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Authors
Global Action, Center for Effective
Garg, Tevrat
Lemus, Jorge
et al.

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A Comparison of Contests and Contracts to Deliver Cost-Effective Energy Conservation*

Teevrat Garg[†] Jorge Lemus[‡] Guillermo Marshall[§] Chi Ta[¶]

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Abstract

A long-standing economic problem is how to incentivize costly but unobservable effort. Contests and contracts have been used in various settings where output, rather than effort, is contractible. We conduct a field experiment to compare the effectiveness of contests and tiered contracts in promoting energy conservation among households. While both mechanisms achieve similar energy savings relative to a control group (7 to 9 percent reductions), contests reduce energy use at half the cost. We develop and structurally estimate a model of energy consumption based on our experimental data. For the same budget, we show that an optimal contest dominates optimal contracts. We calculate the marginal abatement cost at USD 59.45-76.72/Mt CO_2 not accounting for utility savings or social value of avoided blackouts from peak demand reduction. Our findings contribute to the design of demand-side management policies in the residential electricity sector, particularly in low- and middle-income countries.

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[†]School of Global Policy and Strategy, UC San Diego; teevrat@ucsd.edu

[‡]Department of Economics, University of Illinois; jalemus@illinois.edu

[§]Sauder School of Business, University of British Columbia; guillermo.marshall@sauder.ubc.ca

[¶]Lyndon B. Johnson School of Public Affairs, The University of Texas at Austin; chi.ta@austin.utexas.edu

1 Introduction

Climate change presents significant challenges to global well-being and every credible pathway to limit carbon emissions to levels that would prevent widespread damage requires decarbonizing the electricity sector. Decarbonizing the energy sector in low- and middle-income countries (LMICs) is particularly vital because these countries are likely to drive most of the expansion of global energy demand (Wolfram et al., 2012; Gertler et al., 2016). However, incorporating clean energy sources such as wind, solar and hydro into the existing grid poses enormous challenges including intermittency and transmission constraints that raise wholesale electricity prices (Ryan, 2021).[1] Together, these contribute to blackouts (Jha et al., 2022) with massive economic costs (Allcott et al., 2016).

Although wholesale electricity prices can vary greatly, sometimes even by the hour, retail prices are set well in advance and often highly regulated. In many instances, utilities may be unable to dynamically change electricity prices. In response to these concerns and because electricity markets traditionally clear almost entirely on the supply side, policymakers and utilities are actively promoting demand-side management initiatives, including energy conservation programs in urban households, such as tiered pricing (Ito, 2014), time-varying pricing (Fowlie et al., 2021), behavioral nudges (Allcott and Mullainathan, 2010; Allcott, 2015; Brandon et al., 2017, 2019; Allcott and Kessler, 2019), automation (Blonz et al., forthcoming) and direct "bonus" payments to keep energy use below a target maximum. When effective, such programs can mitigate utility losses by curtailing the need to purchase expensive peak electricity in day-ahead or spot markets, achieve carbon and particulate emissions reductions from peak electricity production (typically coal, oil or gas) and reduce blackouts (Callaway et al., 2018).[2]

Determining a cost-efficient mechanism for incentivizing agents (households) to exert costly, unobservable effort (energy abatement) is a long-standing and open question in economics. In many settings, including Hanoi, Vietnam, where we conduct our study, the principal (Hanoi's utility) observes a noisy performance measure (energy use) that is correlated with an agent's effort (energy conservation). It is an imperfect measure because the principal is unable to observe the effort directly, and households face idiosyncratic shocks beyond their control (e.g., malfunctioning appliance, unexpected travel). Economic theory

[1] Wind, solar and hydroelectric, unlike conventional sources such as coal and oil, cannot always be generated on demand.

[2] An extensive literature has documented the negative external costs of particulate emissions, including those from production of energy from fossil fuels. See Graff Zivin and Neidell (2018) and Aguilar-Gomez et al. (2022) for recent reviews.

suggests that rewarding relative rather than absolute performance may be more effective in delivering aggregate effort, in this case energy conservation (see, e.g. Green and Stokey, 1983). That is, a contest that rewards the best performance relative to other contest participants may achieve more cost-effective energy reductions than individual contracts that reward the absolute savings of each individual household.

Contests and individual contracts create different incentives for household energy conservation. An individual contract provides a predictable reward (e.g., $5 for achieving a 10% reduction), while a contest offers a larger prize (e.g., $100 to the top saver) but introduces strategic uncertainty. Some households may be discouraged by contests if they believe they are unlikely to win. However, a key advantage of contests is that incentives are unaffected by common shocks—events like extreme weather that impact all households—because relative performance remains comparable. In contrast, common shocks can affect individual contracts by making it impossible or too easy for households to meet the reduction target, which can increase the costs for the principal since most households may achieve the reduction target due to a favorable shock rather than effort.

Individual contracts and contests present different administrative challenges and financial implications for the utility. Implementing individual contracts requires the utility to determine appropriate consumption thresholds and corresponding rewards. This creates uncertainty about the total cost of the program, as it depends on the likelihood that households achieve the consumption reduction thresholds, which can be affected by common factors like weather conditions. Organizing a contest, on the other hand, imposes the burden of setting up the competing groups (e.g., grouping households with similar past consumption patterns), monitoring all participants, and determining the winner(s). However, contests offer more financial certainty for the utility. Contests require only a predetermined fixed budget for the prizes, eliminating the risk of over-spending or under-spending that can occur with individual contracts.

In this paper, we examine the effects of contracts and rank-ordered contests on household energy conservation. We address three key questions. First, what are the effects of rank-ordered contests and tiered contracts on energy conservation? Second, how cost-effective are these programs? Finally, under what conditions do contests dominate contracts in terms of energy conservation per unit cost to the utility?

To answer these questions, we first conduct a randomized field experiment during the summer of 2023 in Hanoi, Vietnam. We partner with a state-owned electric utility – EVN Hanoi – and utilize their mobile app as a platform for our energy savings contests and contracts. This collaboration has made it possible for us to recruit around 12,000 households to participate in our study. Then, we randomize the participants into a control and three treatment groups. The first two treatment groups receive contracts of different terms and conditions, while the third treatment group engages in contests. Our treatment period spans 30 days, commencing from July 15, 2023, and concluding on August 13, 2023.

Hanoi is an ideal setting for this experiment. First, Hanoi relies on hydropower (in addition to coal and small amounts of oil), which fluctuates on a daily rather than hourly level, which is the case for solar and wind. Most households in Hanoi and all households in our sample have smart meters, but the information technology systems of the utility only record energy use at the daily level (as opposed to hourly or five-minute intervals in, for example, California). Second, the grid in Hanoi has been increasingly stressed during the summer months, as high temperatures drive up air conditioning usage, coinciding with lower water levels, making demand management a priority for the utility. Finally, the utility has already been deploying low-cost approaches in the form of behavioral nudges. While these have been cost-effective in delivering demand reductions during peak months, they cannot deliver large enough reductions that the utility needs to avoid startup costs associated with coal- and oil-based plants.

We utilize the experimental data along with a reduced-form empirical analysis to estimate the impacts of our experimental contests and contracts. We find that both contests and contracts effectively promote energy conservation. On average, households in the treatment groups reduce their energy consumption by approximately 7% to 9% in comparison to the control group. Importantly, we find that the energy savings persist for at least one week after the end of the experiment before returning to or just below pre-experimental levels. That is, the energy reductions were additional relative to an otherwise identical control group.

Next, we leverage our experimental data to estimate a structural model to recover an optimal contract and compare it with our cost-equivalent contest. This is important because it is not feasible to ex-ante determine the payouts under contests and contracts that would ex-post be equal because it is not clear how many households would achieve the requisite reductions in energy use under contracts. Additionally, our structural model provides a means to assess the welfare implications associated with various energy savings programs.

In our model, households consume energy to approximate an ideal consumption level and dislike paying for energy. Households can be incentivized to reduce energy by individual contracts or contests. In this setting, for a fixed set of parameters and number of players, the comparison of energy consumption for contests versus contracts is generally ambiguous, which is also the case in other frameworks (see, e.g., Green and Stokey, 1983). However, we show that when the contest designer can choose the number of participants in each contest (that is, there is no endogenous entry into contests), then the optimal contest dominates the optimal contract, assuming the expected payment per household in both mechanisms is the same. Specifically, the aggregate consumption of all contest participants is lower than that of the same individuals under individual contracts with average payouts equal to the contest prize. The experimental results discussed above are in line with our model's prediction.

Using the model estimates, we quantify the cost-effectiveness of contests relative to the optimal contract. We also show that the performance gap widens as the average payout increases, with the optimal contest always outperforming optimal contracts per dollar spent. Our model also allow us to recover the first experimental estimate of the short-run price elasticity of demand in Vietnam. We estimate a short-run elasticity of -0.11, which is larger than estimates in the United States (Jessoe and Rapson, 2014; Bollinger and Hartmann, 2020) but smaller than in other LMICs such as India (Mahadevan, 2024).

Finally, we estimate marginal abatement costs of CO_2 emissions under contests and contracts. When ignoring the foregone profit from reducing electricity demand, emissions reductions are achieved at USD 59.5-76.7/Mt CO_2. These are upper-bound estimates since they do not account for other positive externalities from demand management such as reduced blackouts, avoided capital investments in new power plants or importing electricity. Generating reliable estimates for these is challenging but are often quoted as reasons for utilities investing in such programs. We also compute marginal abatement costs considering the foregone profit from reducing electricity demand—from the utility's perspective, these are an indirect cost of the incentive program. When oil is the marginal source of electricity, there is a business case for contests even without accounting for social cost of carbon since the production costs of the oil plant far exceed the average retail electricity price: the marginal abatement cost is negative at USD -85.6/Mt CO_2. When coal is the marginal source of electricity, emissions reductions are achieved at USD 80.5/Mt CO_2.

Our paper builds on two distinct areas of inquiry. First, we provide new evidence on a classic question in the tournaments literature: whether tournaments dominate contracts (Lazear and Rosen, 1981; Green and Stokey, 1983). Some articles have examined similar questions but in other contexts, and not in a large-scale randomized control trial (see, e.g. Knoeber and Thurman, 1994). A strength of our analysis is that the tournament and contract designs faced by participants are randomly assigned and we observe a high-frequency performance measure (i.e., energy use) before, during, and after the competitions. Importantly, relative to prior work, we provide a large-scale field experiment in a typical major urban metropolitan city in an LMIC. Furthermore, our paper relates more broadly to a growing empirical literature on contest design (e.g., Gross, 2017, 2020; Lemus and Marshall, 2021; Bhattacharya, 2021; Lemus and Marshall, 2024).

Second, our paper informs the design of energy conservation policies and the efficiency in incentivizing behavior to manage demand in the context of LMICs. Prior work has examined policies and programs aimed at reducing energy consumption in high income countries (Ito, 2014, 2015; Levinson, 2016; Houde and Aldy, 2017; Fowlie et al., 2018; Ito et al., 2018; Fowlie et al., 2021), but there is a notable dearth of evidence on such programs in LMICs.[3] This is especially crucial since the marginal source of electricity is much more likely to be coal and so the reductions in carbon emissions could be greater (Boomhower and Davis, 2014; Berkouwer and Dean, 2022; Costa and Gerard, 2021; Ta, 2024). Additionally, we provide the first experimental estimates of the short run price elasticity of demand for electricity in Vietnam and amongst the few that exist for LMICs. Such estimates are crucial to planning effective grid management.

The paper is organized as follows: Section 2 describe our randomized field experiment. Section 3 examines the empirical specifications and findings regarding the impacts of our experimental contracts and contests on electricity consumption, including their heterogeneous effects. Section 4 describes our structural model and the estimation process. Section 5 assesses the marginal abatement costs of energy conservation programs. Finally, Section 6 concludes.

2 The Experiment and Data

2.1 Background and context

We conduct our experiment in the city of Hanoi which is situated in the northern part of Vietnam. Hanoi experiences four seasons with the hottest months being June through

[3] A number of papers have also evaluated the effects of behavioral nudges such as peer comparisons on electricity consumption (Allcott and Mullainathan, 2010; Allcott, 2015; Brandon et al., 2017, 2019; Allcott and Kessler, 2019). These interventions are highly cost-effective at delivering reductions of approximately 1%. Our work complements these existing approaches that are already in place by testing contract designs that deliver higher aggregate demand reductions over and above conservation from nudges.

September where maximum temperatures exceed 35°C (95°F). Rising temperatures and demand for air conditioning during these months create complications for the utility which is increasingly concerned about meeting demand. To avoid blackouts and reduce expensive peak electricity procurement, EVNHANOI, the only utility in Hanoi, has already been implementing low-cost demand side management programs that employ behavioral nudges and moral suasion to reduce energy use during these months. However, these programs, while highly cost-effective, are unable to achieve large-scale energy savings. The utility is particularly interested in incentivizing consumers to reduce energy consumption during these months because the regulator does not allow the utility to employing dynamic pricing, ostensibly to protect consumers from volatile pricing.

2.2 Experimental Design

In the summer of 2023, we conducted a randomized field experiment in the context of a residential energy conservation program in Hanoi, Vietnam. Collaborating with EVNHANOI, the exclusive electricity provider in the city, we advertised our program and recruited participants through different channels, including the utility's official website, the utility's app, and offline marketing. Given our emphasis on advertising through banners and ads within the utility's app, the majority of our study's sample consists of households that use the app to monitor energy usage and pay bills.[4]

During the enrollment period, which ran from June 15th, 2023, to July 6th, 2023, a total of 16,365 households signed up for the experiment. Subsequently, we narrowed down the pool of households using the criteria specified in our pre-analysis plan, resulting in a final cohort of 11,194 participants (Garg et al., 2023). These criteria primarily served the purpose of eliminating outliers and households with extensive missing or zero daily energy consumption data.

We randomized each participating household into one of four groups: three treatment groups and one control group. Two treatment groups were assigned to contracts, with each group differing in the thresholds of energy savings they needed to reach to win a prize. The third treatment group was assigned to contests. The duration of the treatments was 30 days, from July 15th, 2023, to August 13th, 2023. The control group was not assigned to a contest or contract. Participants could use their smart meters to monitor their progress by

[4]EVNHANOI, has over 2.8 million customers, and all of them have smart meters. About 25% of all households in Hanoi have installed the utility's app.

default, so all households, including the control group, received information about their past and current daily electricity use on the utility company's app.

The groups were as follows:

- Treatment 1, Contract with low thresholds (henceforth, 'Contract 1'). This group was offered \$4.35 USD if they conserved 5% of electricity compared to their average daily energy use during the same treatment period in the previous year, \$6.52 if they conserved 10%, and \$10.87 if they conserved 15%. This group also received weekly text message reminders, saying "There are [insert number] days left in the contract which ends on [insert end date]. Check the app to see your energy savings."
- Treatment 2, Contract with high thresholds (henceforth, 'Contract 2'). This group was offered \$6.52 USD if they conserved 10% of electricity compared to their average daily energy use during the same treatment period in the previous year, \$10.87 if they conserved 15%, and \$15.22 if they conserved 20%. This group also received weekly text message reminders, saying "There are [insert number] days left in the contract which ends on [insert end date]. Check the app to see your energy savings."
- Treatment 3, Contest (henceforth, 'Contest'). Households were entered into contests of 50 households. In every contest, the household that conserved the most energy, compared to their average daily energy use during the same treatment period in the previous year, was to receive a prize of \$87. This group also received weekly text message reminders, saying "There are [insert number] days left in the contest which ends on [insert end date]. Check the app to see your energy savings."
- Control group, No contest or contract participation. This group was not offered any incentive to conserve energy. This group received weekly text message reminders, saying: "Please check the app to see your energy savings."[5]

Households assigned to the contest treatment were randomized into groups based on their average consumption in the period between July 15, 2022, and August 13, 2022 (i.e., the comparison period for the experimental period) to ensure that contest participants were competing with households that were similar in energy consumption.

[5]To avoid dissatisfaction and exclusion, we pay out a small amount of about \$0.40 USD to participants selected in the control group and thank them for enrolling in the program after the program ends. We did do not inform them about this payment until after the program ended.

The contests and contracts started on July 15, 2023, and ended on August 13, 2023. After completing our recruitment, registration, and randomization, on July 15, 2023, households were scheduled to receive individual information about their specific treatment or energy savings program through the app display as well as notification that the incentive period had started. The experiment experienced unexpected delays due to technical issues. Households needed to update the app to view the specific rules for their treatment. All households received individual information about their treatment via a text message containing a link to the rules on July 24, 2023.[6] In these communications, households were not informed about the presence of other treatments within our study. On August 17, 2023, the utility sent text messages to households in the treated group to inform them about the program's culmination and express gratitude for their participation. The utility also informed participants that the results of contracts and contests will be communicated through app notifications and text messages within the following 10 days.

We chose this structure of incentives for the treatment groups for two main reasons. First, our ideal comparison between a contest and a contract fixes the expected payment received by a household. Without knowing in advance the weather that households will face, any prediction of the expected payment of a contract is uncertain (recall that contracts give rewards contingent on achieving certain levels of energy savings against a pre-specified benchmark – in our case the energy consumption in the same period the previous year). The same is not true for contests, which are fully predictable in expected payments, as we know the winner's payment and that there will always be a winner. Assigning two treatment groups to a contract allowed us to ex-ante increase the chances of comparing a contest and a contract with similar expected payments.[7]

Second, making the contracts have tiers (i.e., different payments for achieving different levels of savings) ex-ante increases the chances that the contracts will provide households with marginal incentives (i.e., a non-trivial tradeoff with costs and benefits to saving energy created by our incentive program). To see this, imagine a scenario in which the experimental period is significantly cooler than the reference period. Saving 5 percent may be achieved without effort, but saving 15 percent may require significant effort. If instead, the weather is warmer during the experimental period, households may find it too costly to save more than 5 percent. Having a contract with several tiers thus increases the chances

[6] Examples of the treatment rules, which are displayed in the app and available through a link in text messages, can be found in online Appendix A. This appendix includes the rules in both the local language and their English translation.

[7] Given our sample size, our power calculations suggested that no more than three treatment groups were prudent. With a greater sample size, however, we would have added additional contract treatments.

that the contract will provide marginal incentives, regardless of the weather.

There are two departures from our pre-specified research plan that are worth mentioning, although neither affects the validity of our estimates. First, the notification about the start of the program was delayed by 10-12 days which resulted in a delayed start of the program. Second, in the summer of 2023, during our experiment, the electric utility sent numerous text messages and notifications to all customers, urging them to save energy to protect the power grid. On average, each customer received 2-3 messages per week. In effect, our treatment effects could be interpreted as net-of or over and above effects from standard nudges.

2.3 Data

The main variable of interest is daily electricity consumption at the household level. This variable is obtained from the utility company, which measures electricity consumption through smart meters installed in every home. We collect daily electricity consumption data at the household level for 12 months prior to the start of the experiment and six months following its conclusion.[8] As noted before, data on a household's electricity consumption within a day (e.g., hourly data) is unavailable as the current IT systems for the utility in Hanoi do not store such data.

Weather plays a significant role in influencing a household's electricity consumption and their likelihood of winning a prize in a contract or contest. As a result, we gather daily air temperature data for Hanoi from Visual Crossings. This dataset encompasses the air temperature variable, along with a "feels like" temperature variable, which takes into account temperature and humidity to provide a more accurate representation of the perceived outdoor temperature. We utilize these data to study heterogeneous responses by weather conditions on a given day.

To assess the cost-effectiveness and welfare impacts, we also obtain administrative data from the utility, allowing us to quantify the benefits of energy savings in terms of reduced energy production, carbon emissions, and the prevention of blackouts and system failures.

[8]Our attrition rate is notably low, with only 8 out of 11,194 participants discontinuing their involvement. Attrition occurred since those 8 participants stopped their service with the utility. Also, due to intermittent technical issues, the daily consumption of some households is sometimes not transmitted to the utility immediately although it is accounted for in the billing cycle. We drop these small number of household-day combinations for which this occurs and importantly, these are balanced across all treatment and control groups.

Table 1: Balance analysis: Past electricity consumption

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Control	Treatment 1		Treatment 2		Treatment 3		F-test
Month	Mean (kWh)	Coeff.	p-value	Coeff.	p-value	Coeff.	p-value	p-value
July 2022	12.388	0.233	0.164	0.111	0.500	0.130	0.430	0.581
August 2022	11.488	0.211	0.170	0.160	0.295	0.154	0.312	0.543
September 2022	10.621	0.140	0.329	0.134	0.350	0.116	0.413	0.733
October 2022	8.441	0.077	0.482	0.123	0.260	0.099	0.366	0.697
November 2022	8.324	0.079	0.462	0.131	0.222	0.133	0.215	0.562
December 2022	8.601	0.097	0.423	0.164	0.174	0.072	0.549	0.594
January 2023	8.814	0.114	0.377	0.223	0.081	0.027	0.827	0.294
February 2023	8.762	0.086	0.480	0.134	0.265	0.079	0.512	0.733
March 2023	8.423	0.116	0.309	0.119	0.286	0.055	0.619	0.677
April 2023	9.053	0.026	0.832	0.168	0.173	0.070	0.566	0.541
May 2023	11.447	0.120	0.439	0.235	0.130	0.214	0.166	0.410

Notes: An observation in each row is a household. Columns 2-7 report the coefficients and p-values from OLS regressions of average daily consumption on three indicators: treatment 1, treatment 2, and treatment 3. Column 8 reports the p-value from a joint test of statistical significance of all three indicators.

2.4 Experimental Balance

We assess the balance between the treatment and control groups by examining household historical electricity consumption data. More precisely, we analyze the average daily electricity consumption for each month leading up to the intervention, spanning from July 2022 to May 2023, as part of our balance checks. For each of these variables, we run the following specification:

$$y_i = \alpha + \sum_{k=1}^{3} 1\{\text{treatment}_i = k\}\beta_k + \varepsilon_i,$$

where treatment$_i$ is a variable indicating the treatment assignment of household i. The regression includes indicators for all treatment groups except for the control group (the omitted category). In our balance analysis, we report estimates for the coefficients $\{\beta_k\}$, their standard errors, and the p-value from a joint test of statistical significance of all coefficients on the treatments indicators (i.e., a test where $H_0 : \beta_1 = \beta_2 = \beta_3 = 0$) for every variable listed above. Table 1 presents the outcomes of our balance checks, showing no noticeable disparities in historical electricity consumption patterns between the control and

Table 2: Treatment effects: Cross-sectional variation

	(1)	(2)	(3)	(4)
	Daily consumption (kWh)		Daily consumption (kWh) (in logs)	
Contract 1	-0.763	-0.914	-0.071	-0.085
	(0.180)	(0.087)	(0.015)	(0.009)
Contract 2	-0.538	-0.794	-0.054	-0.074
	(0.182)	(0.089)	(0.015)	(0.009)
Contest	-0.629	-0.835	-0.055	-0.072
	(0.182)	(0.093)	(0.015)	(0.009)
Controls	No	Yes	No	Yes
Observations	329752	329192	326283	325724
Mean	12.998	12.999	2.368	2.368
Test	0.454	0.346	0.441	0.272

Notes: Standard errors clustered at the household level in parentheses. All specifications include day fixed effects. Columns 2 and 4 include controls for the average daily consumption of the household in each of the months before the experiment (July 2022 to May 2023). Row 'Mean' reports the mean of the dependent variable in the estimation sample. Row 'Test' reports the two-sided p-value of an F-test where the null is that treatments 1, 2, and 3 have equal coefficients.

treatment groups.

3 Experimental Results

In our study, consenting households opted in to participate in the summer energy conservation program and were subsequently randomized into a control group, two tiered contracts and contests. Thus, we estimate average treatment effects on the households interested in participating in our study.

To measure these treatment effects, we use two different sources of variation. First, we exploit the random cross-sectional variation in treatment assignment during the experimental period and run the following regression:

$$y_{i,t} \quad = \quad \alpha + \sum_{k=1}^{3} 1\{\text{treatment}_i = k\}\beta_k + X_i'\delta + \gamma_t + \varepsilon_{i,t}, \tag{1}$$

where $y_{i,t}$ is the daily consumption of household i on day t during the study period, X_i is a set of covariates (one specification includes no covariates, another specification includes the covariates used in the balance analysis), γ_t is a day fixed effect, and $\varepsilon_{i,t}$ is an error term clustered at the household level.

Table 2 presents the estimates for equation (1). In columns 1 and 2, the dependent variable is daily energy consumption in levels, whereas in columns 3 and 4, the dependent variable is the natural logarithm of daily energy consumption.[9] All specifications include day fixed effects. Columns 2 and 4 include controls for the average daily consumption of the household in each of the months before the experiment (July 2022 to May 2023). The results suggest that households participating in contracts and contests reduce energy use by approximately 5% to 9% compared to households in the control group. All coefficients are statistically significant at the 1% significance level. While both contracts and contests achieved energy reductions that are statistically different from the pure control group, we cannot reject the null hypothesis that the effects of contracts and contests are identical.[10]

Next, we exploit the within-household week-by-week variation in incentives to conserve energy utilizing energy consumption data from before, during, and after the experimental period. We estimate the following equation:

$$y_{i,t} \quad = \quad \alpha + \sum_k \sum_t 1\{\text{treatment}_i = k\}1\{t = \tau\}\beta_{k,\tau} + \gamma_t + \psi_i + \varepsilon_{i,t}, \tag{2}$$

where $y_{i,t}$ is daily energy use of household i on day t, $t \in \{-\bar{T}, -\bar{T}-1, \ldots, 0, 1, \ldots, \bar{T}\}$ periods relative to the beginning of the study, and $\beta_{k,\tau}$ measures the average impact of treatment k on electricity consumption τ periods relative to the beginning of the study (where the control group is the excluded category), γ_t and ψ_i are day and household fixed effects, respectively, and $\varepsilon_{i,t}$ is an error term clustered at the household level. Note that given the within-household variation in incentives to conserve energy, equation (2) includes household effects.

Table 3 shows the estimates of equation (2), using data from before and during the experimental period. We restrict all pre-treatment coefficients $\beta_{k,\tau}$ to zero, and all post-treatment coefficients to a single time-invariant value, β_k. Columns 1 and 2 report estimates in kWh whereas Columns 3 and 4 report results in logs. Column 1 and 3 consider the full sample where as Columns 2 and 4 demonstrates robustness to limiting our sample from June 1,

[9]Less than 0.1% of household days have have zero recorded energy consumption so we obviate the need for adjustments for logs with zeros (Chen and Roth, 2024).

[10]The row 'Test' reports the two-sided p-value of an F-test where the null is that treatments 1, 2, and 3 have equal coefficients.

Table 3: Treatment effects: Within-household variation

	(1)	(2)	(3)	(4)
	Consumption (kWh)		Consumption (kWh) (in logs)	
	Full sample	June 1, 2023 –	Full sample	June 1, 2023 –
Post * Contract 1	-0.892	-0.969	-0.080	-0.085
	(0.100)	(0.073)	(0.008)	(0.006)
Post * Contract 2	-0.740	-0.944	-0.077	-0.078
	(0.101)	(0.074)	(0.008)	(0.006)
Post * Contest	-0.756	-0.938	-0.072	-0.081
	(0.104)	(0.075)	(0.008)	(0.006)
Observations	4430382	718792	4397592	711137
Mean	10.313	13.084	2.131	2.373
Test	0.236	0.910	0.606	0.523

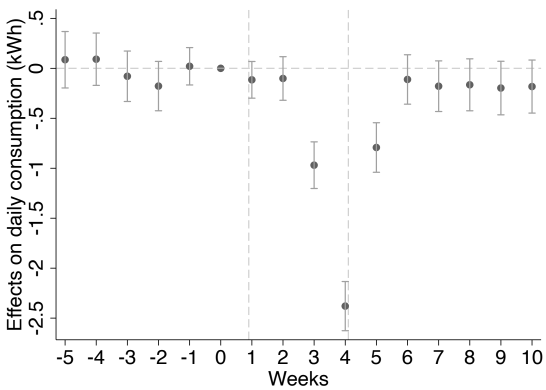
Notes: Standard errors clustered at the household level in parentheses. All specifications include day fixed effects and household fixed effects. Row 'Mean' reports the mean of the dependent variable in the estimation sample. Row 'Test' reports the two-sided p-value of an F-test where the null is that treatments 1, 2, and 3 have equal coefficients. Columns 1 and 3 use the full sample (July 1, 2022 to August 13, 2023). Columns 2 and 4 restrict the sample from June 1, 2023 to August 13, 2023.

2023 onward since households in Hanoi experienced rolling blackouts in May 2023 and consumption is higher in the summer months. The findings remain consistent across various specifications, indicating that households engaging in contracts and contests reduce their energy consumption by around 7% to 9% when compared to households in the control group. All coefficients exhibit statistical significance at the 1% level. Similar to the results presented in Table 2, we cannot reject the null hypothesis that the effects of contracts and contests are equal.[11]

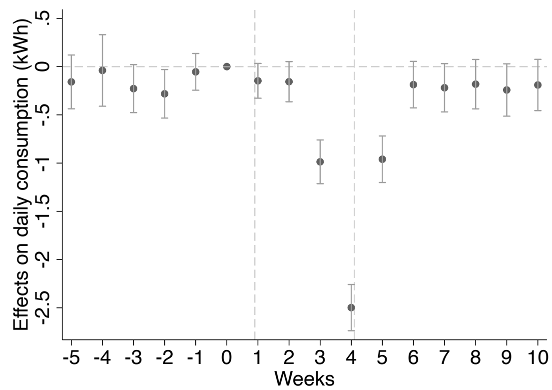
Figure 1 presents estimates for equation (2), where we allow for the treatment coefficients to vary over time, using data from after the experimental period. Figure 1A illustrates the difference in energy usage (in kWh) between treatment group 1 (contract with low thresholds) and the control group over time, as measured by week-level indicators. Likewise, Figure 1B and 1C display the energy usage difference (in kWh) for treatment group 2 (contract with high thresholds) and 3 (contests), respectively, relative to the control group across time. All model specifications incorporate day fixed effects and household fixed effects. The dataset covers the period from June 1, 2023, to September 22, 2023. Week 0

[11] According to our power calculation provided our pre-analysis plan, it is highly probable that our sample size is inadequate for detecting any difference of 3% or less in magnitude.

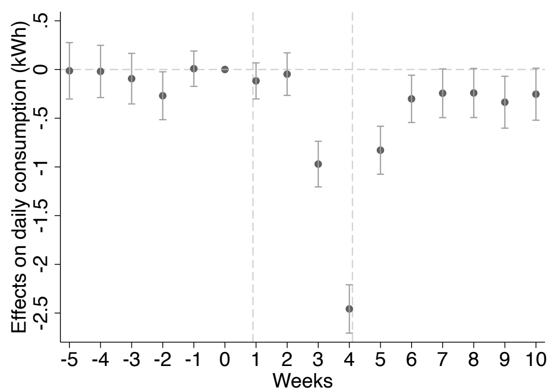
Figure 1: Time effects: Within-household variation



A) Contract 1 vs Control



B) Contract 2 vs Control



C) Contest vs Control

Notes: Standard errors clustered at the household level in parentheses. An observation is a household–day combination. Each figure plots the differential energy use (in kWh) of the treatment group X relative to the control group over time, measured by indicators at the week level. All specifications include day fixed effects and household fixed effects. The sample includes data from June 1, 2023 until September 22, 2023. Week 0 is the week before the experiment started, week 1 is the first week of the experiment and week 4 is the last one. Weeks -6 and -5 are grouped together, given the sample restriction. Week 4 has 3 additional days, to cover the entire experimental period.

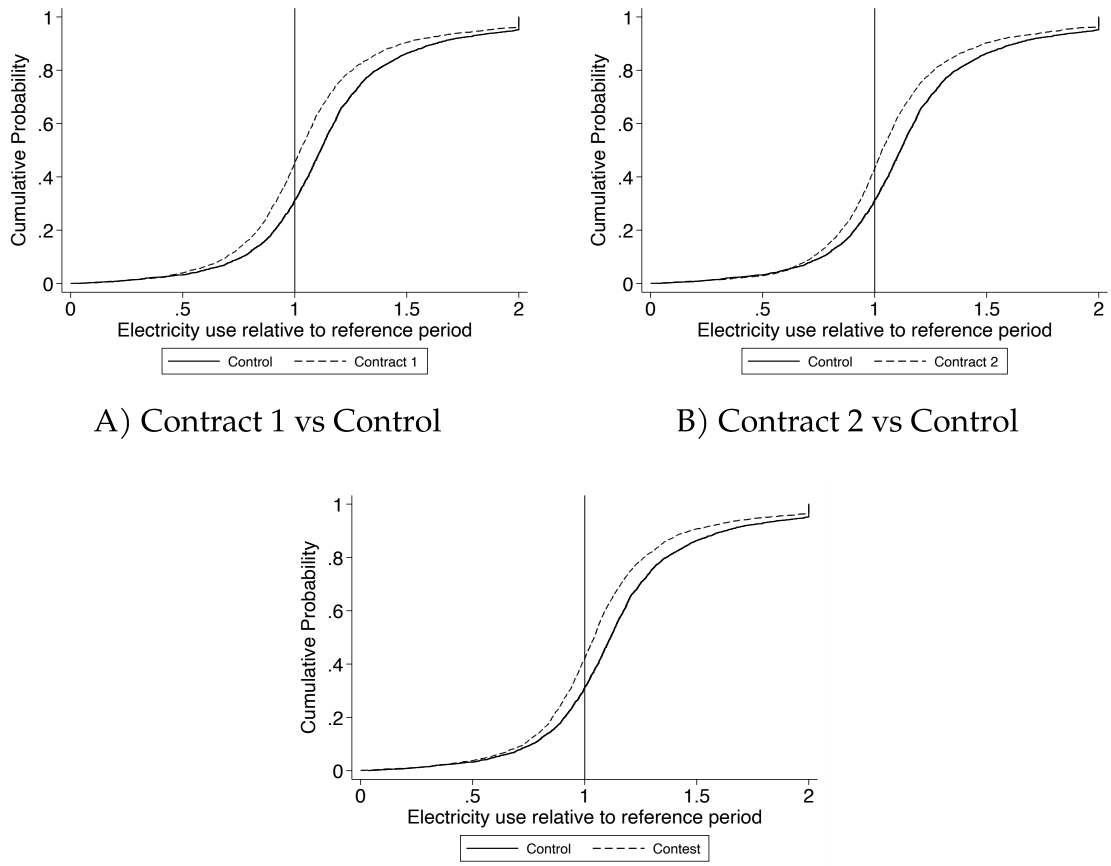
represents the week before the experiment commencement, and weeks 1 through 4 correspond to the experiment period (July 15 through August 13).[12]

The coefficients observed before the treatment period affirm the balance between the control and treatment groups, providing limited evidence of statistically significant differences in daily energy consumption across these groups before the experiment. Similarly, the coefficients two weeks after the experiment started are not statistically significant, implying that the treatments do not exhibit any immediate effect within the initial two weeks. As previously discussed, households received a delayed notification that the incentive period had started (on July 24, 2023, instead of on the first day of the incentive period, July 15, 2023). This most likely explains the null effect in the first two weeks. The treatment effects begin to emerge and become more pronounced in week 3, with the most substantial effects occurring during the final week of treatment. The results suggest that as households approached the conclusion of their contracts or contests, they intensified their efforts to enhance their chances of winning a prize. Although most of the treatment effects dissipate two weeks after the end of the treatment period, we observe some evidence of a small persistent effect. The lower bound of the 95% confidence interval on this estimate is just above pre-treatment consumption for contracts and just below pre-treatment consumption for contests with point estimates suggesting a modest persistent effect.

Also noteworthy in Figure 1 (and Tables 2 and 3) is that the treatment effects are statistically indistinguishable across treatment groups. As we will note later, this is despite the expected payment of households in the contract treatment groups being 80 to 85 percent greater than that of households in the contest group. A few explanations could rationalize this finding. First, it is possible that the treatment effects are different, but we do not have the statistical power to detect such small differences precisely. Given the precision of our treatment effects relative to the control group, it is unlikely that such undetected differences across treatments are economically meaningful. Second, it is possible that households make discrete choices to conserve energy when offered an incentive and exert fixed levels of effort (e.g., change the temperature setting in the AC unit by 1 degree) rather than responding to marginal incentives (i.e., fine tuning effort based on the perceived costs and benefits of saving an additional kWh). Third, it is possible that there are local limits to conservation and beyond a certain level, additional incentives will not deliver any further conservation. Our experiment was not designed to test these different possibilities and instead focused on the cost-effectiveness of different incentive structures. We explore het-

[12]Week 4 has 3 additional days, to cover the entire experimental period. Similarly, weeks -6 and -5 are combined, as week -6 includes only two days, given the sample restriction.

Figure 2: Household-level electricity consumption reductions, by treatment



A) Contract 1 vs Control

B) Contract 2 vs Control

C) Contest vs Control

Notes: An observation is a household. Each figure plots the distribution of the ratio between a household's average daily consumption during the experimental period (July 15, 2023 through August 13, 2023) and the household's average daily consumption during the reference period (July 15, 2022 through August 13, 2022). For presentation purposes, we cap the ratio at 2.

erogeneity and cost-effectiveness in subsequent sections.

To summarize, we find evidence that incentive programs (contracts and contests) reduced electricity consumption by 7-9% relative to a control group, but we do not detect meaningful differences across the three different incentive programs. Depending on the specification, the household level treatment effects range between reductions of 21 kWh to 33 kWh.

3.1 Heterogeneity analysis

In this section, we explore heterogeneity along two dimensions. First, we ask how the energy use reductions are distributed across participants within a treatment group. Second, we examine how variation in temperature shapes heterogeneous treatment effects.

To explore how the energy use reductions are distributed across participants, we compute the ratio between the average daily consumption during the experimental period (July 15 to August 13, 2023) and the average daily consumption during the reference period (July 15 to August 13, 2022). A ratio of one or less indicates that the household's energy use during the experimental period was less or equal to the energy use during the reference period. Figure 2 displays the cumulative distribution functions of these ratios, by treatment. The distribution functions for the treatment groups are smooth and appear to be first-order stochastically dominated by that of the control group, suggesting that the incentives to save energy influenced all treated households. The figures also suggest that the energy reductions we find in Figure 1 are not driven by a subset of households, as the distribution functions of the treated groups depart uniformly from that of the control group.

Table 4 replicates our within-household analysis in Table 3 but allowing for heterogeneous effects, where all interaction variables are standardized (mean zero and standard deviation one). Column 1 shows that treated households with a larger daily average consumption during the reference period, on average used less electricity during the experimental period, but the effect disappears when looking at percentage change in electricity use (Column 4). That is, households that use more can reduce more in levels but the reduction is similar to other households when measured in percentage terms.

Table D.1 in the Online Appendix shows the results of a similar heterogeneity analysis where we exploit that there was a delay in notifying participants that the incentive program had started. We compute the average daily consumption in the first two weeks of the experimental period and exclude those two weeks from the regression. Similar to the results in the previous paragraph, we find that treated households that used more in the first days of the experimental period (before knowing that the experiment had started) reduced their energy consumption by more, but this reduction is no different from that of other households when measured in percentage terms. This suggests that a greater consumption in the early days of the incentive program did not discourage participants from saving energy later in the experimental period.

How does the weather impact the effectiveness of the incentives? Table 4 shows that house-

Table 4: Heterogeneity analysis: Within-household variation

	(1)	(2)	(3)	(4)	(5)	(6)
	Consumption (kWh)			Consumption (kWh) (in logs)		
Post * Contract 1	-0.967	-1.227	-1.435	-0.085	-0.110	-0.126
	(0.073)	(0.114)	(0.103)	(0.006)	(0.009)	(0.009)
Post * Contract 2	-0.946	-1.278	-1.462	-0.078	-0.108	-0.121
	(0.074)	(0.114)	(0.103)	(0.006)	(0.009)	(0.008)
Post * Contest	-0.938	-1.348	-1.528	-0.081	-0.118	-0.130
	(0.075)	(0.116)	(0.106)	(0.006)	(0.009)	(0.009)
Post * Contract 1 * Reference consumption	-0.297			-0.007		
	(0.063)			(0.005)		
Post * Contract 2 * Reference consumption	-0.156			0.001		
	(0.057)			(0.004)		
Post * Contest * Reference consumption	-0.244			-0.002		
	(0.069)			(0.005)		
Post * Contract 1 * Feels like max		0.311			0.030	
		(0.103)			(0.007)	
Post * Contract 2 * Feels like max		0.401			0.036	
		(0.103)			(0.007)	
Post * Contest * Feels like max		0.492			0.044	
		(0.104)			(0.007)	
Post * Contract 1 * Temp max			0.625			0.055
			(0.093)			(0.007)
Post * Contract 2 * Temp max			0.695			0.057
			(0.093)			(0.007)
Post * Contest * Temp max			0.791			0.065
			(0.095)			(0.007)
Observations	718792	718792	718792	711137	711137	711137
Mean	13.084	13.084	13.084	2.373	2.373	2.373
Test	0.923	0.582	0.666	0.529	0.531	0.524

Notes: Standard errors clustered at the household level in parentheses. All specifications include day fixed effects and household fixed effects. Row 'Mean' reports the mean of the dependent variable in the estimation sample. Row 'Test' reports the two-sided p-value of an F-test where the null is that treatments 1, 2, and 3 have equal coefficients. All columns restrict the sample from June 1, 2023 to August 13, 2023. The variables 'Feels like max' (maximum feels like temperature), 'Temp max' (maximum temperature), 'Reference consumption' (household's average daily consumption during July 15, 2022, and August 13, 2022) are standardized (mean zero, standard deviation one).

holds consume more energy on warmer days (measured by the maximum daily "feels like" or the actual maximum temperature). This suggests that households adjust their usage to align with an ideal level of comfort. Despite increased consumption on warmer days, households still save energy relative to the counterfactual under the incentive programs. Our estimates suggest that, on average, a one standard deviation increase in the maximum temperature reduces the daily energy savings by between 0.3 and 0.8 kWh. Moreover, the treatments generated energy savings even on the hottest days of the treatment period (roughly a 1.8 standard deviation increase in temperature), which is when the utility needs consumption reductions the most. These findings suggest that the incentive programs are effective in reducing emissions and managing demand on extremely hot days, which in turn can prevent blackouts.

3.2 Average payouts, by treatment

In this subsection, we analyze the cost of implementing each of these treatments in terms of average payouts per household. Although we have found that we cannot reject that the treatment effects across treatment groups are equal, the cost of each treatment may differ, creating differences in the cost effectiveness of each intervention.

Table 5 summarizes the details of each treatment together with the average payout per participant. The table shows that both contracts were similarly costly in terms of average payout per household ($3.14 and $3.21, respectively, for contracts 1 and 2), and they were 80 to 85% more costly than the contest treatment (average payout of $1.74). This implies that although the reductions in energy use were similar across the incentive programs, the contest achieves these reductions for the least amount of money, suggesting that contests are a substantially more cost-effective way of incentivizing households to reduce energy demand.

4 Structural Model and Estimation

In this section, we introduce a model to rationalize households' optimal energy consumption choices under different incentive structures: no incentives (control group), individual contracts, or contests. Using data from our experiment, we estimate the model parameters by comparing observed outcomes to the model's predictions.

Table 5: Average Payouts and Consumption by Treatment

	Contract 1			Contract 2			Contest
Minimum Reduction to Earn Reward	5%	10%	15%	10%	15%	20%	–
Prize (USD)	$4.34	$6.52	$10.86	$6.52	$10.86	$15.22	$87
Observed winning probability	36%	29%	22%	26%	20%	15%	2%
Number of participants		2,795			2,799		2,799
Average payout per participant (USD)		$3.14			$3.20		$1.74
Average monthly consumption (kHW)		375.61			381.66		379.76

Notes: An observation is a household. The threshold and prize rows show the prizes awarded for saving more than x%. There are no pre-determined thresholds for the contest treatment. The average monthly consumption of the control group during the experimental period was 396.76.

The structural model complements our experimental results in two important ways. First, it allows us to compare the performance of a contest against an *optimal* contract. Using data from our experiment, we are able to compare the performance of a contest and two contracts that are potentially sub-optimal because their reward structure was chosen before the experiment under limited information. Our structural model allows us to find an optimal contract for a given set of primitive parameters, which we use to provide an upper bound on how well contracts can do. Second, the structural model allows us to compute demand functions—both aggregate demand and individual household demand based on average consumption—and to evaluate the cost-effectiveness of contests and contracts under different conditions (e.g., budget, weather).

4.1 Modeling Household Energy Consumption

A household's *ideal* energy consumption is $S \geq 0$. The household chooses its energy consumption, $e \geq 0$, which is then affected by a shock, $\varepsilon \sim F(\cdot) \equiv N(0, \sigma^2)$, so the *actual* energy consumption is $\hat{e} = e + \varepsilon$.[13] The household's expected payoff is

$$E_{\varepsilon}[-\gamma(\hat{e} - S)^2 - p\hat{e}].$$

This payoff captures that the household values matching its actual consumption with its ideal consumption but dislikes paying for energy, which is priced at p per kWh. The parameter γ measures the importance of matching the ideal consumption relative to the cost of energy. Simple algebra shows that, ignoring a constant σ^2, the household payoff can be

[13] That is, the shock realizes *after* the choice of e.

written as

$$-\gamma(e - e_0^*)^2,$$

where $e_0^* = S - \frac{p}{2\gamma}$. We have the following result.

Proposition 1 (No Incentives; Control Group)**.** *Without an incentive for energy reduction, a household's energy consumption* (*assuming an interior solution*) *is given by*

$$e^*_{\mathit{control}} = S - \frac{p}{2\gamma}. \tag{3}$$

The household always consumes less than its its ideal point, S. A high energy price (high p) or low value of matching the ideal consumption (low γ), pushes the household to reduce its energy consumption further away from S. Conversely, if energy were very cheap (low p) or if matching S was highly important (high γ), the household would consume very close to S.

We now consider the use of individual contracts or a contest as an incentive for energy reduction.

Consider a set of N households with the same preferences, i.e., the same parameters S, γ, and σ. These households make simultaneous energy-consumption choices. Let $\hat{\mathbf{e}} = (\hat{e}_1, ..., \hat{e}_N)$ be the realized consumption profile, where $\hat{e}_i = e_i + \varepsilon_i$ is the realized consumption of household i. We assume that the shocks ε_i are independent and identically distributed, $\varepsilon_i \sim N(0, \sigma^2)$.

Under an incentive program that rewards energy reduction, household i receives a reward of $I_i(\hat{\mathbf{e}})$, which can depend on the realized consumption of all households. Taking the energy-consumption choices by other household as given, household i choose its energy consumption to maximize

$$U_i(e_i, e_{-i}) = E_\varepsilon[I_i(\hat{e}_i, \hat{\mathbf{e}}_{-i})] - \gamma(e_i - e_0^*)^2. \tag{4}$$

Individual Contract. Consider first an individual contract. In Appendix B, we show that a *threshold contract* is an optimal individual contract to allocate a fixed reward, B. That is, the household receives a prize B if and only if its realized consumption is below the threshold ℓ.[14] Under an individual contract with threshold ℓ, household i's chooses its

[14] It can be shown that not every optimal contract is necessarily a single threshold. That is, other type of contracts (e.g., multiple thresholds) can also be optimal under some conditions.

energy consumption to solve

$$\max_{e_i \geq 0} \; B \cdot F(\ell - e_i) - \gamma(e_i - e_0^*)^2. \tag{5}$$

Our focus is on interior solutions of this problem. Households may ignore contracts that are too demanding, i.e., those that require to very little consumption to receive a relatively small reward. Alternatively, if the reward is too large, households would "shut down" and consumer zero. We ignore this corner solution as the monetary incentives we consider are relatively small.

Proposition 2 (Contracts)**.** *Consider a contract that pays B to the household if its realized consumption is below ℓ. An interior solution for (5) is characterized by the fixed point*

$$e^*_{contract} = e^*_{control} - \frac{Bf(\ell - e^*_{contract})}{2\gamma}, \tag{6}$$

where $f(\cdot)$ is the density of $N(0, \sigma^2)$.

Fixing the individual reward B, the sponsor of an energy conservation program can choose a threshold ℓ that minimizes the household's expected consumption. The optimal threshold, denoted ℓ^*, is characterized by the solution to

$$\min_{\ell \geq 0} e^*_{\text{contract}}(\ell).$$

Proposition 3. *If the density of the idiosyncratic shock is satisfies $f(\varepsilon) = f(-\varepsilon)$, $f'(\varepsilon) = 0$ if and only if $\varepsilon = 0$, and consumption is interior at the optimal threshold, then $\ell^* = e^*_{contract}(\ell^*)$.*

Proposition 3 establishes that, as long as the solution is interior, the household reduces its consumption up to the point of just achieving the reward. This occurs because reducing energy further from the ideal point is costly. Using this proposition we can also get a closed-form solution for the household consumption for an optimal contract. Using the fact that $e^*_{\text{contract}} = \ell^*$ in (6) we obtain

$$e^*_{\text{contract}} = e^*_{\text{control}} - \frac{Bf(0)}{2\gamma}. \tag{7}$$

The energy reduction induced by the optimal contract is $\frac{Bf(0)}{2\gamma}$, which depends on the reward, B, the sensitivity to matching the ideal consumption, γ, and the density of receiving an idiosyncratic shock of zero, $f(0)$. Also note that as B increases, the optimal threshold ℓ^*

(which equals e^*_{contract}) decreases. That is, when the reward is larger, the contract becomes more demanding in terms of energy reduction.

Importantly, the optimal threshold ℓ^* depends on the preference parameters S and γ. If a household's ideal consumption level S varies due to common shocks—such as seasonal changes throughout the year—implementing an optimal contract becomes significantly burdensome. This is because it requires tailoring the contract to each household's expected ideal consumption point, which may fluctuate over time.

Individual Contest. We now consider the use of a contest to promote energy reduction. Suppose the sponsor of an energy-reduction program organizes a contest where N households of similar characteristics simultaneously make energy consumption choices, e_i. The household with the lowest realized consumption, $\hat{e}_i$, receives a prize of V.[15] Suppose that every household other than i chooses consumption e^*. Then, household i solves the problem

$$\max_{e_i \geq 0} \; V \cdot \int (1 - F(e_i + \varepsilon_i - e^*))^{N-1} dF(\varepsilon_i) - \gamma(e_i - e_0^*)^2. \tag{8}$$

In this expression, given e_i and ε_i, household i wins the contest by consuming the least amount among N households, which occurs with probability $(1 - F(e_i + \varepsilon_i - e^*))^{N-1}$. Given that e_i is chosen before the realization of ε_i, the household computes the expectation of this probability with respect to ε_i.

Proposition 4 (Contests)**.** *Consider a contest between N households. In a symmetric equilibrium with interior consumption, each household chooses an energy consumption of*

$$e^*_{\textit{contest}} = e^*_{\textit{control}} - \frac{I(V, N; F)}{2\gamma}, \tag{9}$$

where $I(V, N; F) = V \int (N-1)(1 - F(\varepsilon_i))^{N-2} f^2(\varepsilon_i) d\varepsilon_i$.

For any fixed prize V, as the contest grows large, the competition induced by the contest dampens a household's incentive to save energy.[16] Thus, larger contests require larger prizes to counteract the increased competitive pressure that demotivates individual household to save energy. It is important to note that the incentive induced by the contest is independent on the preference parameters S and γ, so the induced reduction is unaffected by common shocks or seasonal effects.

[15] When households have equal baseline consumption levels, giving the prize to the household with the lowest consumption or greatest energy savings is equivalent.

[16] That is, $\frac{\partial I(V,N;F)}{\partial N} < 0$.

4.2 Individual Contracts versus Contests

How does a household's energy consumption under a contract compare to its energy consumption under a contest? Depending on the model's parameters, a contest can dominate individual contracts or vice-versa. This can be readily seen by comparing the optimal consumption in equations (7) and (9).

Let us first compare the energy-saving incentives of an individual household participating in an optimal contract offering a reward of B or in an N-household contest offering a prize of V. The household saves more energy under an individual contract when

$$Bf(0) \geq V \int (N-1)(1-F(\varepsilon_i))^{N-2} f^2(\varepsilon_i) d\varepsilon_i. \tag{10}$$

In terms of total energy savings, the contest provides energy-conservation incentives to N households, whereas the individual contract targets a single one. Therefore, an individual contract (provided to a single household) saves more energy than a N-household contest when

$$Bf(0) \geq NV \int (N-1)(1-F(\varepsilon_i))^{N-2} f^2(\varepsilon_i) d\varepsilon_i. \tag{11}$$

In terms of the cost of each energy-conservation program, the expected cost of an individual contract (offered to a single household) is $BF(0)$, whereas the (certain) cost of the contest is V. Imposing that both programs cost the same, $BF(0) = V$, incentivizing a single household with an individual contract dominates an N-household contest when

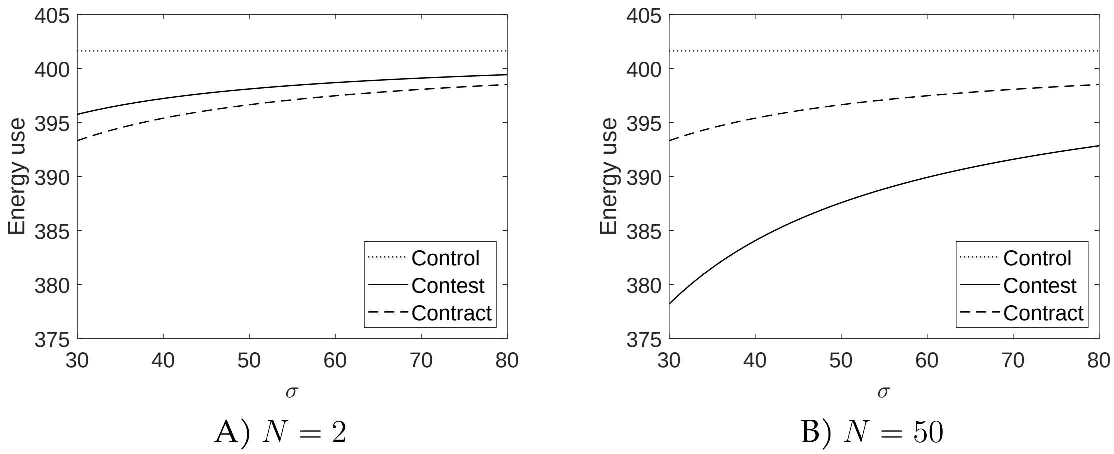
$$2f(0) \geq N \int (N-1)(1-F(\varepsilon_i))^{N-2} f^2(\varepsilon_i) d\varepsilon_i, \tag{12}$$

where we use that $F(0) = 0.5$. This condition can only hold when $N = 2$.[17] Therefore, the N-household contest dominates giving an optimal contract to a single household when $N \geq 3$ and vice-versa when $N = 2$. Moreover, an energy-conservation program that offers an optimal contract to $k \geq 1$ households, generates an expected total energy savings of $kBf(0)$ at cost $kBF(0)$. If we again impose that both programs cost the same (in expectation), the rewards in these programs must satisfy $kBF(0) = V$. Comparing the energy savings induced by both programs yields again inequality (12). Therefore, a contest dominates individual contracts when $N > 2$.

As mentioned, increasing the number of players in a contest decreases a player's incentive to exert effort, for a fixed prize. However, imposing that both incentives programs cost the

[17] When $f(0) \geq f(x)$ for all $x \neq 0$, we have $\int (N-1)(1-F(\varepsilon_i))^{N-2} f^2(\varepsilon_i) d\varepsilon_i \geq f(0)$.

Figure 3: Comparing contests and optimal contracts



Notes: The figures fix $B = 2$, $\gamma = 0.0016$, $p = 0.11$, and $S = 436$, and they show the optimal energy use under an optimal contract and a contest for different values of σ and N. The expected payment per household is equivalent in all comparisons between contests and contracts. These parameters approximate our average empirical estimates.

same (in expectation), scales the prize of the contest with the number of players. A greater prize encourages players in a contest to exert more effort, all else equal. In our setting, the encouragement effect of a greater prize more than compensates for the discouragement effect of greater competition as the number of players grows, delivering the result.

Figure 3 compares the energy consumption of all the households participating in a N-household contest with the consumption of a single household under an individual (optimal) contract. In every comparison, the expected payout per household is equal across the contest and contract. The figure also shows the consumption of a household facing no energy-conservation incentives ("control"). Following the discussion above, the figure shows that an optimal contract can dominate or be dominated by a contest depending on the size of the contest. In the figure, when $N = 2$, the contract dominates, and when $N = 50$, the contest does.

4.3 Estimation

In the empirical analysis, we classify each households as one of $K = 56$ types, each type denoted by $\kappa = 1, ..., K$. We defined types based on energy consumption between July 15 and August 13, 2022—i.e., one year before the beginning of our experiment. Figure D.1 in the Online Appendix plots the consumption of each type.

Let N_κ be the number of households of type κ. On average, there are 180.73 households associated to each type, with some types having as few as 145 households and other as many as 250.[18] In our experiment, households of each type where randomly assigned to four treatment conditions: control, contract I, contract II, and contest. $N_{\kappa,t}$ is the number of households of type κ assigned to treatment t.

We assume that each household makes monthly energy consumption choices according to our model in Section 4.1. A household's type determines its preferences over energy consumption through the parameters γ_κ, S_κ, and the distribution of the shocks $N(0, \sigma_\kappa^2)$. We assume the shocks are independent. For estimating the parameters, $\Theta_\kappa = (\gamma_\kappa, S_\kappa, \sigma_\kappa)$ for $\kappa = 1, ..., K$, we leverage the model's predictions and the variation in consumption induced by each treatment.

Our estimation procedure consists of the following steps:

1. **Estimation of σ_κ:** To estimate σ_κ, we compare the consumption of a household of type κ assigned to treatment t predicted by the model with its observed consumption. That is, the observed energy consumption of household i assigned to treatment t according to our model is given by

   $$e_{i,\kappa,t} = e^*_{t,\kappa}(\Theta_\kappa) + \varepsilon_{i,\kappa,t},$$

   where $e^*_{t,\kappa}(\Theta_\kappa)$ denotes the optimal consumption choice of household κ predicted by our model, given the parameters. Given that idiosyncratic shocks have mean zero, taking expectation we obtain $E[e_{i,\kappa,t}] = e^*_{t,\kappa}(\Theta_\kappa)$. Therefore,

   $$\varepsilon_{i,\kappa,t} = e_{i,\kappa,t} - E[e_{i,\kappa,t}].$$

   By the law of large numbers, we can estimate $E[e_{i,\kappa,t}]$ by computing the average consumption of households of type κ in treatment t, i.e.,

   $$E[e_{i,\kappa,t}] \approx \bar{e}_{i,\kappa,t} \equiv \frac{1}{N_{\kappa,t}} \sum_{j=1}^{N_{\kappa,t}} e_{j,\kappa,t}.$$

   Thus, we can recover σ_κ by computing the standard deviation of $e_{i,\kappa,t} - \bar{e}_{i,\kappa,t}$ over i

[18] The median number of households in each type is 180, and a standard deviation of 21.8.

and t. Thus, our estimator for σ_κ is

$$\hat{\sigma}_\kappa = \text{St.Dev.}[e_{i,\kappa,t} - \bar{e}_{i,\kappa,t}].$$

2. **Estimation of γ_κ:** To estimate γ_κ, we rely on an estimate of σ_k, as well as the price of energy in our experiment, $p = 0.11$ USD/kWh, the prize in each contest, \$87, and the fact that our experiment assigned 50 households to each contest. We compare the energy consumption of households of type κ across the contest treatment using that from equations (3) and (9) we have

$$e^*_{\kappa,\text{control}} - e^*_{\kappa,\text{contest}} = \frac{I(N,V;F_\kappa)}{2\gamma_\kappa},$$

where $I(N,V;F_\kappa) = V\int(N-1)(1-F_\kappa(\varepsilon_i))^{N-2} f^2_\kappa(\varepsilon_i)d\varepsilon_i$ can be computed numerically given $F_\kappa = N(0,\sigma_k)$, $N = 50$, and $V = \$87$. We can again estimate $e^*_{\kappa,\text{control}}$ and $e^*_{\kappa,\text{contest}}$ by the average observed consumption for households of type κ assigned to the control and contest groups, $\bar{e}_{\kappa,\text{control}}$ and $\bar{e}_{\kappa,\text{contest}}$, respectively. Our estimator of γ_κ minimizes the difference between the model prediction for $e^*_{\kappa,\text{control}} - e^*_{\kappa,\text{contest}}$ and its empirical analog. When γ_k varies by type, our estimator is given by

$$\hat{\gamma}_\kappa = \frac{I(N,V;F_\kappa)}{2(\bar{e}_{\kappa,\text{control}} - \bar{e}_{\kappa,\text{contest}})}.$$

3. **Estimation of S_κ:** Given an estimate of γ_κ, S_κ can be estimated by $S_\kappa = e^*_{\kappa,\text{control}} + \frac{p}{2\gamma_\kappa}$, using equation (3), or $S_\kappa = e^*_{\kappa,\text{contest}} + \frac{p+I(V,N;F_\kappa)}{2\gamma_\kappa}$, using equation (9). Since S_κ is over-identified, we consider an estimator that puts equal weights on these two equations,

$$\hat{S}_\kappa = \frac{1}{2}\left(\bar{e}_{\kappa,\text{control}} + \frac{p}{2\gamma_\kappa}\right) + \frac{1}{2}\left(\bar{e}_{\kappa,\text{contest}} + \frac{p+I(V,N;F_\kappa)}{2\gamma_\kappa}\right).$$

4. **Practical Considerations.** In practice, to gain power in estimating the parameters γ_κ and σ_κ, we group types into four groups: $(\gamma_\kappa,\sigma_\kappa) = (\gamma_1,\sigma_1)$ for $\kappa = 1,...,14$; $(\gamma_\kappa,\sigma_\kappa) = (\gamma_{15},\sigma_{15})$ for $\kappa = 15,...,28$; $(\gamma_\kappa,\sigma_\kappa) = (\gamma_{29},\sigma_{29})$ for $\kappa = 29,...,42$; $(\gamma_\kappa,\sigma_\kappa) = (\gamma_{43},\sigma_{43})$ for $\kappa = 43,...,56$. This grouping requires an estimation of 8 different parameters. We estimate $(S_\kappa)^{56}_{\kappa=1}$ separately for each type. Hence, we estimate a total of $8 + 56 = 64$ parameters.

4.4 Estimation Results

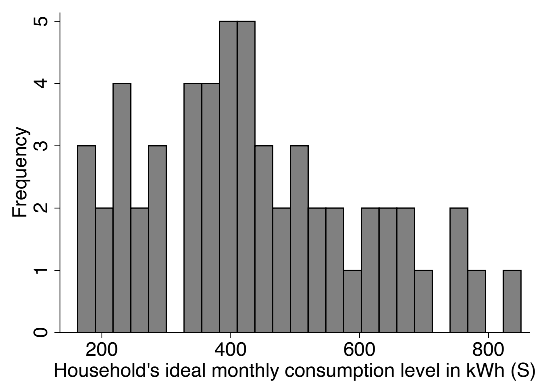
Model Estimates. Using our experimental data, we estimate $(S_\kappa, \gamma_\kappa, \sigma_\kappa)$ for $\kappa = 1, ..., 56$. As mentioned, we constrain the parameters γ_κ and σ_κ to be the same for groups of 14 types to gain statistical power. Table 6 reports our estimates for γ_κ and σ_κ, while Figure 4 reports our estimates for S_κ.

Table 6: Estimates of the model parameters: γ and σ

Type	γ	St. Error	σ	St. Error
Type 1-14	0.0024	5.9615e-05	66.343	0.1217
Type 15-28	0.0013	1.7218e-05	65.074	0.0949
Type 29-42	0.0014	3.2103e-05	82.479	0.0995
Type 42-56	0.0009	2.8263e-05	124.41	0.1435

Notes: Bootstrapped standard errors.

Figure 4: Estimates of the model parameters: S



Notes: The histogram shows the estimates of S_κ for all 56 types.

4.5 Counterfactual Analysis

We use our structural model's estimates to uncover the short-run price elasticity of demand, as well as to evaluate different programs. It is important to mention that our model in section 4.1 features a *quadratic* loss when energy consumption deviates from a household's ideal point. This assumption can be reasonable for small deviations relative to the ideal point but it might be questionable for very large deviations. For this reason, the exercises in this section focus on policies that moderately change consumption, in which case our model's predictions are robust.

Price Elasticity of Demand. Using our structural model's estimates, we compute the price elasticity of expected demand for energy. We then use this elasticity to simulate the impact of a policy solution that includes a price increase. Without an energy-saving incentive, the expected consumption of household of type κ is given by equation (3). Thus, the expected energy demand is the weighted sum of energy consumption across households

$$D(p) = \sum_{\kappa=1}^{K} \alpha_\kappa \left(S_\kappa - \frac{p}{2\gamma_\kappa} \right),$$

where α_κ is the fraction of household of type κ, which we compute from our experimental data according to

$$\alpha_\kappa = \frac{\text{Number of Households of type } \kappa}{\text{Total Number of Households}}.$$

Assuming a quadratic loss from deviations relative to the ideal consumption imposes a linear demand function:

$$D(p) = \lambda - \beta p,$$

where

$$\lambda = \sum_{\kappa=1}^{K} \alpha_\kappa S_\kappa \quad \text{and} \quad \beta = \sum_{\kappa=1}^{K} \frac{\alpha_\kappa}{2\gamma_\kappa}.$$

Then, the price-elasticity of energy consumption is

$$\frac{dD(p)}{dp} \frac{p}{D(p)} = \frac{-\beta p}{\lambda - \beta p}.$$

Using our estimates for α_κ, S_κ, and γ_κ, and the price of electricity in Vietnam of $p = 0.11$ dollars per kWh, we get $\lambda = 434.6$ and $\beta = 380.8$, so we estimate a price-elasticity of energy consumption at current prices of -0.1067. Figure 5 plots the estimated average demand for energy (monthly consumption). It shows that at the current price of 0.11 dollars per kWh, the average monthly consumption is 392 kWh. If energy was free, households would consume 434 kWh on average. A 10 percent increase in the price of a kWh reduces energy consumption by about 1 percent.

The figure also shows the average consumption level under a contest like the one offered in our experiment (i.e., 50 households per contest with a prize of $87), which is given by 375 kWh. Instead of providing incentives to save energy, the consumption reduction in the contest can be replicated via a price increase from $0.11 per kWh to $0.157 per kWh, i.e., a 43 percent price increase.

Figure 5: Estimated Average Demand Curve

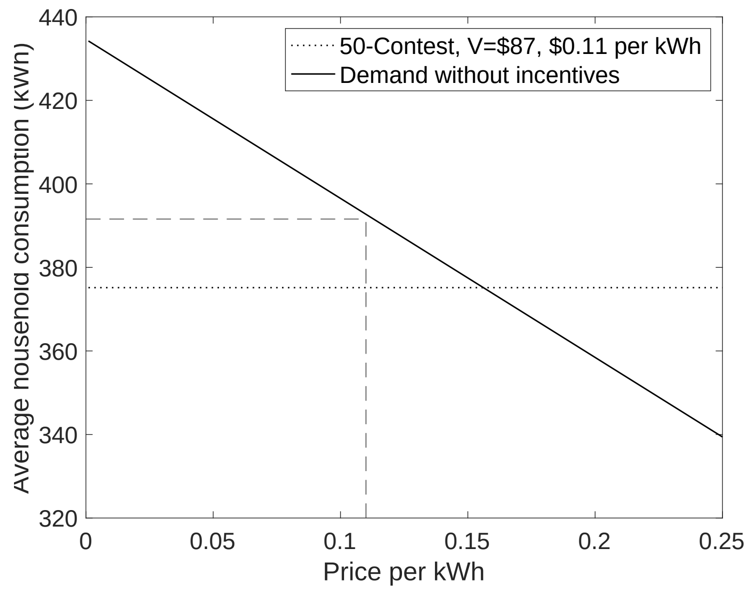


Table 7: Energy Savings for Expected Payout in Our Experiment

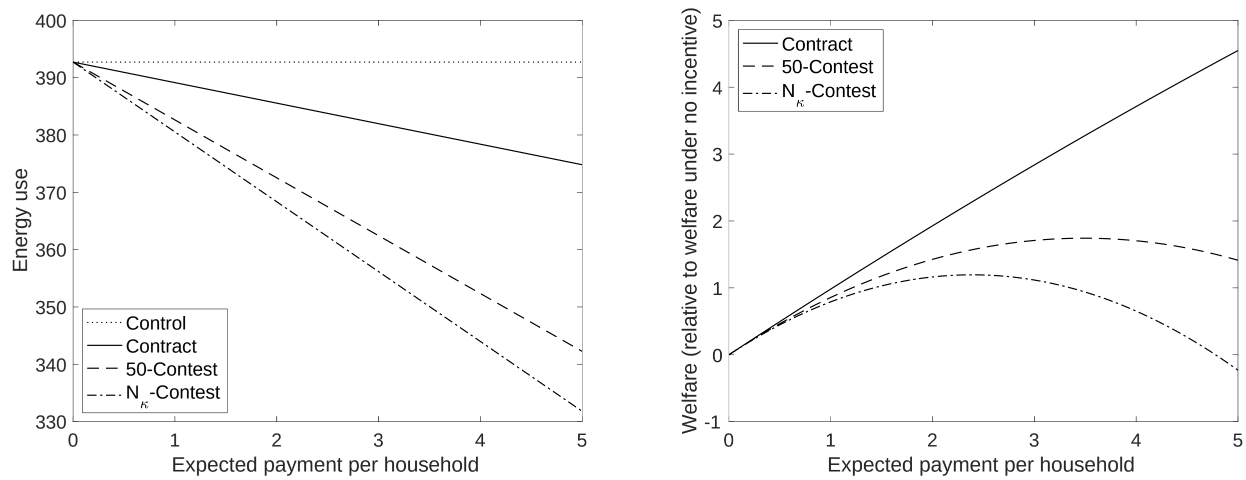
E[Payout]	Control	ℓ^*-Contract	50-Contest	N_κ-Contest
3.1400	392.7142	381.4724	361.0260	354.4635
3.2100	392.7142	381.2218	360.3196	353.6108
1.7400	392.7142	386.4847	375.1545	371.5180

Counterfactual Contracts and Contests. We use our empirical model to simulate the average household consumption under different energy-saving incentives, keeping the expected payment per household fixed across incentive programs.

Figure 6 (Panel A) shows the average monthly energy consumption under different programs when each household receives an expected payout ranging from 0 to 5 dollars. The figure shows energy consumption under an optimal contract and contests of two different sizes: 50 households and N_κ households (the number of households of type κ in our sample). As a benchmark, the figure also shows the average consumption when no incentive is provided (i.e., a control group). The figure shows that contests dominate the optimal contract and that larger contests induce larger gains.

Table 7 provides similar information but restricting attention to the the actual expected payouts in our experiment (see Table 5). For instance, using the expected payout of 3.14 dollars per household, which is the average payment in the experiment to households enrolled in contract 1, an optimal contract achieves an expected monthly consumption of

Figure 6: Comparing contests and optimal contracts using our model estimates



A) Electricity use B) Welfare

Notes: The figures plot the average energy consumption (Panel A) and average welfare (Panel B) of a household when faced with various incentive schemes using our model estimates. We measure welfare using the utility function given by equation (4).

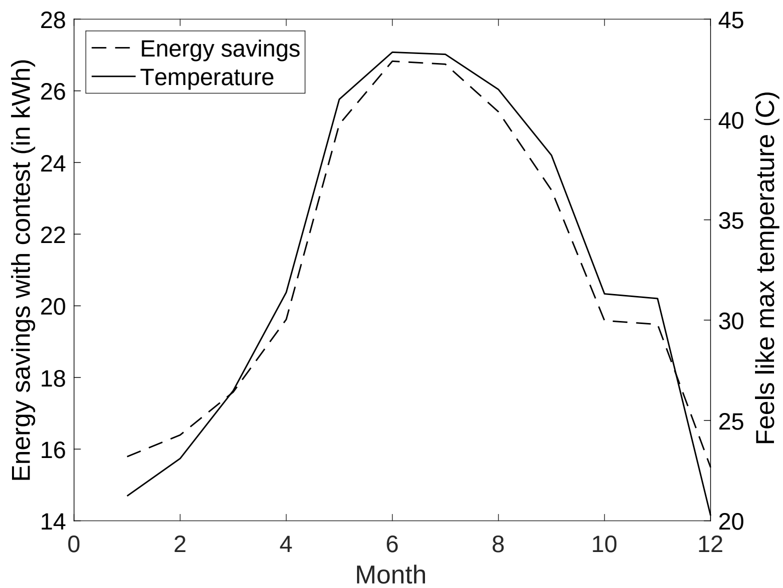
381.47. This represents a 2.56 percent reduction relative to the control group. Instead, a 50-household contest achieves an 8 percent reduction. Across payout levels, the contests dominate the optimal contract and the energy savings increase in the size of the expected payout.

Figure 6 (Panel B) compares the average welfare of a household when offered an incentive to save electricity, for a given expected payment, relative to their welfare when offered no such incentive (i.e., no payment to save electricity). We measure welfare using the utility function given by equation (4). As before, we impose equality in the expected payout of a contract and a contest, i.e., $BF(0) = V/N$. The figure shows that for every expected payment value, the household is better off when incentivized with a contract. This is because the contract induces less electricity savings (which are a source of disutility to the household) relative to the contest treatments, for a given expected payment (see Figure 6, Panel A).[19] This is in contrast to the optimization problem of the electric utility, which would prefer to incentivize households using a contest because it induces more electricity savings per dollar spent than the optimal contract.

Weather Variation. The cost-effectiveness of a energy-saving program can vary over the

[19]The expected utility for a household under an optimal contract is $U_{contract} = BF(0) - \gamma(e^*_{contract} - e^*_0)^2$, and for one competing in a N-household contest is $U_{contest} = V/N - \gamma(e^*_{contest} - e^*_0)^2$. Equating expected payouts, $BF(0) = V/N$, implies that $U_{contract} \geq U_{contest}$ if and only if $e^*_{contract} \geq e^*_{contest}$.

Figure 7: Energy reductions of a contest across months



Notes: The figure plots the energy reduction of a household (in kWh) when households face the contest treatment in our experiment (i.e., 50 participants, a prize of $V = \$87$, and a price per kWh of \$0.11) in different months of the year. The figure also plots the average maximum "feels like" temperature for each month. Months are enumerated from 1 (January) to 12 (December).

year due to differences in weather. We simulate the energy savings caused by a contest like the one offered in our experiment (i.e., 50 households per contest with a prize of \$87) across all months one year before the experiment. To compute the energy savings, we estimate values of $S_{t,\kappa}$ and $\sigma_{t,\kappa}$ using household-level consumption data for period t following the same estimation procedure discussed above. We assume that the values of γ_κ remain constant throughout.[20]

Figure 7 shows the average energy savings of a household (in kWh) by month, where months are enumerated from 1 (January) to 12 (December). The figure also plots the average maximum "feels like" temperature for each month. The figure shows that energy savings are greatest in the summer months, when temperatures are higher, making contests most cost-effective in summer months. Incidentally, this is aligned with the electric utility's goals with demand management as the grid is most strained in the summer months.

[20]We project the estimated values of $S_{t,\kappa}$ and $\sigma_{t,\kappa}$ on average monthly temperature, average monthly temperature (squared), and type fixed effects, and use the fitted values of $S_{t,\kappa}$ and $\sigma_{t,\kappa}$ for the analysis.

5 Marginal Abatement Cost of the Energy Conservation Program

During four summer months from early May to August 2023, Vietnam faced significant challenges with its primary energy sources, hydropower and thermal power. The intense heat and prolonged drought led to a depletion of water levels in lakes and the incapacitation of numerous generating units. The electricity utilities must mobilize many power plants to meet the demand for electricity and resort to oil-fired sources, despite their significantly higher costs compared to other options. Oil power plants are also amongst the more environmentally polluting source of electricity production. Therefore, energy conservation not only enhances power supply reliability and reduces the necessity for deploying costly electricity sources but also relieves pressure on the country's investment capacity and helps mitigate emissions from fossil-fuel electricity generation.

What is the marginal abatement cost implied by the program? Consider the two last power plants to be turned on on a hot summer day in Hanoi. The last plant burns oil and has a marginal cost per kWh of \$0.2609 and a carbon intensity of 0.00104 tons of CO_2 per kWh, whereas the second to last one burns coal and has a marginal cost per kWh of \$0.0913 and a carbon intensity of 0.001 tons of CO_2 per kWh. The average price per kWh collected by the utility is \$0.11.

Consider the contest incentive. Using our estimates from Table 3 (Column 1), we know that households assigned to a contest on average decrease their consumption during the incentive period by 22.68 kWh. If the oil plant is in operation, the contest incentive will cause a decrease in emissions of 22.68 kWh $\times\, 0.00104$ tons of CO_2 per kWH $= 0.024$ tons of CO_2 per household. The direct cost of the incentive program is the payout of \$1.74 per household. Using these values, the marginal abatement cost of reducing 1 ton of CO_2 is then given by MAC = 1.74/0.024 = \$73.76. When using instead the estimates from Table 3 (Column 2)—which imply an average consumption reduction of 28.14 kWh per household—the MAC is given by \$59.45, as summarized in Table 8. This is well-below widely used estimates of the social cost of carbon—the U.S. Environmental Protection Agency uses a social cost of carbon of \$190/Mt CO_2.

From the perspective of the utility, however, an indirect cost (or benefit) of the program is the avoided profit (or loss) on the kWhs that households no longer consume as a consequence of the incentive program. When the oil plant is in operation, there is an avoided profit loss of $(0.11 - 0.2609) \times 22.68 = -\$3.42$, where the latter comes from the fact that the

Table 8: Marginal Abatement Cost Estimates

	(1)	(2)	(3)	(4)
Marginal Plant	Oil		Coal	
Consumption reduction (kWh)	22.68	28.14	22.68	28.14
CO_2 abated (in tons)	0.024	0.029	0.023	0.028
Payment	1.74	1.74	1.74	1.74
Profit loss (in USD)	-3.422	-4.246	0.424	0.526
MAC (in USD)	73.769	59.455	76.720	61.834
MAC, including profit/loss (in USD)	-71.327	-85.641	95.420	80.534

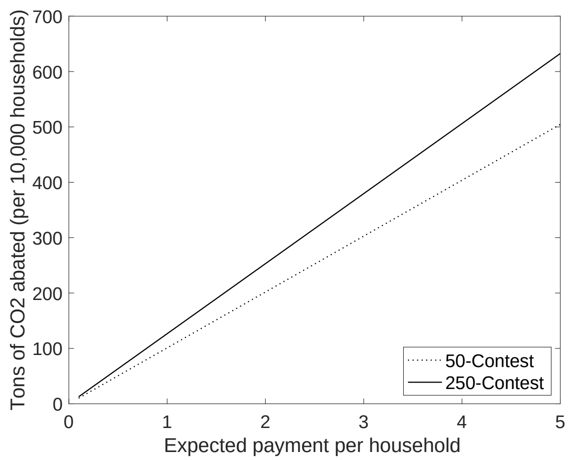
Notes: The consumption reduction values are based on the estimates in columns 1 and 2 of Table 8. Consumption reduction, profit loss, payment, and CO_2 abated are measured at the household level. MAC is computed using the formula Payment/CO_2 abated (in tons). MAC, including profit loss (in USD) is computed using the formula (Payment + profit loss)/CO_2 abated (in tons). See the discussion in the text for more details.

marginal cost of generation of the oil plant is higher than the price per kWh (i.e., the utility saves money by not supplying these kWhs). When considering both the direct (payment per household) and indirect (profit loss) costs of the program, the marginal abatement cost of reducing 1 ton of CO_2 is then given by MAC $= (1.74 - 3.42)/0.024 = -\$71.33$, implying that reducing emissions saves the utility money. When using instead the estimates from Table 3 (Column 2), the MAC is given by -85.64, as summarized in Table 8. These MAC estimates likely represent an upper bound, as they do not account for the value of power reliability to customers, the safety of electrical grid facilities and equipment, and the alleviation of pressure on the country's investment in capacity.

Consider instead the case in which the coal plant is the marginal plant (see Table 8, columns 3 and 4). The contest incentive will cause a decrease in emissions of 0.023 tons of CO_2 per household when using the estimates from Table 3 (Column 1). As before, the cost per household has two components: the expected payout of \$1.74 per household and the avoided profit gain of $(0.11 - 0.0913) \times 22.68 = \$0.42$, since the utility makes money on the kWhs conserved. When ignoring the indirect cost (profit loss) to the utility, the marginal abatement cost of reducing 1 ton of CO_2 is given by MAC $= 1.74/0.023 = \$76.72$ (or \$61.83 when using the estimates from Table 3, column 2). When instead considering both the direct and indirect costs of program, the marginal abatement cost of reducing 1 ton of CO_2 is then given by MAC $= (1.74 + 0.42)/0.023 = \$95.42$ (or \$80.53 when using the estimates from Table 3, column 2). These MAC estimates, again, are likely an upper bound.

The estimate of the MAC when the coal plant is the marginal plant in operation is higher than some estimates in the prior literature(Berkouwer and Dean, 2022; Jayachandran et al.,

Figure 8: Emissions reductions under alternative contest designs



Notes: The figure plots the tons of CO_2 abated per month (per 10,000) for different contests using our model estimates. We assume that the marginal plant is the coal plant with a carbon intensity of 0.001 tons of CO_2 per kWh and a marginal cost of generation of $0.0913 per kWh.

2017) but lower than many others (Ito, 2014; Davis et al., 2014). Note that this estimate of MAC is still greater than zero, meaning that the energy conservation program is costly for the utility. If we ignore the foregone profit from reducing electricity demand (this is often in the interest of the utility since the grid is constrained), the MAC ranges from $61.83-$76.72/Mt CO_2 depending on the choice of specification. Although carbon pricing could make the program viable from the utility's private perspective, Vietnam has no carbon tax or offset market. Carbon offset revenue paying at least $80.5 per ton of CO_2 could make the program profitable for the utility.

Could a different contest reduce the marginal abatement cost? We examine whether an optimized incentive program can bring the MAC down by inducing energy savings in a more cost-effective way. We use our model estimates to compute the MAC and emissions reductions for different contest designs, assuming the coal plant is the marginal plant (carbon intensity of 0.001 tons of CO_2 per kWh and a marginal cost of generation of $0.0913 per kWh). We find that using a contest with 250 households (as opposed to 50 households in our experiment) can significantly drop the MAC for every level of payment per household. For example, the MAC in a contest with 250 households with a payment per household of $1.74 (same as in our experiment) can decrease the MAC by $20 relative to the MAC when using a contest with 50 households. Figure 8 plots the emissions reductions for different contests and expected payment amounts, and it shows that the emissions abated per month are meaningful, suggesting that demand-side incentive programs can be a cost-effective tool to reduce emissions.

Our estimates suggest that the energy conservation program should save the utility money when all plants are in operation. Even when the marginal plant is a more efficient plant, the marginal abatement cost is less than the many estimates of the social cost of carbon, implying that the program could plausibly raise carbon offset revenue that would make it viable.

6 Conclusion

In this paper, we experimentally evaluate the cost-effectiveness of contracts and contests as instruments for incentivizing energy conservation in Hanoi, Vietnam. We find that contests and contracts achieve similar energy reductions, but contests are nearly twice as cost-effective. We build a model of household energy consumption and using our experimental variation we simulate energy conservation under optimal contracts and contests. We find that when there is no endogenous entry into contests (that is the number of participants is fixed) and when effort is only loosely correlated with observed measures of performance, optimal contests dominate optimal contracts. We use information on carbon intensity of energy sources to compute marginal abatement cost between \$59.45-\$76.72/Mt CO_2 without accounting for any other benefits from demand management. When oil is the marginal source of electricity, utility savings from differences in generation costs from oil and retail prices alone justify demand management. When coal is the marginal source, accounting for avoided profits from demand reduction implies a marginal abatement cost of \$80.50-\$95.42/Mt CO_2, well below the EPA's social cost of carbon of \$190/Mt CO_2.

Our findings have important implications for the design and implementation of demand side management programs. First, we show that working alongside utility partners and tweaking existing programs can deliver potentially large savings. Our finding is particularly relevant for low- and middle-income countries, where maximizing the impact of scarce dollars spent on energy conservation is crucial. By developing a framework to compare contests and contracts, we offer evidence that contests are an effective strategy for managing demand and reducing emissions, particularly in areas dependent on fossil fuels for electricity. Second, our model relies on minimal data on electricity consumption that is increasingly available to utilities around the world that are deploying smart meters. Using our experimental variation, we are able to provide counterfactual simulations that allow us to comment on the design of optimal contracts and contests. Finally, our contest design complements existing "nudge" approaches such as peer comparisons. These have been shown to be extremely cost-effective given their low implementation costs. However, such nudges alone cannot deliver large scale demand reductions during peak months which is much needed by utilities such as the one we work with. Indeed, our results should be interpreted as over and above any demand reduction from nudge interventions.

Implementing contests at scale for energy conservation requires understanding two important parameters. First, what is the "voltage drop" from scaling this program beyond those who signed up for some program in the first place (List, 2022). The take-up and demand response could be lower, driving down the cost-effectiveness of these programs. However, at the same time, number of participants in contests could be expanded to increase cost-effectiveness. Second, it remains an open question as to whether such demand reductions can be derived over and over again, especially if there are discouragement effects from not meeting contract thresholds or not winning the contests. In principle, utilities would like to be able to rely on such programs each year during peak months. We leave tackling these important questions to future research.

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Appendix A: Proofs

Proof of Proposition 1

Proof. Without an incentive to reduce energy, each household solves:

$$\max_{e \geq 0} -(e-S)^2 - pe + \sigma^2.$$

The solution to this optimization problem is given in the proposition. □

Proof of Proposition 3

Proof. The optimal consumption in an interior solution is characterized by the first-order condition

$$-Bf(e^* - \ell) = 2\gamma(e^* - e_0^*),$$

where $e_0^* = S - \frac{p}{2\gamma}$. Using the implicit function theorem and taking derivative with respect to ℓ we obtain

$$-Bf'(e^* - \ell)\left(\frac{\partial e^*}{\partial \ell} - 1\right) = 2\gamma\frac{\partial e^*}{\partial \ell}.$$

Solving for $\frac{\partial e^*}{\partial \ell}$ we obtain:

$$\frac{\partial e^*}{\partial \ell} = \frac{Bf'(\ell - e^*)}{2\gamma + Bf'(\ell - e^*)}.$$

Then, using that at the optimum for an interior solution, $\frac{\partial e^*}{\partial \ell} = 0$, it must be that $f'(\ell^* - e^*) = 0$ and since $f'(\varepsilon) = 0$ if and only if $\varepsilon = 0$, we conclude that $\ell^* = e^*$. □

Proof of Proposition 4

Suppose there are N households competing in a static contest. Households are ranked according to their reduction (measured relative to consumption one year ago, e_i^{past}), from the largest reduction to the lowest one. The energy reduction for household i is given by $\hat{e}_i - e_i^{past}$. With a single prize, V, household i wins the contest if

$$\hat{e}_i - e_i^{past} < \hat{e}_j - e_j^{past} \text{ for all } j \neq i.$$

This expression is the same as

$$e_i + \varepsilon_i - e_i^{past} < e_j + \varepsilon_j - e_j^{past} \Leftrightarrow e_i - e_j + e_j^{past} - e_i^{past} + \varepsilon_i < \varepsilon_j.$$

In our experiment, households were grouped according to their past consumption, so in each contest $e_i^{past} = e^{past}$ for all i. Therefore, household i wins the contest if, for all $j \neq i$,

$$\varepsilon_j > e_i + \varepsilon_i - e_j.$$

In a symmetric equilibrium, each household optimally chooses $e_i = e^*$. Fixing ε_i and given e^*, player i wins with probability

$$\psi(e_i, \varepsilon_i, e^*) \equiv (1 - F(e_i + \varepsilon_i - e^*))^{N-1}.$$

Household i chooses her effort before knowing the realization of the shock ε_i. Then, the optimal choice of e_i solves

$$\max_{e_i \geq 0} V \int \psi(e_i, \varepsilon_i, e^*) f(\varepsilon_i) d\varepsilon_i + E_{\varepsilon_i}[U(e_i, \varepsilon_i)].$$

The FOC yields

$$-V \int \frac{\partial \psi(e_i, \varepsilon_i, e^*)}{\partial e_i} f(\varepsilon_i) d\varepsilon_i + E_{\varepsilon_i}[U'(e_i, \varepsilon_i)] = 0.$$

In a symmetric equilibrium we must have $e_i = e^*$. Thus, the contests create an incentive to reduce energy consumption, I, given by

$$I = V \int (N-1)(1 - F(\varepsilon_i))^{N-2} f^2(\varepsilon_i) d\varepsilon_i.$$

The optimal energy consumption solves

$$e^* = S - \frac{(c + I)}{2\gamma}. \tag{13}$$

Intuitively, spending energy becomes "more costly" when there is a prize for being the household with the lowest consumption.

Appendix B: Threshold Contracts are Optimal

Consider a principal with the objective of *minimizing* the household energy consumption subject to a budget constraint. Equivalently, the principal minimizes the household's expected consumption since $E[\varepsilon] = 0$.

For household i, the principal considers an individual contract that rewards a household based on its *realized consumption* regardless of the consumption by other households, i.e., $I_i(\hat{\mathbf{e}}) = W(\hat{e}_i)$. Moreover, the reward is subject to the constraint $0 \leq W(\hat{e}_i) \leq B$, where B is the principal's per-household budget. Thus, the principal solves

$$\min_{e_i, W(\cdot)} E_{\varepsilon_i}[\hat{e}_i] \tag{14}$$

subject to

1. $e_i \in \underset{\widetilde{e}_i \geq 0}{\arg\max} \; -\gamma(\widetilde{e}_i - S)^2 - p\widetilde{e}_i + \sigma^2 + E_{\hat{e}_i}[W(\hat{e}_i)|\widetilde{e}_i]$
2. $0 \leq W(\hat{e}_i) \leq B$ for all $\hat{e}_i \geq 0$.

Proposition 5 (Optimal Individual Contract)**.** *A* threshold contract *is an optimal individual contract*

$$W(\hat{e}) = \begin{cases} B & \hat{e} \leq \ell, \\ 0 & \hat{e} > \ell. \end{cases}$$

In other words, the principal's optimal contract rewards the household whenever the energy consumption is below an optimal threshold, ℓ, which is determined by the parameters of the model.

Proof. Let $u(e) = -\gamma(e - S)^2 - pe + \sigma^2$ and define

$$V(e, W(\cdot)) = u(e) + \int W(\hat{e}) f(\hat{e} - e) d\hat{e}$$

At the optimal interior solution we have $V_e(e^*, W^*(\cdot)) = 0$.

Consider the relaxed problem

$$\min_{e, W(\cdot)} e$$

subject to

1. $V_e(e, W(\cdot)) \leq 0$,
2. $W(\hat{e}) - B \leq 0$ for all $\hat{e}$,
3. $-W(\hat{e}) \leq 0$ for all $\hat{e}$,
4. $-e \leq 0$.

The Lagrangian of this problem is

$$\mathcal{L} = e + \lambda \left(u'(e) - \int W(\hat{e}) f_e(\hat{e} - e) d\hat{e} \right) + \theta(\hat{e})(W(\hat{e}) - B) + \eta(\hat{e})(-W(\hat{e})) + \mu e$$

where $\lambda, \mu, \theta(\hat{e}), \eta(\hat{e}) \geq 0$.

Taking FOC w.r.t. $W(\hat{e})$ we get

$$\frac{\partial \mathcal{L}}{\partial W(\hat{e})} = -\lambda f_e(\hat{e} - e) + \theta(\hat{e}) - \eta(\hat{e}).$$

At the optimal solution we have $\frac{\partial \mathcal{L}}{\partial W(\hat{e})} = 0$. Since $f_e(\hat{e} - e) \neq 0$ a.e. we cannot have $\theta(\hat{e}) = \eta(\hat{e}) = 0$ simultaneously when $\lambda > 0$. This means that either $W(\hat{e}) = B$ or $W(\hat{e}) = 0$ for all $\hat{e} \geq 0$. Moreover, $W(\hat{e})$ is non-increasing, since the principal wants to minimize energy consumption. Lastly, at the optimum incentive compatibility requires $V_e(e^*, W^*(\cdot)) = 0$, so $\lambda > 0$ satisfies complementary slackness. $\square$

Online Appendix

A Comparison of Contests and Contracts to Deliver Cost-Effective Energy Conservation

by Teevrat Garg, Jorge Lemus, Guillermo Marshall, and Chi Ta

Supplemental Material – Intended for Online Publication

Appendix C: Treatment rules are provided through the app and via a link included in text messages

Figure C.1: Treatment rules are provided through the app and via a link included in text message

(**a**) App's display

(**b**) Via a link in text messages

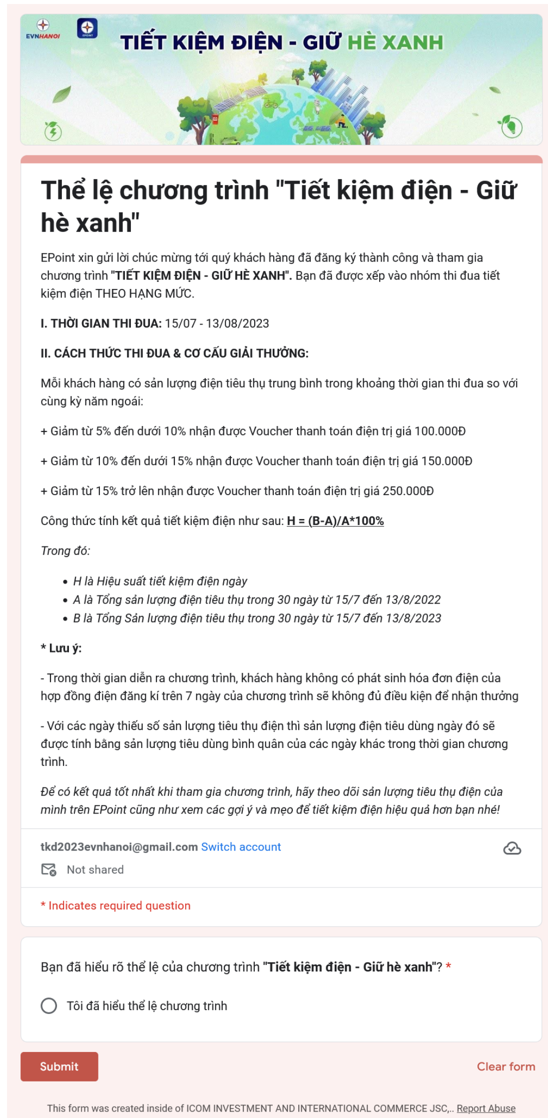


Figure C.2: English Translation of treatment rules

(**a**) Contract 1

Rules of the program "Saving Electricity - Keeping Summer Green"

EPoint is pleased to congratulate customers who have successfully registered and participated in the **"SAVING ELECTRICITY - KEEPING SUMMER GREEN"** program. You have been assigned to the energy-saving competition group based on saving thresholds.

I. PROGRAM DURATION: 15/07 - 13/08/2023

II. PARTICIPATION RULES & AWARD STRUCTURE:

Each customer who reduces their average electricity consumption during the program period compared to the same period last year:

+ by 5% to less than 10% will receive an electricity payment voucher worth 100,000 VND

+ by 10% to less than 15% will receive an electricity payment voucher worth 150,000 VND

+ by 15% or more will receive an electricity payment voucher worth 250,000 VND

The formula for calculating the electricity saving result is as follows: **H = (B-A)/A*100%**

In which:

• H is Daily Electricity Saving Performance

• A is the total electricity consumption in 30 days from 15/7 to 13/8/2022

• B is the Total Electricity Consumption in 30 days from July 15 to August 13, 2023

***Note:**

- Customers who do not incur electricity charges on their registered electricity contract for more than 7 days during the promotion period will not be eligible for the reward.
- On days with missing electricity consumption data, the consumption for that day will be calculated based on the average consumption of the other days during the program period.

For the best results in the program, monitor your electricity consumption on EPoint and check out suggestions and tips to save electricity more effectively!

(**b**) Contract 2

Rules of the program "Saving Electricity - Keeping Summer Green"

EPoint is pleased to congratulate customers who have successfully registered and participated in the **"SAVING ELECTRICITY - KEEPING SUMMER GREEN"** program. You have been assigned to the energy-saving competition group based on saving thresholds.

I. PROGRAM DURATION: 15/07 - 13/08/2023

II. PARTICIPATION RULES & AWARD STRUCTURE:

Each customer who reduces their average electricity consumption during the program period compared to the same period last year:

+ by 10% to less than 15% will receive an electricity payment voucher worth 150,000 VND

+ by 15% to less than 20% will receive an electricity payment voucher worth 250,000 VND

+ by 20% or more will receive an electricity payment voucher worth 350,000 VND

The formula for calculating the electricity saving result is as follows: **H = (B-A)/A*100%**

In which:

• H is Daily Electricity Saving Performance

• A is the total electricity consumption in 30 days from 15/7 to 13/8/2022

• B is the Total Electricity Consumption in 30 days from July 15 to August 13, 2023

***Note:**

- Customers who do not incur electricity charges on their registered electricity contract for more than 7 days during the promotion period will not be eligible for the reward.
- On days with missing electricity consumption data, the consumption for that day will be calculated based on the average consumption of the other days during the program period.

For the best results in the program, monitor your electricity consumption on EPoint and check out suggestions and tips to save electricity more effectively!

(**c**) Contest

Rules of the program "Saving Electricity - Keeping Summer Green"

EPoint is pleased to congratulate customers who have successfully registered and participated in the **"SAVING ELECTRICITY - KEEPING SUMMER GREEN"** program. You have been assigned to the energy-saving group to compete against other participants.

I. PROGRAM DURATION: 15/07 - 13/08/2023

II. PARTICIPATION RULES & AWARD STRUCTURE:

Each customer will compete in electricity savings within a group of no more than 50 households.

Each group will award one electricity voucher worth VND 2 million to the customer who achieves the greatest reduction in electricity consumption compared to the same period in 2022.

The formula for calculating the electricity saving result is as follows: **H = (B-A)/A*100%**

In which:

• H is Daily Electricity Saving Performance

• A is the total electricity consumption in 30 days from 15/7 to 13/8/2022

• B is the Total Electricity Consumption in 30 days from July 15 to August 13, 2023

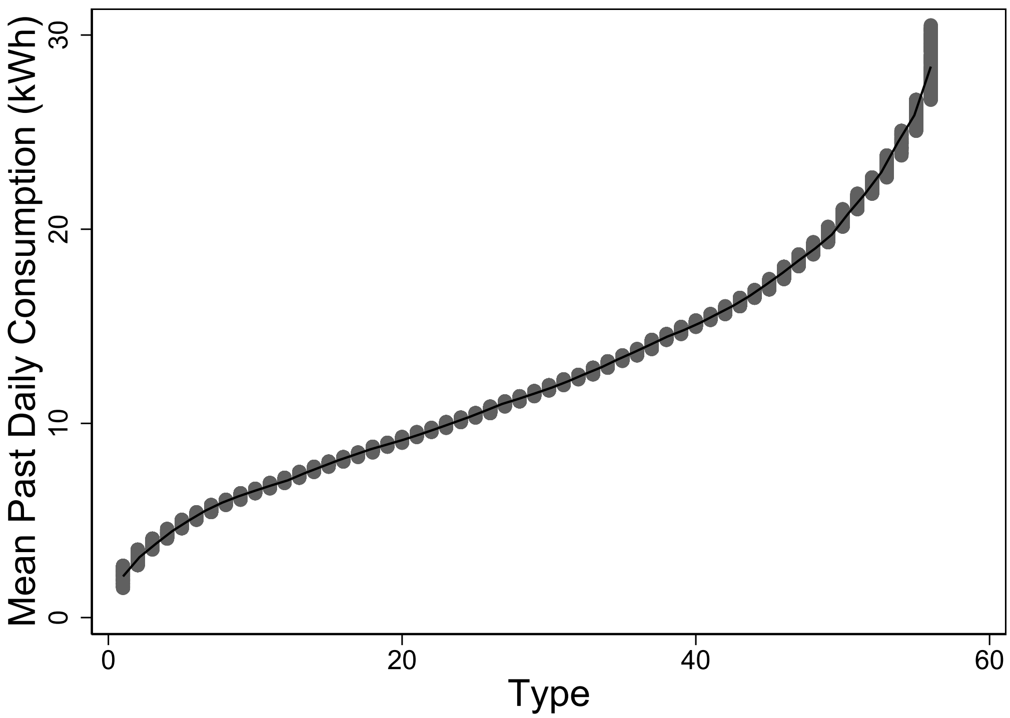
***Note:**

- Customers who do not incur electricity charges on their registered electricity contract for more than 7 days during the promotion period will not be eligible for the reward.
- On days with missing electricity consumption data, the consumption for that day will be calculated based on the average consumption of the other days during the program period.
- If multiple customers achieve the same energy-saving performance, EPoint will prioritize those who registered for the program earlier.

For the best results in the program, monitor your electricity consumption on EPoint and check out suggestions and tips to save electricity more effectively!

Appendix D: Additional tables and figures

Figure D.1: Definition of "type" based on past consumption



Notes: Types are defined based on a household consumption during one-month period, one year prior to the experiment. Higher types typically have higher consumption.

Table D.1: Heterogeneity analysis II: Within-household variation

	(1)	(2)	(3)	(4)
	Consumption (kWh)		Consumption (kWh) (in logs)	
Post * Contract 1	-1.759	-1.742	-0.152	-0.152
	(0.089)	(0.089)	(0.008)	(0.008)
Post * Contract 2	-1.716	-1.722	-0.142	-0.142
	(0.087)	(0.088)	(0.008)	(0.008)
Post * Contest	-1.744	-1.747	-0.150	-0.150
	(0.091)	(0.091)	(0.008)	(0.008)
Post * Contract 1 * Consumption first two weeks	-0.843		-0.005	
	(0.084)		(0.006)	
Post * Contract 2 * Consumption first two weeks	-0.712		-0.003	
	(0.105)		(0.006)	
Post * Contest * Consumption first two weeks	-0.756		-0.011	
	(0.161)		(0.006)	
Post * Contract 1 * Reference consumption		-0.709		-0.007
		(0.075)		(0.005)
Post * Contract 2 * Reference consumption		-0.612		-0.002
		(0.065)		(0.005)
Post * Contest * Reference consumption		-0.688		-0.007
		(0.079)		(0.005)
Observations	564187	564187	558264	558264
Mean	12.820	12.820	2.355	2.355
Test	0.878	0.957	0.374	0.377

Notes: Standard errors clustered at the household level in parentheses. All specifications include day fixed effects and household fixed effects. Row 'Mean' reports the mean of the dependent variable in the estimation sample. Row 'Test' reports the two-sided p-value of an F-test where the null is that treatments 1, 2, and 3 have equal coefficients. All columns restrict the sample from June 1, 2023 to August 13, 2023, dropping the days between July 15 and July 29, 2023. The variables 'Reference consumption' (household's average daily consumption during July 15, 2022, and August 13, 2022) and 'Consumption first two weeks' (the household's average daily consumption between July 15 and July 29, 2023) are standardized (mean zero, standard deviation one).