [end]

Your decision

* 45. Would you prefer the 50/50 chance or a safe gain of **110 euros**?

- 50/50 chance of either winning 150 euros or losing 150 euros. [end]
- Safe gain of 110 euros. [end]

Your decision

* 46. Would you prefer the 50/50 chance or a safe gain of **90 euros**?

- 50/50 chance of either winning 150 euros or losing 150 euros. [end]
- Safe gain of 90 euros. [end]

Your decision

* 47. Would you prefer the 50/50 chance or a safe gain of **70 euros**?

- 50/50 chance of either winning 150 euros or losing 150 euros. [end]
- Safe gain of 70 euros. [end]

Your decision

* 48. Would you prefer the 50/50 chance or a safe gain of **50 euros**?

- 50/50 chance of either winning 150 euros or losing 150 euros. [end]
- Safe gain of 50 euros. [end]

Your decision

* 49. Would you prefer the 50/50 chance or a safe gain of **30 euros**?

- 50/50 chance of either winning 150 euros or losing 150 euros. [end]
- Safe gain of 30 euros. [end]

Your decision

* 50. Would you prefer the 50/50 chance or a safe gain of **10 euros**?

- 50/50 chance of either winning 150 euros or losing 150 euros. [end]
- Safe gain of 10 euros. [end]

Your decision

* 51. Would you prefer the 50/50 chance or a safe loss of **10 euros**?

- 50/50 chance of either winning 150 euros or losing 150 euros. [end]
- Safe loss of 10 euros. [end]

Your decision

* 52. Would you prefer the 50/50 chance or a safe loss of **30 euros**?

◯ 50/50 chance of either winning 150 euros or losing 150 euros. [end]

◯ Safe loss of 30 euros. [end]

Your decision

* 53. Would you prefer the 50/50 chance or a safe loss of **50 euros**?

◯ 50/50 chance of either winning 150 euros or losing 150 euros. [end]

◯ Safe loss of 50 euros. [end]

Your decision

* 54. Would you prefer the 50/50 chance or a safe loss of **70 euros**?

◯ 50/50 chance of either winning 150 euros or losing 150 euros. [end]

◯ Safe loss of 70 euros. [end]

Your decision

* 55. Would you prefer the 50/50 chance or a safe loss of **90 euros**?

◯ 50/50 chance of either winning 150 euros or losing 150 euros. [end]

◯ Safe loss of 90 euros. [end]

Your decision

* 56. Would you prefer the 50/50 chance or a safe loss of **110 euros**?

- 50/50 chance of either winning 150 euros or losing 150 euros. [end]
- Safe loss of 110 euros. [end]

Your decision

* 57. Would you prefer the 50/50 chance or a safe loss of **130 euros**?

- 50/50 chance of either winning 150 euros or losing 150 euros. [end]
- Safe loss of 130 euros. [end]

Your decision

* 58. Would you prefer the 50/50 chance or a safe loss of **150 euros**?

- 50/50 chance of either winning 150 euros or losing 150 euros. [end]
- Safe loss of 150 euros. [end]

Lottery

You are almost done—there are only two questions left!

You can now win money by answering the following two questions. In these questions, you will make decisions between different payment options. Among all participants, some decisions will be selected at random for payment.

The probability that one of your decisions is selected is about 1 to 528. To compare: The chance of winning in the LOTTO is about 1 to 15 million.

If one of your decisions is selected for payment, you will receive the monetary amount of this decision exactly on the said date, paid out as an Amazon e-gift card.

Hence, always remember that whenever making a choice: This choice may really be paid out to you!

Lottery

* 59. For the following monetary amounts, please choose between a payment **in the next 24 hours** OR a payment **in four weeks.**

If one of your decisions is selected payment, you will receive the monetary amount as an Amazon e-gift card on the specified date.

	Amount in the next 24 hours	Amount in four weeks
Do you prefer **100 euros** in the next 24 hours OR **100 euros** in four weeks?	○	○
Do you prefer **100 euros** in the next 24 hours OR **100.20 euros** in four weeks?	○	○
Do you prefer **100 euros** in the next 24 hours OR **100.50 euros** in four weeks?	○	○
Do you prefer 1**00 euros** in the next 24 hours OR **101 euros** in four weeks?	○	○
Do you prefer **100 euros** in the next 24 hours OR **102 euros** in four weeks?	○	○
Do you prefer **100 euros** in the next 24 hours OR **104 euros** in four weeks?	○	○
Do you prefer **100 euros** in the next 24 hours OR **107 euros** in four weeks?	○	○
Do you prefer **100 euros** in the next 24 hours OR **110 euros** in four weeks?	○	○
Do you prefer **100 euros** in the next 24 hours OR **115 euros** in four weeks?	○	○
Do you prefer **100 euros** in the next 24 hours OR **120 euros** in four weeks?	○	○
Do you prefer **100 euros** in the next 24 hours OR **130 euros** in four weeks?	○	○
Do you prefer **100 euros** in the next 24 hours OR **140 euros** in four weeks?	○	○
Do you prefer **100 euros** in the next 24 hours OR **150 euros** in four weeks?	○	○
Do you prefer **100 euros** in the next 24 hours OR **160 euros** in four weeks?	○	○

Lottery

* 60. For the following monetary amounts, please choose between a payment **in four weeks** OR a payment **in eight weeks.**

If one of your decisions is selected payment, you will receive the monetary amount as an Amazon e-gift card on the specified date.

	Amount in four weeks	Amount in eight weeks
Do you prefer **100 euros** in four weeks OR **100 euros** in eight weeks?	◯	◯
Do you prefer **100 euros** in four weeks OR **100.20 euros** in eight weeks?	◯	◯
Do you prefer **100 euros** in four weeks OR **100.50 euros** in eight weeks?	◯	◯
Do you prefer **100 euros** in four weeks OR **101 euros** in eight weeks?	◯	◯
Do you prefer **100 euros** in four weeks OR **102 euros** in eight weeks?	◯	◯
Do you prefer **100 euros in** four weeks OR **104 euros** in eight weeks?	◯	◯
Do you prefer **100 euros in** four weeks OR **107 euros** in eight weeks?	◯	◯
Do you prefer **100 euros in** four weeks OR **110 euros** in eight weeks?	◯	◯
Do you prefer **100 euros in** four weeks OR **115 euros** in eight weeks?	◯	◯
Do you prefer **100 euros in** four weeks OR **120 euros** in eight weeks?	◯	◯
Do you prefer **100 euros in** four weeks OR **130 euros** in eight weeks?	◯	◯
Do you prefer **100 euros in** four weeks OR **140 euros** in eight weeks?	◯	◯
Do you prefer **100 euros in** four weeks OR **150 euros** in eight weeks?	◯	◯
Do you prefer **100 euros in** four weeks OR **160 euros** in eight weeks?	◯	◯

Thank you! The survey is now finished.

A 0.06% **Congratulations, you have won this lottery!**
Your personal winning code is: **XWE987R**. Please send an email to **gewinnspiel@wwu.de** stating this winning code. The members of our project team will then send you your Amazon e-gift card.

B 0.06% **Congratulations, you have won this lottery!**

Your personal winning code is: **OWW895R**. Please send an email to **gewinnspiel@wwu.de** stating this winning code. The members of our project team will then send you your Amazon e-gift card.

C 0.06% **Congratulations, you have won this lottery!**

Your personal winning code is: **TZQ632U**. Please send an email to **gewinnspiel@wwu.de** stating this winning code. The members of our project team will then send you your Amazon e-gift card.

D 99.82% Unfortunately, you did not win this lottery. The project team of the University of Münster thanks you for your participation!

As a thank you for your participation in this survey, you will receive the 45€ voucher for the online shop www.gruenspar.de. In addition, you have collected another ticket for the lottery of three trips to the Mediterranean Sea worth 1,000€ each, eight iPads and ten vouchers for local shops worth 100€ each.