Energy Conservation under Repeated Contracts and Contests: Registered Report

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Abstract

We propose an experimental evaluation of whether performance-based incentives for residential energy conservation remain effective when repeated. Building on our prior study with EVNHANOI, which found that both contests (rewards based on relative performance) and contracts (rewards based on absolute savings) reduced electricity use by 7–9%, with contests proving significantly more cost-effective, we aim to test whether these effects hold when households are repeatedly exposed to these incentives. Does the comparison of contests versus contracts change when households are repeatedly exposed to these? Does repeated exposure reduce energy conservation after households fail to earn a reward? To answer these questions, we will randomly assign 6,400 households in Hanoi to one of six groups. The first four groups vary by incentive type (contest or contract) and reward level (high or low). The fifth group will receive nudges without being offered explicit incentives to conserve energy, and the last group will be a pure control. Each treated household will participate in two consecutive two-week conservation periods. Using smart meter data, we will measure electricity consumption to assess both immediate effects and dynamic behavioral responses over time.

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Table of Contents

1 Introduction

Electricity consumption in rapidly growing urban areas in low- and middle-income countries (LMICs) is placing increasing strain on power systems. This is especially true during peak summer months, when demand spikes due to cooling needs and electricity supply systems must rely on costly and carbon-intensive generation sources. In these settings, electric utilities often confront a dual mandate: deliver reliable service while keeping costs and emissions in check. However, utilities face limited options to meet surging demand in the short run, especially in contexts where adding generation or storage capacity is expensive, politically difficult, or environmentally undesirable. Against this backdrop, demand-side management (DSM)—interventions that encourage households to reduce electricity use—has emerged as a promising alternative (Auffhammer et al., 2008; Borenstein, 2012).

The economics of DSM is conceptually straightforward. By shifting or reducing electricity demand, especially during peak periods, utilities can avoid costly spot market purchases, delay capacity expansion, and reduce carbon and particulate emissions (Callaway et al., 2018; Graff Zivin and Neidell, 2018). However, implementing DSM programs at scale in LMICs presents both logistical and behavioral challenges. One central issue is how to design incentive schemes that are cost-effective, easily communicable, and capable of sustaining behavioral change across diverse households. While tiered pricing, social comparison nudges, and direct subsidies have all been employed (Allcott and Mullainathan, 2010; Allcott, 2011; Allcott and Kessler, 2019), there is growing interest in performance-based financial incentives, particularly in the form of either contracts or contests.

Contracts are individually targeted incentives that reward households if their energy savings exceed pre-specified thresholds. They are straightforward to understand and implement, and are widely used in DSM programs globally (Ito, 2015; Neveu and Sherlock, 2016; Fowlie et al., 2021). Contests, in contrast, offer rewards based on relative performance—households compete against each other for a fixed prize, with only the top performer in a group receiving a reward. The performance of contests has been investigated in various settings by Gross (2020), Bhattacharya (2021), Lemus and Marshall (2021), among others.

The literature has studied the comparison between contests and contracts, and the comparison is ambiguous (Lazear and Rosen, 1981; Green and Stokey, 1983; Garg et al., 2025). However, most of these results are for one-time interventions, and we still do not know how the comparison between these mechanisms changes as the interventions are repeated over time.[1] If contests dominate contracts in a one-time intervention (as documented in Garg

[1] Learning about the performance of repeated interventions has been studied, for example, by Ito et al. (2018) in the context of an energy conservation program involving price changes during high-demand times as well as moral suasion.

et al., 2025) but contest participants get discouraged over time relatively more than contract participants, then the dominance result could reverse in repeated interventions. This has important implications in the DSM context, where sustained engagement over time is necessary to realize aggregate system benefits.

This project builds on a previous randomized control trial (RCT) conducted in partnership with EVNHANOI, the sole electricity provider for Hanoi, Vietnam. In that study, we compared contracts and contests in terms of their ability to incentivize energy conservation among residential customers during the peak summer month of 2023. Using daily smart meter data for over 8,000 households, we found that both contracts and contests reduced electricity use by approximately 7–9% relative to a control group. However, contests were approximately 40% more cost-effective. Structural modeling suggested that contests, under a wide range of assumptions, outperformed even optimized contract designs (Garg et al., 2025).

Encouraged by these findings, EVNHANOI expressed interest in deploying such DSM programs more broadly across its customer base. However, before such programs can be scaled, several important questions must be answered. First, to what extent are the results from the initial experiment replicable when applied to a new sample of households? Second, what happens when these incentives are offered repeatedly? Specifically, do households that do not win or earn a reward in the first round become discouraged and reduce effort in subsequent rounds? Or are such schemes robust to repeated exposure?

This registered report evaluates a new RCT designed to answer these questions. The experiment involves 6,400 households randomly assigned to one of six groups: two control arms (with and without messaging), two contest treatments (with prizes of either $40 or $80), and two contract treatments (offering rewards of either $2 or $4 for achieving a 5% reduction). Each household is exposed to two rounds of treatment—two identical two-week intervention periods, separated by a one-week break. We measure daily electricity use throughout and examine both immediate and lagged responses.

The design allows us to test several hypotheses of interest. First, we assess whether contest-based incentives continue to outperform contracts in terms of cost-effectiveness and energy savings in a new sample. Second, we compare behavioral responses across rounds, unconditionally or conditional on whether a household won or received a reward in the first round. If effort declines more in contests than in contracts among non-recipients, this would suggest that rank-order mechanisms may underperform when used repeatedly. Third, we examine whether the generosity of the incentive (i.e., contest prize size or contract reward amount) modulates both initial participation and subsequent retention or engagement.

Our setting offers several advantages for answering these questions. First, EVNHANOI provides high-frequency smart meter data, enabling precise measurement of household electricity use. Second, because the utility controls customer communication channels and app-based monitoring infrastructure, we can easily randomize information provision, incentive structures, and reminders. Third, EVNHANOI serves as a testbed for other state-owned utilities in Vietnam allowing for a pathway to scale experimental findings.

2 Context of the Experiment

Vietnam's electricity sector is undergoing a transformation driven by rapid economic growth, rising residential demand, and national commitments to reduce emissions. In Hanoi—the capital and second-largest city—these dynamics are especially pronounced. As households enter the middle class, they increasingly adopt energy-intensive appliances such as air conditioners and washing machines, leading to sharp increases in electricity use during the hot summer months (Gertler et al., 2016). This seasonal demand surge strains grid infrastructure and raises the risk of blackouts and service disruptions.

EVNHANOI, the city's state-owned electric utility, relies heavily on hydropower generation. As a result, unlike solar-dominated systems where peak supply and peak demand are misaligned at the daily level, Hanoi's power shortages emerge from a different mismatch. Hydropower generation is highly seasonal and dependent on rainfall. Reservoirs are often depleted by mid-summer, which coincides with the hottest weeks of the year. This makes electricity shortages in Hanoi a *week-of-year* problem—rooted in the cumulative seasonal imbalance between supply and demand—rather than a *time-of-day* problem. Peak reductions during this critical seasonal window are therefore of particular value for grid reliability and emissions mitigation.

To address this challenge, EVNHANOI has begun exploring demand-side management (DSM) strategies. While dynamic pricing and energy efficiency subsidies are still in early stages, the utility has invested in digital infrastructure, including widespread installation of smart meters and a mobile app that allows households to track their daily usage. These tools provide a platform to test behavioral and incentive-based DSM interventions.

In a 2023 field experiment, we partnered with EVNHANOI to evaluate two such interventions: individual contracts (which reward households for reducing consumption relative to their own past use) and contests (which reward the household with the greatest relative savings within a group). That study found that both mechanisms led to significant energy savings—7 to 9 percent during the hottest month of the year—with contests delivering these savings at roughly half the cost of contracts (Garg et al., 2025). The contest design was particularly appealing to utility managers because it fixed total payouts ex ante, independent of the number of households meeting consumption targets.

Encouraged by these findings, EVNHANOI is considering scaling such programs, but open questions remain. The first is whether similar results can be achieved outside of the initial self-selected sample of app users. The second is whether these incentive mechanisms sustain participation and effort over time. In a seasonal grid stress environment, repeated interventions are necessary, yet repeated exposure may lead to *discouragement effects*—especially in contests where many households do not win.

This follow-up study seeks to address these questions through a new randomized controlled trial involving a broader sample of 6,400 households. By implementing two consecutive rounds of incentives, we aim to evaluate not only the replicability of our prior results but also the behavioral persistence of effort under repeated incentives in a hydropower-dependent grid facing seasonal shortages.

3 Research Design

We will collaborate with EVNHANOI to recruit participants for the experiment through a two-week outreach campaign, beginning in the second week of June (or earlier if administrative procedures are completed ahead of schedule). Recruitment will be conducted via text messages sent to eligible households in Hanoi, inviting them to participate in the study. Interested customers will be directed to register through a provided link (e.g., Google Form).

In our recruiting phase, we will target households that meet the following criteria based on their daily consumption data for the year prior to the target start date:

1. Households that have less than 20% of observations with their daily consumption equal to 0 kWh.

2. Households that have less than 10% of observations with missing information about their daily consumption. These data are not truly "missing" but due to IT system glitches are recovered later on. While this is not an issue for data analysis, it precludes including households in randomization.

3. Households whose average daily consumption (during the entire year) is between 1 kWh and 24 kWh.

4. Households whose average daily consumption during the reference period (i.e., the same period as the proposed experiment period but one year prior) is between 1 kWh and 30 kWh.

These criteria are in line with those used in our previous study (Garg et al., 2025), and their goal is to limit the variance in the outcome variable of interest by excluding households that are in the thin but long tail of the distribution of electricity consumption.

Upon registration, participants will be randomly assigned to one of six groups. All the interventions below will receive text message reminders (except where otherwise noted). See below for the reminder schedule and message content. The interventions will be repeated twice, each lasting two weeks.

- Group 1: Contract (Low Reward) — All participants who reduce their electricity consumption by 5 percent relative to the same period in the previous year will receive a reward of $2 (USD).
- Group 2: Contract (High Reward) — All participants who reduce their electricity consumption by 5 percent relative to the same period in the previous year will receive a reward of $4 (USD).
- Group 3: Contest (Low Reward) — Participants will be placed into a 50-household contest, and a single prize of $40 will be awarded to the household that achieves the highest percentage reduction in electricity consumption compared to the same period in the previous year.
- Group 4: Contest (High Reward) — Participants will be placed into a 50-household contest, and a single prize of $80 will be awarded to the household that achieves the highest percentage reduction in electricity consumption compared to the same period in the previous year.
- Group 5: Control Group (with text message reminders). This group will only receive text messages but will not participate in a contest or individual contract.
- Group 6: Control Group (without text message reminders). This group will not receive text messages, and will not participate in a contest or in an individual contract.

Customers who are selected and grouped to participate will receive a message containing a link to the competition program's rules and details. By confirming receipt of the message, customers acknowledge that they have read and understood the rules of the competition. Based on power calculations, previous experiment data, and the project's budget, we expect approximately 6,400 households to register and participate.

The experiment will consist of two phases. Participants are required to take part in both phases and have the opportunity to win prizes in each. The tentative dates for each phase are:

- Phase 1: Runs from July 7 to July 20.
  Results will be announced during the week of July 21–27.

- Phase 2: Runs from July 28 to August 10.
  Results will be announced during the week of August 11–17.

The tentative reminder schedule is outlined below, and it will be the same for both interventions.

- Day 3: A reminder that the contest/contract has started.

- Day 7: A message noting that the first week is complete, with one more week to go.

- Day 10: A reminder that only a few days remain in the contest/contract period.

- Day 14: Notification that the contest/contract has ended. The message will include: "The contest/contract has ended. Winners will be notified soon. (For phase 1 only:) A new contest/contract will begin in one week, on July 28. "

The content of the message in day t will also inform the recipient about their electricity consumption since day 1, relative to the same period in the year prior.

4 Data

4.1 Variables

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 with smart meters installed in every home. We collect daily consumption data for each household for a period of at least twelve months prior to the intervention, throughout the duration of the experiment, and for at least twelve months following the conclusion of the interventions. This extended data collection period allows us to assess both the immediate and long-term effects of the treatments on household energy use.

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

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

4.2 Balance Checks

We will check balance between the treatment and control groups using data on the historical electricity use of households. Specifically, we will use the daily electricity consumption at the household level computed month by month for the 12 months prior to our experiment.

For each of these variables, we will run the following specification:

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

where treatment_i is a variable indicating the treatment assignment of household i. The regression includes indicators for all treatment groups except for the control group (the omitted category). In our balance analysis, we will 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 $H_0 : \beta_1 = \beta_2 = \cdots = \beta_K = 0$) for every variable listed above.

5 Analysis Plan

We will estimate the impact of repeated incentive treatments on household electricity consumption using the following household-day level specification:

$$y_{it} = \alpha + \sum_{r=1}^{2} \sum_{k=1}^{K} \beta_{k,r} \cdot \text{Treat}_{ik} \cdot \text{Round r}_t + \gamma_i + \delta_t + \varepsilon_{it}, \tag{1}$$

where y_{it} is daily electricity consumption (in kWh) for household i on day t, Treat_{ik} is an indicator for assignment to treatment group k (including control, two contest variants, and two contract variants), Round r_t is an indicator that takes the value one during the round r intervention period, γ_i are household fixed effects, and δ_t are day fixed effects that control for time-varying factors such as temperature or calendar effects.[2] Standard errors will be clustered at the household level. We will also estimate a version of Equation 1 where the dependent variable is the natural logarithm of the electricity consumption (in kWh). All of

[2] δ_t also captures experimental round fixed effects.

these econometric models will make use of daily household-level electricity consumption data from before, during, and after the experimental period.

Our parameters of interest are $\beta_{k,r}$, which measure the average impact of intervention k in round r on electricity consumption. The omitted category in this equation is the pure control group.

5.1 Primary Hypotheses

1. **Effectiveness of Incentives (Round 1):** We test whether households in any treatment group reduce electricity use during the first intervention period relative to the control group.

2. **Persistence and Discouragement (Round 2):** We test whether treatment effects persist or attenuate in the second round, with specific attention to differences in performance between those who did and did not win or earn rewards in Round 1.

3. **Relative Performance of Mechanisms:** We compare contests to contracts on both behavioral and cost-effectiveness dimensions, using average energy saved and reward payouts.

As discussed, we will estimate treatment effects separately for Round 1 and Round 2, and compare changes in consumption between rounds within households to test for effort persistence or discouragement effects.

5.2 Heterogeneity Analysis

We will explore heterogeneity in treatment effects along key household characteristics and the outcomes of the first-round interventions, including:

i) Baseline electricity consumption (above vs. below median)

ii) Baseline variance in electricity consumption (above vs. below median)

iii) Electricity consumption in round 1

iv) Discouragement from not winning a reward in round 1

v) Weather

We will include interaction terms between these variables and treatment indicators and examine both Round 1 and Round 2 responses (or only Round 2 responses when relevant, e.g., items iii) and iv) above).

5.3 Inference and Robustness

All regressions will report robust standard errors clustered at the household level. To address potential imbalance in covariates across treatment arms, we will control for baseline electricity use (same month in the previous year) and household-level averages during the 30 days prior to the experiment.

We will also conduct robustness checks using alternative outcome definitions, including log consumption and winsorized consumption at the top 1%. Additional specifications will test sensitivity to outliers and alternative baselines (e.g., consumption relative to the 30-day moving average instead of year-over-year).

Finally, we will conduct cost-effectiveness calculations for each treatment arm, defined as the monetary cost per kWh saved and per metric ton of CO_2 abated, using emission factors provided by EVNHANOI for summer peak generation.

5.4 Potential Mechanisms

Different patterns can emerge from our two-round experiment:

- Persistent Contest Advantage: Contests outperform contracts in both rounds, with the treatment effect remaining approximately constant. This result would reinforce our earlier findings Garg et al. (2025) and suggest that the contest can be effective at scale. In this scenario, moderate declines in performance for contests and contracts may still occur, which could reflect households' learning about their discomfort-savings trade-off.

- Reversal of Contest Advantage: Contests outperform contracts in round 1 but underperform them in round 2. If contract performance stays stable, this reversal could arise from participants updating beliefs about how competitive their rivals are, becoming discouraged by the contest after the first stage.

- Contract dominating in Round 1: Contracts initially outperform contests, contrary to our prior evidence (Garg et al., 2025). Such a pattern would suggest that anticipation of a second round disproportionately weakens incentives under the contest treatment than the contract.

6 Previous Results

This study builds on a randomized controlled trial we conducted in partnership with EVN-HANOI, the city's exclusive electricity utility, during the summer of 2023. The experiment aimed to evaluate the relative cost-effectiveness of two incentive mechanisms for household energy conservation: *contracts*, which offered fixed rewards for meeting absolute consumption reductions, and *contests*, which rewarded the top performer in each group based on relative performance. The intervention was implemented through EVNHANOI's mobile app and leveraged the city's widespread smart meter infrastructure.

We recruited 11,194 households, primarily through the utility's app, and randomized them into one control group and three treatment arms: two variants of contract schemes and a contest. The experiment ran from July 15 to August 13, 2023. Households in the contract groups were offered tiered payments for reducing electricity consumption by 5%, 10%, or 15% (low-threshold contract), or by 10%, 15%, or 20% (high-threshold contract), compared to their usage in the same period the previous year. Contest participants were grouped into cohorts of 50, with the top saver in each group receiving a prize of approximately $87. Control households received no financial incentives but could monitor their consumption via the app, as could all other groups.

We found that both contracts and contests significantly reduced electricity consumption, with average reductions of 7–9% relative to the control group. These effects were consistent across multiple empirical specifications, including both cross-sectional and within-household analyses. Moreover, we found that these reductions were additional—persisting for at least a week after the end of the experiment—before gradually returning to pre-treatment levels.

While the average energy savings were statistically indistinguishable across the treatment groups, the cost per household differed significantly. On average, contest participants received $1.74 in rewards, compared to $3.14–$3.21 in the contract arms. Thus, contests achieved similar conservation outcomes at roughly half the cost.

We also found evidence that households responded to marginal incentives: those offered a financial reward for reaching a given threshold (e.g., 5%) were more likely to achieve that exact reduction than those who were not. This supports the idea that households fine-tune their effort based on expected payoffs.

Using structural estimates from a model of household energy demand with idiosyncratic and common shocks, we compare the performance of cost-equivalent optimal contracts and contests. The model predicts—and the data confirm—that contests can outperform even optimal contracts when contest size is sufficiently large and expected payments are held constant. This performance gap emerges because contests, unlike contracts, are not sensitive to common shocks (e.g., temperature variation) and allow utilities to fix program budgets ex ante.

Finally, we estimate the marginal abatement cost (MAC) of CO_2 under each mechanism. Contests delivered emissions reductions at $59–76 per ton of CO_2, well below the social cost of carbon. From the utility's perspective, the net cost of conservation under contests was often negative when oil was the marginal electricity source—making the case for these programs even absent explicit climate policy.

These findings demonstrate that contests are not only behaviorally effective but also financially attractive for utilities facing summer peak constraints and emission reduction goals. The current study seeks to test the replicability of these results in a new sample and to examine whether repeated exposure to such incentives results in diminished effort, particularly for contest participants who do not win.

7 Power Calculations

Our previous study, Garg et al. (2025), finds energy conservation effects of around 7 percent (and as large as 9 percent). Our power calculations below are more conservative, assuming a 3 percent effect. This means that a smaller sample is needed if the actual effect is similar to our previous finding.

We use the following equation to calculate the appropriate sample size for our study:

$$J = \frac{\left(t_{1-\kappa} + t_{\frac{\alpha}{2}}\right)^2}{P\left(1-P\right)} \frac{\sigma^2}{MDE^2} \left(\rho + \frac{1-\rho}{T}\right)$$

where

- J is the sample size
- κ is the probability of correctly rejecting a false null or the power
- α is the probability of a type I error
- $t_{\frac{\alpha}{2}}$ and $t_{1-\kappa}$ are the critical values of t distributions
- MDE or the mean detectable effect is the smallest effect size where an effect can still be detected if there is one
- P is the proportion of the sample that is treated
- σ^2 is the variance of the treatment effect estimator
- ρ is the intracluster correlation coefficient
- T is the length of the experiment in days

We set κ to 0.80, or 80%, and α to 0.05, or 5%, values typically used for these calculations. The proportion of the sample that is treated is 67%. We use the pilot data to calculate the variance of the outcome variable and the intraclass correlation coefficient $\rho = 0.566$. For sensitivity, we vary σ^2 from 0.1 to 0.25. For the low variance $\sigma^2 = 0.1$, to detect $MDE = 7\%$ (the low end of the estimates in Garg et al. (2025)), the required sample size of each treatment and control group is 421. For the somewhat implausibly high variance case of $\sigma^2 = 0.25$ (given the precision of estimates in our previous experiment), to detect $MDE = 7\%$, the required sample size of each treatment and control group is $1,053$.

8 Proposed Timeline

Table 1 provides a tentative project timeline.

Table 1: Project Timeline

Task	Start date	Completion date
Recruiting	June 2025	July 2025
Randomization	July 2025	July 2025
First-round incentives	July 2025	July 2025
Second-round incentives	August 2025	August 2025

9 Administrative Information

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Institutional Review Board (ethics approval): This project was approved by IRB committees at UCSD (IRB #23047) and UBC (BREB #H22-00785).

Pre-registration: The pre-analysis plan will be registered at the AEA RCT registry before outcome data collection begins.

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