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

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Abstract

Contests and individual contracts are widely used to incentivize costly, unobservable effort. Which is more cost-effective is theoretically ambiguous and thus an empirical question. We conduct a field experiment to inform the design of energy conservation incentives in a city with a strained electrical grid, where prices cannot be used for demand management, and find that both mechanisms achieve similar energy savings (7 to 9 percent), but contests do so at half the cost. Recognizing that our experimental design may be comparing suboptimal contracts and contests, we introduce a novel empirical framework to compute and compare their optimal designs. Using non-parametric estimates from our structural model of energy consumption, we show that optimally designed contests outperform optimal contracts for a given budget per household. Our marginal abatement cost estimates suggest that these programs can be profitable for the utility even without carbon pricing. Our findings contribute to the broader organizational question of contests versus contracts, while also advancing demand-side management policies in residential electricity—particularly in low- and middle-income countries. Our methodology can be applied to various settings using increasingly available utility data.

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1 Introduction

Incentivizing costly, unobservable effort remains a long-standing challenge in economics. Two widely used mechanisms to incentivize effort are individual contracts, which reward absolute performance (e.g., a bonus for meeting a fixed target), and contests, which reward relative performance (e.g., a prize for the top performer). While the literature has documented the performance of contracts (e.g., Lazear, 2000; Bandiera et al., 2005) and contests (e.g., Gross, 2020; Bhattacharya, 2021), which is more cost-effective is theoretically ambiguous (e.g., Green and Stokey, 1983) and remains an open empirical question. In this paper, we conduct a field experiment to evaluate the cost-effectiveness of contracts and contests in the context of an energy conservation program.

In many countries, the electrical grid experiences strain during the summer months, prompting the electric utilities to incentivize energy conservation. When electricity prices are fixed by regulation—precluding, for instance, the use of dynamic pricing—electric utilities must find non-price mechanisms to provide these incentives. These conditions are present in Hanoi, Vietnam, where the electric utility has already been deploying low-cost approaches in the form of behavioral nudges.[1] In collaboration with EVN Hanoi, the city's state-owned and exclusive electric utility, we evaluate the effectiveness of contracts and contests above and beyond nudges. We ask three questions: (1) How much energy conservation do contests and individual contracts induce? (2) How cost-effective are these programs? (3) Under what conditions are contests more cost efficient?

We first address these questions by implementing a randomized controlled trial (RCT) that offered financial incentives to households for energy conservation. We recruited nearly 12,000 households to participate in our study and randomized them into a control and three treatment groups spanning 30 days from July 15, 2023 to August 13, 2023. The first two treatment groups received contracts with different terms and conditions, whereas households in the third treatment group participated in contests. We used the electric utility's mobile app as a platform for our energy savings contests and contracts.

We utilize the experimental data to show that both contests and contracts can deliver energy conservation. On average, households in the treatment groups reduce their energy consumption by approximately 7 to 9 percent in comparison to the control group during

[1] Beyond Vietnam, 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., 2025) and direct "bonus" payments to keep energy use below a target maximum.

the intervention. Importantly, we find that the energy savings persist for at least one week after the end of the experiment before returning to just below pre-experimental levels. That is, the energy reductions were additional relative to an otherwise identical control group.

We leverage variation in the compensation schemes of the contract treatment groups to show that households are responding to marginal incentives in their electricity use choices. Specifically, we find that offering a payment for achieving a specific reduction (e.g., $5 for a 5 percent energy reduction) leads to a higher probability of achieving that target reduction compared to when no such payment is offered. In other words, we can nominally reject a model where households respond with a pre-determined level of conservation effort that is invariant to level of incentives. In terms of cost-effectiveness, although we cannot reject the null hypothesis that the treatment effects are equal between contracts and contests, we show that contests deliver a statistically similar level of energy conservation as contracts do, but at nearly half the cost per household.

Under what conditions do contests yield greater energy conservation per unit cost to the utility than individual contracts? While our experimental results suggest that contests are more cost-effective, they do not provide a direct answer to this question for two reasons. First, in our experiment, we compare a contest with two contracts, neither of which is necessarily optimal. We wish to compare the performance of optimized contracts against an optimal contest. Second, average payments per household are not equal across treatments in our experiment. We thus answer the question using a structural model of household energy consumption when faced with incentives to conserve energy. We estimate the model using our experimental variation, and we use the model estimates to recover an optimal contract and compare it with a cost-equivalent contest.

In our model, a household trades off its desire for energy consumption—with a satiation point defined as demand when energy is free—against the cost of energy. Households face idiosyncratic shocks (e.g., appliance malfunction) as well as shocks common to all households (e.g., heat wave). In this context, contests and individual contracts create different incentives for household energy conservation. Common shocks—events like extreme weather—impact all households, so relative performance is unaffected by them. In contrast, common shocks can affect the performance under individual contracts by making it very difficult or too easy for households to meet the contract's energy conservation target.[2] Households in a contest may get discouraged as competition grows, but this hinges on the shape of the distribution of idiosyncratic shocks (List et al., 2020). However, the

[2]If the weather is unusually cool in a summer month, households may achieve low energy use levels without exerting effort.

aggregate effort can increase in a large contest, even if individual households get discouraged. In contrast, households' incentives to save energy caused by an individual contract are unchanged by the behavior of other households.

The comparison of energy consumption across these mechanisms is generally ambiguous (see, e.g., Green and Stokey, 1983), and we derive a condition under which contests are more cost effective. Using our model estimates—including non-parametric estimates of the idiosyncratic shock distributions—we show that when the contest designer can choose the number of participants in each contest (as the utility can in our case) and equalizes the expected payment per household across mechanisms, the optimal contest dominates the optimal contract in terms of delivering energy conservation. This prediction aligns with our experimental finding that contests are more cost-efficient than contracts. Our model also allows us to recover the first experimental estimate of the short-run price elasticity of demand in Vietnam. We estimate a short-run elasticity of -0.0888, which is in the range of estimates in the United States (Jessoe and Rapson, 2014; Bollinger and Hartmann, 2020) and the summer in Japan (Ito et al., 2018), 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. These estimates help answer the question of whether incentivizing households using these incentive schemes is socially (or privately) efficient. 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 in the absence of environmental policy 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.

These findings have implications for the design of demand management interventions. We do note that besides the economic forces favoring contests over contracts, individual contracts and contests present different administrative challenges and financial implications for the utility. Implementing individual contracts requires the utility to determine appropriate energy conservation 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 competition 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.

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, either not using a large-scale randomized field experiment (Bull et al., 1987; Knoeber and Thurman, 1994) or analyzing different types of relative incentives (Bandiera et al., 2005). 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. The paucity of field experiments comparing contests and contracts (or incentive schemes more broadly) can be attributed in part to the need to collaborate with institutions or companies with a large number of agents (e.g., workers or customers). We bridge this gap by providing 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).[3] Our results hinge on the shape of the idiosyncratic shock distribution, which has been shown to be important for contest design features such as the number of players (List et al., 2020) and number of prizes (Drugov and Ryvkin, 2020).

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

[3]Our paper also relates to work on lotteries as a mechanism to incentivize effort. See, for example, DellaVigna and Pope (2018); Fabbri et al. (2019); Duch et al. (2023); Campos-Mercade et al. (2024). The key practical difference between a contest or contract and a lottery is that the former is used to incentivize effort when a non-binary performance measure is available (e.g., energy savings), whereas lotteries are used to reward effort regardless of performance (e.g., individuals who receive a vaccine are rewarded by entry into a lottery).

in LMICs.[4] 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 conducted 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 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, EVN Hanoi, 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 employ dynamic pricing, ostensibly to protect consumers from volatile pricing.

[4] 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.

2.2 Experimental Design

We conducted our randomized field experiment in Hanoi, Vietnam in the summer of 2023. Collaborating with EVN Hanoi, 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.[5]

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 control group was not assigned to a contest or contract. Participants could use their smart meters to monitor their progress by default, so all households, including the control group, received information about their past and current daily electricity use on the utility company's app or by logging into their account on the utility's website.

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 a notification that the incentive period had started. The incentives faced by each group 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

[5]EVN Hanoi, 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.

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."[6]

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.

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.

Structure of incentives We chose this structure of incentives for the treatment groups for three 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

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

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

Lastly, the contest design literature shows that when idiosyncratic shocks follow a distribution that does not have "heavy tails," aggregate effort increases with the number of players (List et al., 2020), and a single prize is optimal (Drugov and Ryvkin, 2020). We presumed that shocks did not have a heavy tail, which is why we chose a single prize and $N = 50$ households per contest.

Departures from our pre-analysis plan There are two departures from our pre-specified research plan that are worth mentioning, although neither affects the validity of our estimates. First, the experiment experienced unexpected delays due to technical issues. Households needed to update the app to view the specific rules for their treatment. To help remedy the issue, all households received individual information about their treatment via a text message containing a link to the rules on July 24, 2023, about 10 days after the start of the program.[8] In these communications, households were not informed about the presence of other treatments within our study. 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.

[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.

[8] 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.

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.[9] 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 and carbon emissions.

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:

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

[9] 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.

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 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 experi-

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.

mental 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.[10] 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

[10]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).

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.

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.[11]

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.

[11] 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 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, 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.[12]

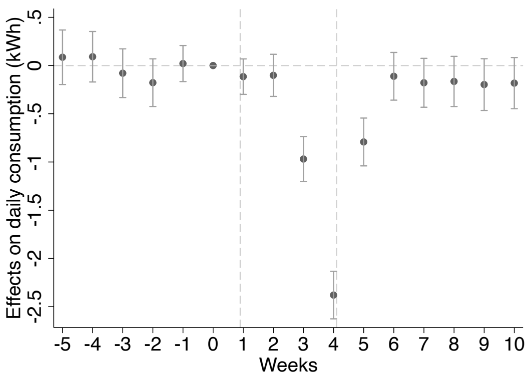
Figure 1 presents estimates for equation (2), where we allow for the treatment coefficients to vary over time, using data from after the experimental period. Figure 1A illustrates the difference in energy usage (in kWh) between treatment group 1 (contract with low thresholds) and the control group over 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 represents the week before the experiment commencement, and weeks 1 through 4 correspond to the experiment period (July 15 through August 13).[13]

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

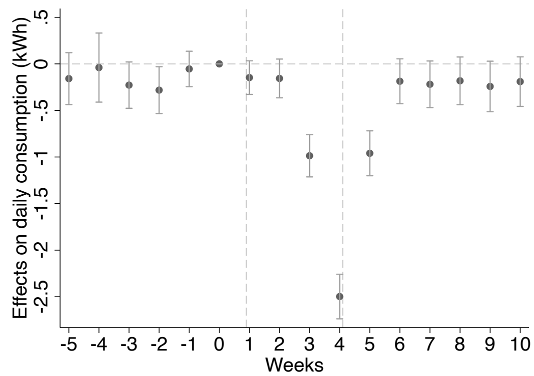
[12] According to our power calculation provided our pre-analysis plan, it is highly probable that our sample size is inadequate for detecting any difference of 3% or less in magnitude.

[13] 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.

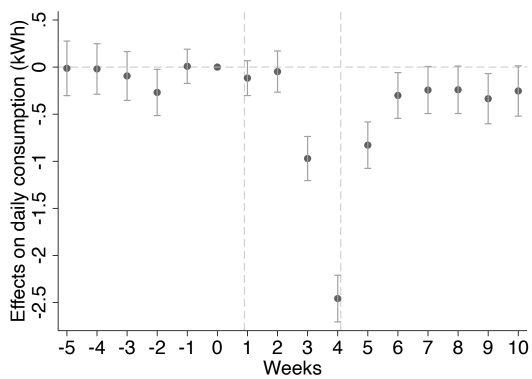
Figure 1: Time effects: Within-household variation



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.

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.[14]

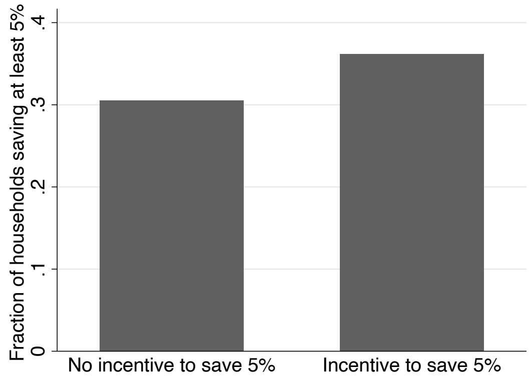
3.1 A comparison of treatment effects

A noteworthy fact in Tables 2 and 3 is that the treatment effects are statistically indistinguishable across treatment groups. A few explanations could rationalize this finding. First, households may respond to marginal incentives, but these vary across treatments, as do expected payments. As we will note later, the expected payment of households in the contract treatment groups was 80 to 85 percent greater than that of households in the contest group (we come back to this later). In this sense, the findings in Figure 1 (and Tables 2 and 3) are consistent with contests being more efficient (i.e., the same marginal incentives can be induced with a lower payment), but the greater expected payment in the contract treatments makes the treatment effects similar. The structural model and estimation framework presented in Section 4 will offer a better comparison of the performance of contracts and contests, given any equivalent expected payout.

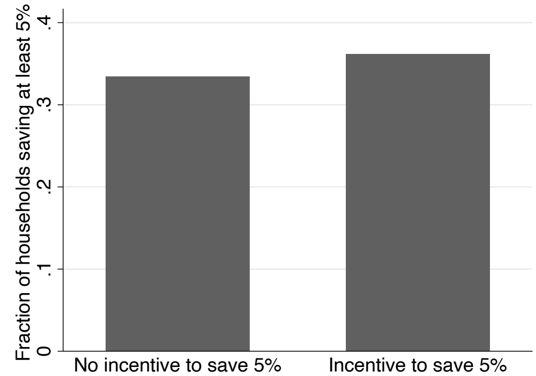
Second, households may not respond to marginal incentives (i.e., fine tuning effort based on the perceived costs and benefits of saving an additional kWh). We test this possibility in Figure 2, where we exploit variation in the design of the contract treatments to measure whether households respond to marginal incentives. Specifically, we measure the impact of being offered a guaranteed payment for saving a predetermined amount (e.g., \$5 for saving at least 5 percent) on an indicator for whether the household saved at least that amount. Note that the contract treatments feature variation in the thresholds that trigger payments—i.e., 'Contract 1' offers payments for saving 5, 10, and 15%, whereas 'Contract 2' offers payments for saving 10, 15, and 20%. The contest and control groups are never offered guaranteed payments for saving more than a predetermined amount. Whether we include all households (Panels A, C, E) or restrict the sample to households being offered some incentive to save energy (Panels B, D, F), we find evidence of households responding to marginal incentives (i.e., households are more likely to save at least x percent if offered

[14] Figure D.1 in the Online Appendix presents a similar analysis but reports means instead of regression coefficients. The same patterns emerge.

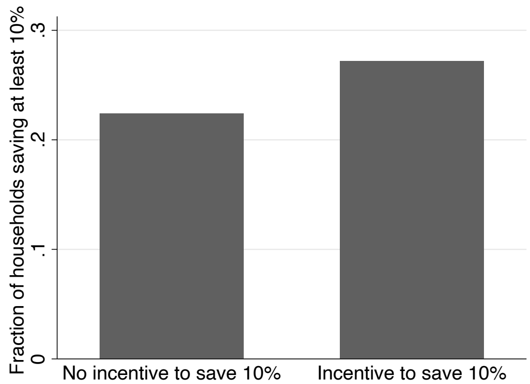
Figure 2: Share of households saving at least X percent when facing incentives to save X percent



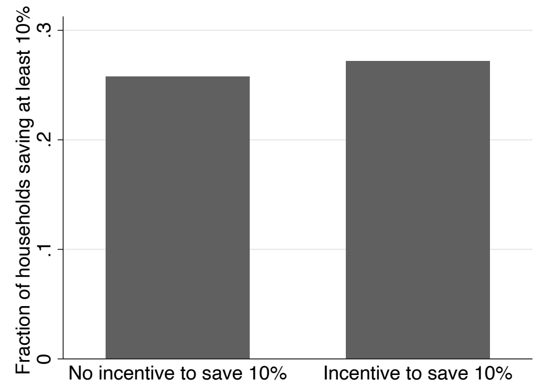
A) 5% threshold



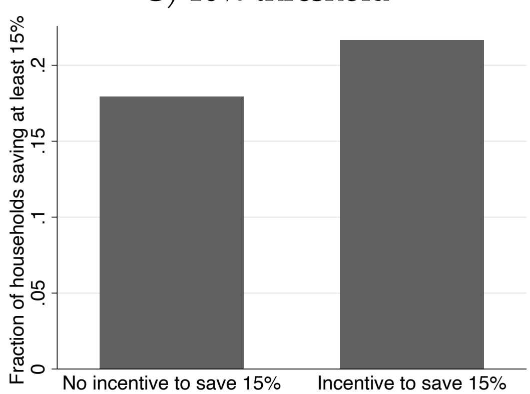
B) 5% threshold (excluding control group)



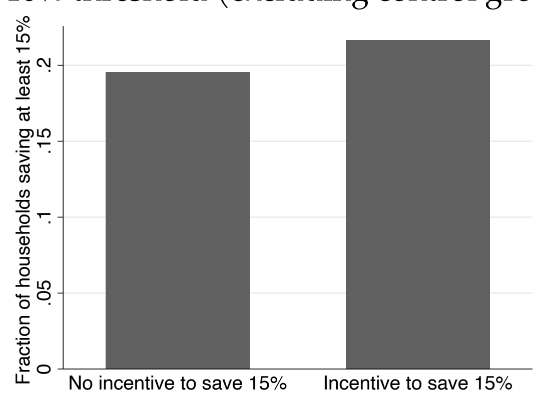
C) 10% threshold



D) 10% threshold (excluding control group)



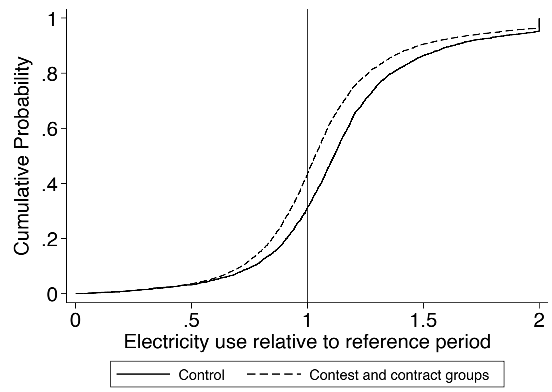
E) 15% threshold



F) 15% threshold (excluding control group)

Notes: An observation is a household. Each graph reports the fraction of households saving at least X percent (relative to their consumption in the same period in the year prior) by whether the household receives a direct payment for saving X percent. Note that households in the contest or control groups are never offered payments for saving more than a predetermined threshold. Panels B, D, and F restrict the sample to households being paid to save electricity (i.e., those assigned to a contract or contest).

Figure 3: Household-level electricity consumption reductions



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.

a payment to save at least that amount). These findings suggest that marginal incentives are at work, rejecting the notion that the design of the incentive scheme is irrelevant.

3.2 Heterogeneity analysis

In this section, we explore heterogeneity along two dimensions. First, we ask how the energy use reductions are distributed across participants. 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 3 displays the cumulative distribution functions of these ratios, by whether the household was incentivized to conserve energy. 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: 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).

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.2 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 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 households 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 a satiation 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 managing demand on extremely hot days.

3.3 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,

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.

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 develop a model that rationalizes households' energy consumption choices under various incentive schemes—namely, no incentives (control), individual contracts, and contests—and also enables us to perform counterfactual analyses to answer additional questions: (1) How do contests and individual contracts compare in inducing energy savings, keeping expected payments per household fixed? (2) What is the optimal design of incentive schemes given households' behavioral responses? (3) What are the welfare and policy implications of different incentive structures?

Using data from our experiment, we estimate the model parameters by comparing observed outcomes with the model's predictions. The structural model complements our experimental findings by allowing us to compare the performance of a contest against an *optimal* contract—one derived from the model's primitive parameters—keeping expected payments per household fixed. Unlike our comparisons in the previous section—where we compare two arbitrary contracts, not necessarily optimal ones, against a contest—this allows us to effectively compare optimized contests and contracts. As well, the model also enables us to compute demand functions and welfare, which allows us to determine the price elasticity of electricity consumption and compare incentive programs from the perspective of households.

4.1 Modeling Household Energy Consumption

A household's *satiation* energy consumption is $S \geq 0$ (i.e., the level consumed when energy is free), which may vary across seasons (e.g., it may be higher in warm months). The household chooses its target (or expected) energy consumption, $e \geq 0$, which is then affected by an ex post idiosyncratic shock ε (i.e., after choosing e)—e.g., malfunctioning appliance, unexpected travel—with $\varepsilon \sim F(\cdot)$, $E[\varepsilon] = 0$, and density $f(\cdot)$, so the *actual* energy consumption is $\hat{e} = e + \varepsilon$.[15] The household's expected payoff is

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

The payoff captures that the household values matching its actual consumption with S but dislikes paying for energy, which is priced at p per kWh.[16] The utility loss from deviations from the household's satiation level reflects discomfort associated with under- or over-consumption. The parameter γ measures the importance of matching S relative to the cost of energy.[17]

Simple algebra shows that, ignoring the constant Var$[\varepsilon]$, the household payoff is

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

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

Proposition 1 (No Incentives; Control Group)**.** *Without an incentive for energy conservation,*

[15] Households set their target consumption based on expected conditions in a given time period. We later study comparative statics on S, which reflect that variation in weather throughout the year determines different values of S and a household's target consumption.

[16] Our formulation assumes that households evaluate uncertain payoffs according to their expected value. This assumption is for tractability. In reality, behavioral biases (e.g., probability weighting or risk aversion) may introduce different effects.

[17] This analysis assumes homogeneous preferences. In our empirical estimation, we allow for different household types.

a household's energy consumption (assuming an interior solution) is given by

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

The household always consumes less than its satiation point, S. If energy was very cheap (low p) or if matching S was highly important (high γ), the household's consumption would be very close to S. However, a high energy price (high p) or low value of matching S (low γ), reduces the household's consumption.

We now consider the use of individual contracts or a contest as an incentive for energy conservation. Consider a set of N households with the same preferences, i.e., the same parameters S, γ, and distribution F. These households make simultaneous energy-consumption choices. Let $\hat{e}_i = e_i + \varepsilon_i$ be household i's realized consumption and $\hat{\mathbf{e}} = (\hat{e}_1, ..., \hat{e}_N)$. We assume that the shocks ε_i are independent and identically distributed, $\varepsilon_i \sim F$.

Under an incentive program that rewards energy conservation, 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 households as given, household i chooses 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.[18] Under an individual contract with threshold ℓ, household i's chooses its 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 to this problem. Households may ignore contracts that are too demanding, i.e., those that require extremely high energy conservation to receive a relatively small reward. Alternatively, if the reward is huge, households could "shut down" and consume zero. We ignore these corner solutions as the monetary incentives we consider are relatively small.

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

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*

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

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 has a unique point z such that $f'(z) = 0$, and consumption is interior at the optimal threshold, then $\ell^* = e^*_{contract} + z$.*

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 satiation 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 $\ell^* = e^*_{\text{contract}} + z$ in (6), we obtain

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

where z is the unique point where $f'(z) = 0$.

The energy reduction induced by the optimal contract is $\frac{Bf(z)}{2\gamma}$, which depends on the reward, B, the sensitivity to matching S, γ, and the density of receiving an idiosyncratic shock z, $f(z)$. Also note that as B increases, the optimal threshold ℓ^* (which equals $e^*_{\text{contract}} + z$) decreases. That is, when the reward is larger, the contract becomes more demanding in terms of energy reduction. Finally, note that the probability that the household earns B is $F(z)$.

Importantly, the optimal threshold ℓ^* depends on the preference parameters S and γ. If a household's satiation 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 satiation consumption point, which may fluctuate over time.

Individual Contest. We now consider the use of a contest to promote energy conservation. Suppose the sponsor of an energy-conservation program organizes a winner-takes-all contest, where N homogeneous households simultaneously make energy consumption choices, e_i, and the household with the lowest realized consumption, $\hat{e}_i$, receives a prize of V.[19] 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 $(1 - F(e_i + \varepsilon_i - e^*))^{N-1}$ (ties occur with probability zero). 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.

It is worth noting that we have not considered multiple prizes because awarding a single prize is optimal when the idiosyncratic shock distribution does not have "heavy tails" (Drugov and Ryvkin, 2020), which turns out to be the case in our empirical application.[20]

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

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

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

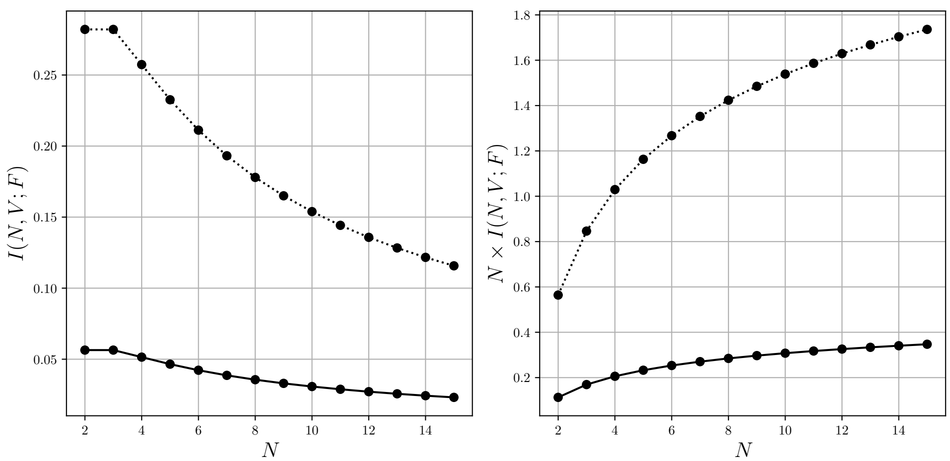
It is important to understand how the incentive generated by a contest changes with the number of competitors. List et al. (2020) show that the shape of the distribution of idiosyncratic shocks plays a critical role. For example, when $\varepsilon \sim \text{Normal}(0, \sigma^2)$, for any fixed prize V, individual households save *less* energy as the number of competitors increases. That is, $I(V, N; F)$ goes to zero as N grows. However, more competitors increase *aggregate* energy savings, that is, $N \times I(V, N; F)$ increases with N.

Figure 4 illustrates these points. The left panel of Figure 4 shows the incentive for energy conservation of one of the households participating in an N-household contest with a prize $V = 1$ and $F = \text{Normal}(0, \sigma^2)$, with $\sigma = 1$ (dotted line) and $\sigma = 5$ (solid line) for different

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

[20] A single prize has also been shown to be optimal in other settings. For instance, Moldovanu and Sela (2001) finds that in a contest where participants have private information about their types, a single prize is optimal when costs are concave.

Figure 4: Comparing contests and optimal contracts



Notes: The left panel shows $I(N, V, F)$, the energy-conservation of one household participating in an N-household contest, for a prize $V = 1$ and $F = \text{Normal}(0, \sigma^2)$, with $\sigma = 1$ (dotted line) and $\sigma = 5$ (solid line) for different values of N. The right panel shows $N \times I(N, V, F)$, the aggregate energy-conservation of an N-household contest, for a prize $V = 1$ and $F = \text{Normal}(0, \sigma^2)$, with $\sigma = 1$ (dotted line) and $\sigma = 5$ (solid line) for different values of N.

values of N. The figure shows that $I(N, V, F)$ decreases with N, reflecting the impact of more competition in the contest. When σ is larger, extreme shocks play a prominent role in determining the contest winner, which hinders energy-conservation incentives. The right panel of Figure 4 shows the aggregate incentive for energy conservation, $N \times I(N, V, F)$, which increases with N.

Lastly, note that the energy-reduction incentive induced by the contest is independent of the common-shock parameter S—capturing, for example, seasonal effects—which matters for the design of an optimal contract but does not play any role in the incentives created by a contest.

4.2 Individual Contracts versus Contests

When considering strategies to reduce household energy usage, is it more effective to give a household an individual contract or place it in an N-households contest? The answer to this question is generally ambiguous. In certain scenarios, the competitive nature of a contest may drive lower energy consumption compared to a standalone contract, while in other cases, contests can discourage participants and individual contracts might provide better incentives. By comparing the equations that determine optimal consumption levels in each scenario—i.e., equations (7) and (9)—we find that the *shape* of the shock distribution plays a central role. To streamline our results, we operate under the following assumption:

Assumption 1. *The density of the idiosyncratic shocks distribution is such that there exists a unique point z such that $f'(z) = 0$.*

Under this assumption, assuming interior solutions, we have the following result:

Proposition 5. *An individual optimal contract with reward B offered to N households induces more energy conservation than a N-household contest awarding a prize $V = NBF(z)$ when*

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

To understand this proposition, 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. Comparing (7) and (9), the household saves more energy under an individual contract when

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

In terms of the cost of each energy-conservation program, the *expected* cost of offering individual contracts is $NBF(z)$, whereas the *certain* cost of the contest is V. Imposing that both programs cost the same, $NBF(z) = V$, individual contracts save more energy than a contest when

$$\frac{f(z)}{F(z)} \geq N \int (N-1)(1-F(\varepsilon_i))^{N-2} f^2(\varepsilon_i) d\varepsilon_i. \tag{12}$$

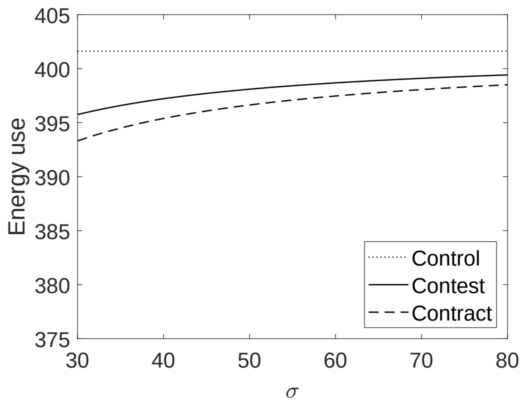
Whether inequality (12) holds hinges on the shape of the shock distribution.

Proposition 6 provides dominance results for particular sets of distributions. It shows that for single-peaked distributions with $F(z) \leq 0.5$, incentivizing two households with individual contracts dominates a 2-household contest. Assuming further that shocks are normally distributed, the proposition also shows that a N-household contest dominates N optimal contracts for $N \geq 3$.

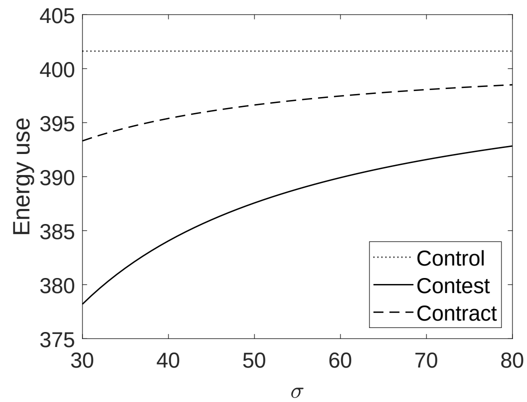
Proposition 6. *When $N = 2$, optimal contracts dominate a contest (that is, inequality (12) holds) for any distribution such that $F(z) \leq 0.5$ and, for all x, $f(x) \leq f(z)$. When $N \geq 3$, if $\varepsilon \sim$ Normal$(0, \sigma^2)$ then an N-household contest induces more energy conservation.*

Figure 5 illustrates this proposition by comparing the per-household energy consumption under three programs: (i) an N-household contest, (ii) an individual optimal contract,

Figure 5: Comparing contests and optimal contracts



A) $N = 2$



B) $N = 50$

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.

and (iii) a no-incentive group ("control"). In each comparison, the expected payout per household is held constant across the contest and the contract. The figure confirms that the relative performance of a contract versus a contest depends on contest size: for $N = 2$, the optimal contract yields greater energy savings, whereas for $N = 50$, the contest leads to larger reductions.

As previously discussed, although adding more households to a contest weakens each individual's marginal incentive to conserve energy, the overall savings may still rise with N, depending on the shape of the idiosyncratic shock distribution. Because a sufficiently large contest can outperform a cost-equivalent individual contract, it is critical to empirically estimate the distribution of those shocks.

4.3 Estimation

In the empirical analysis, we classify each household as one of $K = 56$ types, each type denoted by $\kappa = 1, ..., K$. We define types based on energy consumption between July 15 and August 13, 2022—i.e., one year before the beginning of our experiment. Figure D.2 in the Online Appendix plots the average electricity consumption of each type. Let N_κ be the number of households of type κ. On average, there are 180.73 households of each type, with some types having as few as 145 households and others as many as 250.[21] Let also

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

$N_{\kappa,t}$ be the number of households of type κ assigned to treatment group 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 idiosyncratic shocks F_κ. We assume the shocks are independent. For estimating the parameters, $\Theta_\kappa = (\gamma_\kappa, S_\kappa, F_\kappa(\cdot))$, 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 F_κ:** To estimate F_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 $E[e_{i,\kappa,t}] = e^*_{t,\kappa}(\Theta_\kappa)$, and, therefore, $\varepsilon_{i,\kappa,t} = e_{i,\kappa,t} - E[e_{i,\kappa,t}]$. We estimate F_κ (along with its density, f_κ) using a kernel smoothing function over the vector of residuals $\{e_{i,\kappa,t} - \bar{e}_{i,\kappa,t}\}_{i,t}$, where

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

2. **Estimation of γ_κ:** To estimate γ_κ, we rely on our estimate of F_κ, 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 control and contest groups. Specifically, from equations (3) and (9), we have that

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

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 $\hat{F}_\kappa$, $N = 50$, 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). Specifically, we make use of the following estimator,

$$\hat{S}_\kappa = \bar{e}_{\kappa,\text{control}} + \frac{p}{2\hat{\gamma}_\kappa}.$$

4. **Practical Considerations.** In practice, to gain power in estimating the objects γ_κ and F_κ, we group types into four groups: $(\gamma_\kappa, F_\kappa) = (\gamma_1, F_1)$ for $\kappa = 1, ..., 14$; $(\gamma_\kappa, F_\kappa) = (\gamma_{15}, F_{15})$ for $\kappa = 15, ..., 28$; $(\gamma_\kappa, F_\kappa) = (\gamma_{29}, F_{29})$ for $\kappa = 29, ..., 42$; $(\gamma_\kappa, F_\kappa) = (\gamma_{43}, F_{43})$ for $\kappa = 43, ..., 56$. This grouping requires an estimation of four different γ parameters and four distribution functions. We estimate $(S_\kappa)_{\kappa=1}^{56}$ separately for each type. Hence, we estimate a total of 60 parameters and four distribution functions.

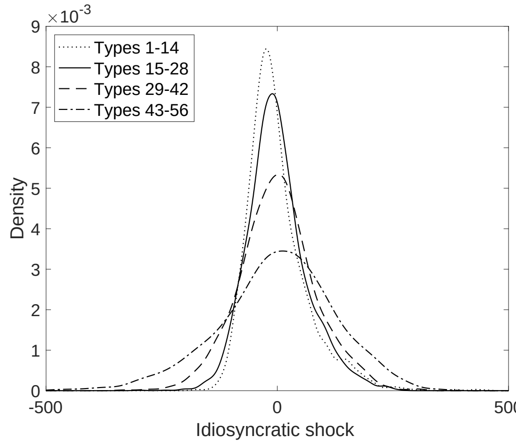
Figure 6 reports our estimates for the density functions of idiosyncratic shocks (Panel A) as well as the distribution of energy consumption satiation points (Panel B). The density function estimates reveal greater dispersion in idiosyncratic shocks for the households with the greatest electricity consumption (types are ordered by average consumption, from lowest to highest). We reject the null hypothesis that the idiosyncratic shocks distribute normal using a Kolmogorov-Smirnov test—the tails of the distributions are too thick. The heterogeneity in satiation points corresponds to the consumption heterogeneity across household types. Lastly, Table D.3 in the Online Appendix reports our estimates for γ_κ.

4.4 Counterfactual Analysis

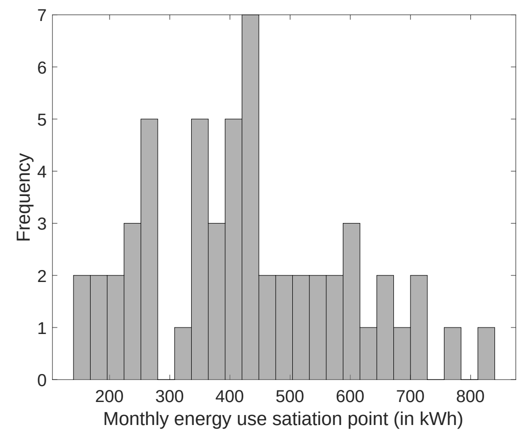
Counterfactual Contracts and Contests. We use our empirical model to simulate the average household consumption under different energy-saving incentives, keeping the expected payment per household fixed across incentive programs.[22] This complements our

[22]Our model in Section 4.1 features a *quadratic* loss when energy consumption deviates from a household's satiation point. This assumption can be reasonable for small deviations relative to S but it might be questionable for very large deviations. For this reason, the exercises in this section focus on policies that moderately change consumption.

Figure 6: Estimates of model parameters: $\{F_\kappa\}_\kappa$ and $\{S_\kappa\}_\kappa$



A) Density functions of idiosyncratic shocks, F_κ



B) Satiation points, S_κ

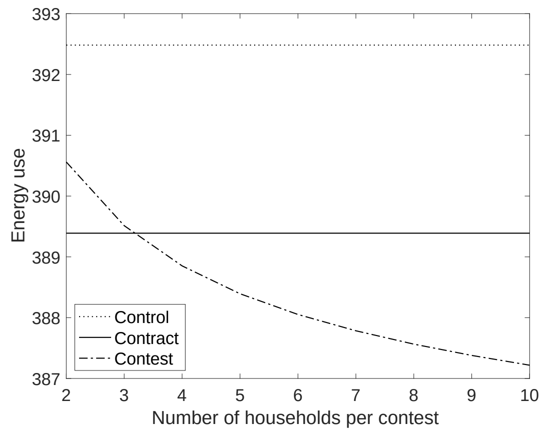
Notes: Panel A: The figure shows the estimated density function of idiosyncratic shocks for the four groups of household types. We use a normal smoothing function with bandwidths 10.99 (types 1-14), 12.07 (types 15-28), 16.57 (types 29-42), and 25.3 (types (43-56). Panel B: The histogram shows the estimates of S_κ for all 56 types, where an observation is a household type.

comparisons in Section 3 in two ways: i) we are now able to compare an optimized contract against a contest, and ii) we keep expected payments per household fixed across programs in all comparisons. Furthermore, Proposition 6 shows that except for some particular cases, the relative performance of contests and contracts is generally ambiguous and thus an empirical question.

Figure 7 shows the average monthly energy consumption of a household when offered no incentive (control), an optimal contract, or participating in a contest with N households. The figure shows that the optimal contract dominates the contest when the number of households per contest is small (i.e., $N \leq 3$), but contests dominate when the contests are larger. In fact, the figure (and our estimates more broadly) show that the average consumption of a contest participant is decreasing in the number of households per contest. Given that we are keeping the expected payment per household fixed across incentives and we are comparing an optimal contract against a contest, these findings suggest that contests are more cost-effective in inducing energy conservation in this setting.

Figure 8 (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). In line with Figure 7, the figure shows that contests dominate the optimal contract

Figure 7: Comparing contests and optimal contracts, by number of households per contest



Notes: The figures plot the average energy consumption of a household when faced with various incentive schemes using our model estimates for different values of the number of households per contest.

Table 6: Energy Savings for Expected Payout in Our Experiment

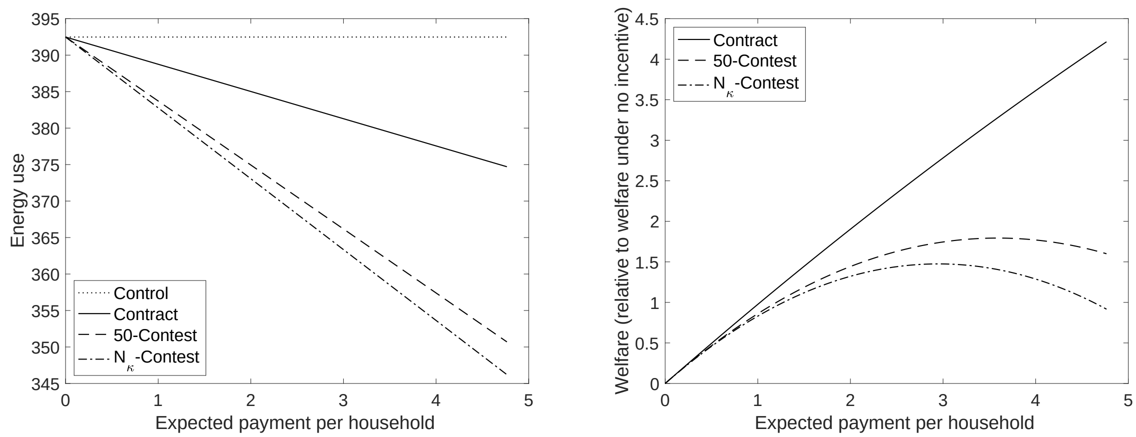
E[Payout]	Control	ℓ^*-Contract	50-Contest	N_κ-Contest
1.74	392.4826	385.9760	377.1894	375.5471
3.14	392.4826	380.7494	364.9047	361.9431
3.21	392.4826	380.5005	364.3197	361.2953

and that larger contests induce larger gains.

Table 6 provides similar information but restricts 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 380.75. This represents a 3 percent reduction relative to the control group. Instead, a 50-household contest achieves a 7 percent reduction. Across payout levels, the contests dominate the optimal contract and the energy savings increase in the size of the expected payout.

Figure 8 (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 per household of a contract and a contest. 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)

Figure 8: Comparing contests and optimal contracts, by expected payment



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).

relative to the contest treatments, for a given expected payment (see Figure 8, Panel A).[23] 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.

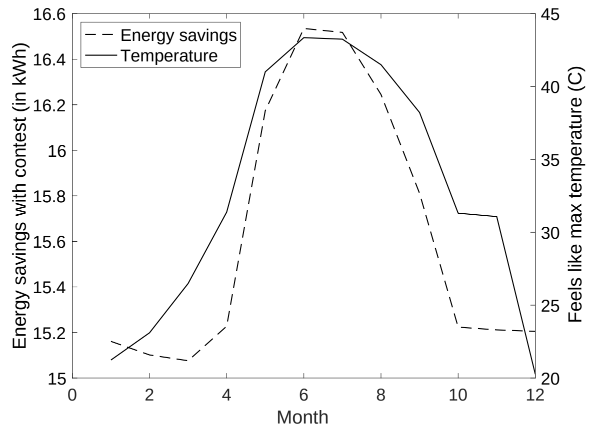
Weather Variation. The cost-effectiveness of an energy-saving program can vary over the year due to differences in weather. We simulate the energy savings caused by a contest like the one offered in our experiment (i.e., 50 households per contest with a prize of $87) across the twelve months before the experiment. To compute the energy savings, we estimate the values of $S_{t,\kappa}$ and the distribution of idiosyncratic shocks $F_{t,\kappa}$ for each period t, using household-level consumption data, following the same estimation procedure discussed above. We assume that the values of γ_κ remain constant throughout.[24]

Figure 9 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

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

[24] We project the estimated values of $S_{t,\kappa}$ and $I(V, N; F_\kappa)$ (see equation 9) on average monthly temperature and average monthly temperature (squared), type by type, and use the fitted values of $S_{t,\kappa}$ and $I(V, N; F_\kappa)$ for the analysis.

Figure 9: 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).

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.

Price Elasticity of Demand. Using our model 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

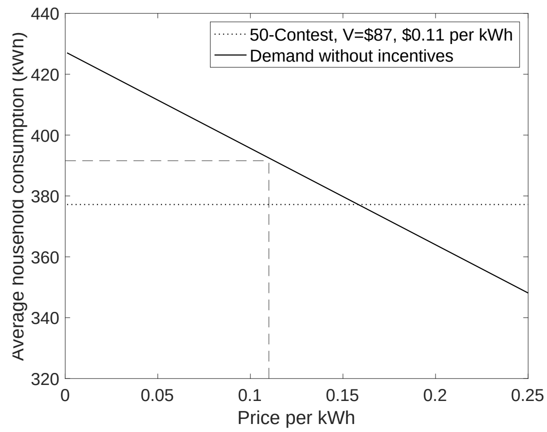
$$D(p) = \sum_{\kappa=1}^{K} \alpha_\kappa \left(S_\kappa - \frac{p}{2\gamma_\kappa} \right),$$

where α_κ is the fraction of households of type κ.

Using our model estimates for α_κ, S_κ, and γ_κ, and the average price of electricity in Vietnam of $p = 0.11$ dollars per kWh, the expected energy demand function is given by $D(p) = 427.3439 - 316.9205p$. Using this demand curve, the price-elasticity of energy consumption at the current price is given by -0.0888.

Figure 10 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 427 kWh on average. The figure also

Figure 10: Estimated Average Demand Curve



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 377 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 $0.158 per kWh, i.e., a 43 percent price increase.

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 7. 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 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 7. 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 7, 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

Table 7: 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

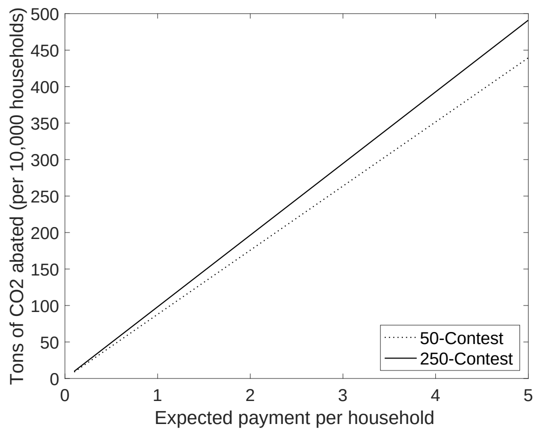
Notes: The consumption reduction values are based on the estimates in columns 1 and 2 of Table 7. Consumption reduction, profit loss, payment, and CO_2 abated are measured at the household level. MAC is computed using the formula Payment/CO_2 abated (in tons). MAC, including profit loss (in USD) is computed using the formula (Payment + profit loss)/CO_2 abated (in tons). See the discussion in the text for more details.

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., 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 \$59.45-\$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 \$11.95 relative

Figure 11: 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.

to the MAC when using a contest with 50 households. Figure 11 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. To understand the conditions under which contests outperform contracts, we develop and estimate a structural model of household energy demand with satiation and unanticipated consumption shocks. Using non-parametric estimates of the idiosyncratic shock distribution, we recover the optimal contract and compare it to a cost-equivalent optimal contest. Our model predicts that, when the contest designer can choose group size and fix expected payments per household, contests systematically dominate contracts in terms of energy savings—a finding that aligns closely with our experimental results. Our model estimates also yield the first experimental estimate of the short-run price elasticity of electricity demand in Vietnam (-0.089), placing it within the range of estimates from other countries.

We use information on the 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

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

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

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

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 at the optimal threshold $f'(\ell^* - e^*) = 0$. Using that $f'(\varepsilon) = 0$ if and only if $\varepsilon = z$, we conclude that $\ell^* = e^* + z$. □

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

$$e^* = e^*_{control} - \frac{I}{2\gamma}. \tag{13}$$

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

Proof of Proposition 6

Proof. First, we show that a contract to a single household dominates an N-household contest under three conditions: $N = 2$; $F(z) \leq 0.5$; and for all x, $f(x) \leq f(z)$.

This follows from the following observations:

1. $(N-1)(1-F(\varepsilon))^{N-2} f(\varepsilon) = \frac{d}{d\varepsilon}[-(1-F(\varepsilon))^{N-1}$,
2. $\int \frac{d}{d\varepsilon}[-(1-F(\varepsilon))^{N-1}] d\varepsilon = 1$,

Using these observations and the assumption $f(\varepsilon) \leq f(z)$ for all ε, we obtain the following upper bound:

$$\int (N-1)(1-F(\varepsilon))^{N-2} f^2(\varepsilon) d\varepsilon \leq f(z) \int \frac{d}{d\varepsilon}[-(1-F(\varepsilon))^{N-1}] d\varepsilon = f(z).$$

Therefore,

$$N \int (N-1)(1-F(\varepsilon))^{N-2} f^2(\varepsilon) d\varepsilon \leq N f(z).$$

When $N = 2$, using that $F(0) \leq \frac{1}{2}$, we get

$$N \int (N-1)(1-F(\varepsilon))^{N-2} f^2(\varepsilon) d\varepsilon \leq N f(z) = \frac{f(z)}{F(z)},$$

which corresponds to (12), establishing that a contract given to a single household dominates an 2-household contest.

Second, we show that a N-household contest dominates the single contract when $N \geq 3$ and $\varepsilon \sim \text{Normal}(0, \sigma^2)$. In this case, $f'(z) = 0$ iff $z = 0$.

The key observation is that

$$f(\varepsilon) = f(0) \exp\left(-\frac{\varepsilon^2}{2\sigma^2}\right) \quad \Longrightarrow \quad f^2(\varepsilon) = \frac{f(0)}{\sqrt{2\pi}\,\sigma} \exp\left(-\frac{\varepsilon^2}{\sigma^2}\right).$$

We also make the variance of the distribution explicit by using the notation

$$F(\varepsilon) = \Phi\left(\frac{\varepsilon}{\sigma}\right).$$

Using these observations we can write

$$J = \int (N-1)\left(1-\Phi\left(\frac{\varepsilon}{\sigma}\right)\right)^{N-2} f^2(\varepsilon)d\varepsilon = f(0)\int_{-\infty}^{\infty}(N-1)\left(1-\Phi\left(\frac{\varepsilon}{\sigma}\right)\right)^{N-2}\frac{\exp\left(-\frac{\varepsilon^2}{\sigma^2}\right)}{\sqrt{2\pi}\,\sigma}d\varepsilon.$$

This expression combines the CDF of a Normal$(0,\sigma^2)$ with the PDF of a Normal$(0,(\sigma/2)^2)$. Ideally, we would also like the CDF of a Normal$(0,(\sigma/2)^2)$, that is, the term $\Phi\left(\frac{\varepsilon\sqrt{2}}{\sigma}\right)$ instead of $\Phi\left(\frac{\varepsilon}{\sigma}\right)$. Since $\sqrt{2}>1$ and $\Phi(\cdot)$ is increasing, we have $\Phi\left(\frac{\varepsilon}{\sigma}\right) < \Phi\left(\frac{\varepsilon\sqrt{2}}{\sigma}\right)$. Therefore, we have the following lower bound:

$$J > f(0)\int_{-\infty}^{\infty}(N-1)\left(1-\Phi\left(\frac{\varepsilon}{(\sigma/\sqrt{2})}\right)\right)^{N-2}\frac{\exp\left(-\frac{\varepsilon^2}{2(\sigma/\sqrt{2})^2}\right)}{\sqrt{2\pi}\,(\sigma/\sqrt{2})}\frac{1}{\sqrt{2}}d\varepsilon$$

If we call $\tilde{F}$ the CDF of a Normal$(0,(\sigma/2)^2)$ and $\tilde{f}$ its PDF, the last expression equals

$$\frac{f(0)}{\sqrt{2}}\int_{-\infty}^{\infty}(N-1)(1-\tilde{F}(\varepsilon))^{N-2}\tilde{f}(\varepsilon)d\varepsilon = \frac{f(0)}{\sqrt{2}}.$$

Hence, we obtain the following lower bound:

$$N\int (N-1)(1-F(\varepsilon))^{N-2}f^2(\varepsilon)d\varepsilon > \frac{Nf(0)}{\sqrt{2}}.$$

The contest dominates for $N>2$ because $\frac{3}{\sqrt{2}}>2$. □

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

Proposition 7 (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

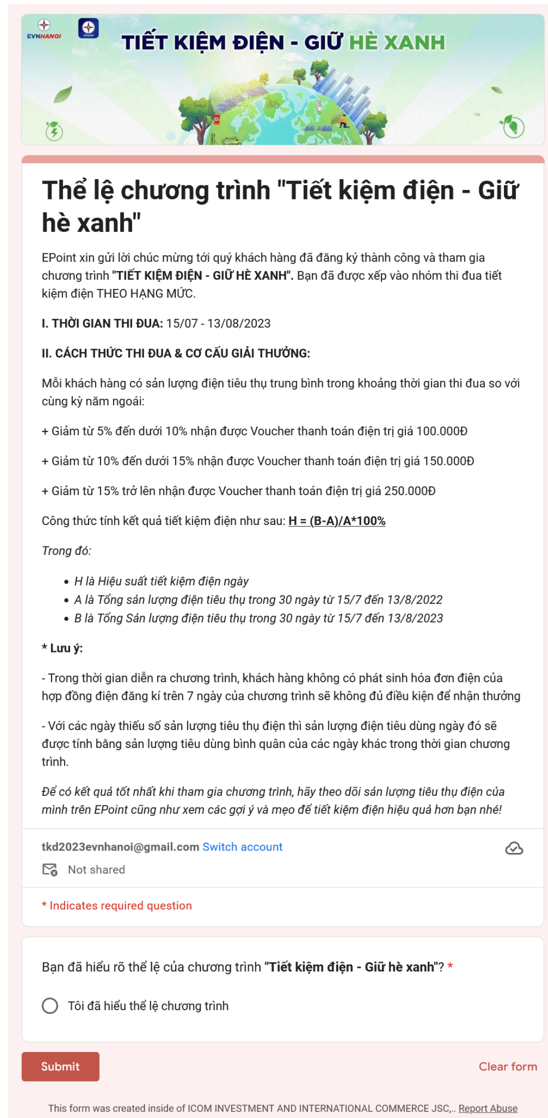


Figure C.2: English Translation of treatment rules

(**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

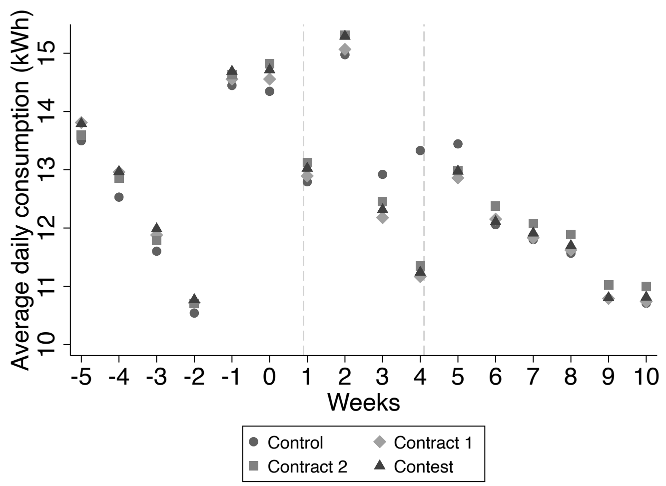
***Note:**

- 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!

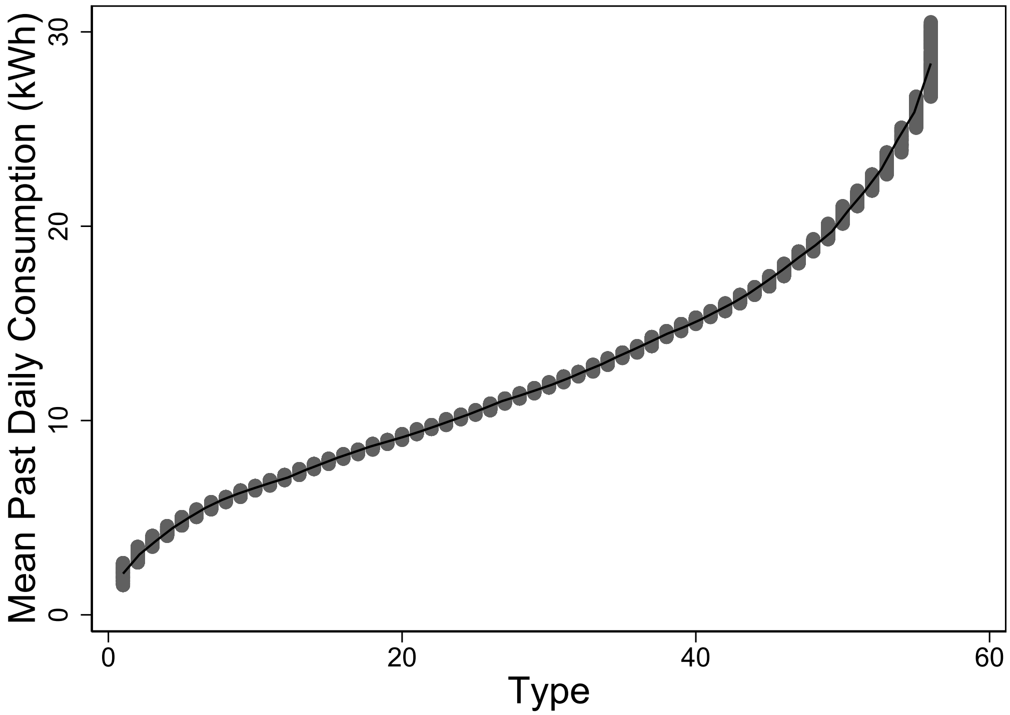
Appendix D: Additional tables and figures

Figure D.1: Mean Comparisons over Time: Within-household variation



Notes: An observation is a household–day combination. The figure plots the average energy use (in kWh) of each treatment group X at the week level. 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.

Figure D.2: 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: Share of households saving at least X percent when facing incentives to save X percent

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample			Households in contests or contracts		
	HH saves at least			HH saves at least		
	5%	10%	15%	5%	10%	15%
Paid to save 5%	0.057			0.028		
	(0.010)			(0.011)		
Paid to save 10%		0.048			0.014	
		(0.008)			(0.010)	
Paid to save 15%			0.039			0.016
			(0.007)			(0.009)
Observations	11178	11178	11178	8388	8388	8388
p-value	0.000	0.000	0.000	0.013	0.162	0.074

Notes: Robust standard errors in parentheses. An observation is a household. Each column reports the output of a regression of an indicator for whether the household saves at least X percent (relative to their consumption in the same period in the year prior) on an indicator for whether the household receives a direct payment for saving X percent. Note that households in the contest or control groups are never offered payments for saving more than a predetermined threshold. Columns 4 through 6 restrict the sample to households being paid to save electricity (i.e., those assigned to a contract or contest). The p-value row reports the p-value for a two-side test of the null that the coefficient on the variable 'Paid to save X%' is zero.

Table D.2: 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).

Table D.3: Estimates of the model parameters: $\{\gamma_\kappa\}_\kappa$

Type	$\hat{\gamma}_\kappa$	St. Error
Type 1-14	0.0046	0.0001
Type 15-28	0.0012	1.6738e-05
Type 29-42	0.0016	4.8683e-05
Type 42-56	0.0012	7.5187e-05

Notes: Standard errors are bootstrapped.