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Theoretical and Empirical Evaluation of a Competitive Energy Rebate Program

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Abstract

Rebates that reward economic agents if they meet a minimum conservation threshold are a popular policy to encourage energy conservation. However, most threshold-based rebates are structured such that they do not encourage reduction beyond the threshold. In this paper, I show theoretically that programs with the additional feature that households compete to win rebates can effectively encourage further conservation among those who can meet the threshold reduction. The theory also identifies factors that determine the effectiveness of the program. I then exploit a unique confidential dataset of monthly residential electricity use with over 45 million observations to estimate the overall effect of a Vietnamese electricity rebate program with this competitive element. Next, I empirically test the model's predictions. I find that the program reduces electricity consumption by 18%, nearly double the threshold level of 10%. Interestingly, the program's effect persists for at least twelve months after it ends, which has important implications for the cost-effectiveness of such interventions.

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Energy conservation has become an urgent issue as global energy demand rises and energy production contributes to growing emissions.[1] Thus, governments worldwide have been keen on designing and adopting policies to reduce energy consumption across all major energy end-use sectors, including the residential sector.[2] Because consumers generally dislike taxes, policymakers often favor rebates or subsidies as incentives for energy and resource conservation efforts. However, subsidy programs have the potential to be costly. Most energy subsidies are structured such that they do not effectively penalize increases in energy use, which can make them less cost-effective (Ito, 2015). Therefore, while conservation subsidies are often popular with their constituencies, policymakers may want to be sure that they are also as cost-effective as possible.

I study the effects of an electricity rebate program in Vietnam that provides households with information about energy savings tips and benefits as well as cash rebates to encourage energy conservation. The program also has the unique attribute that households compete to get rebates. Like most other threshold-based rebate programs, the Vietnamese program requires households to meet a minimum level of reduction in electricity consumption as compared to their baseline consumption of the previous year in order to be eligible for the rebates. However, the program awards the rebates to only a small portion of those eligible households that have the largest percentage reduction in electricity use. In essence, households compete for the rebates by trying to achieve lower electricity consumption relative to other households. I examine the effect that this competitive rebate program has on the amount of energy conservation.

This paper contributes to the existing literature in several ways. First, it contributes to research on energy conservation in the developing world. Many studies have examined policies designed to reduce energy consumption and increase energy efficiency investments in developed countries (Ito, 2015; Levinson, 2016; Houde and Aldy, 2017; Fowlie et al., 2018). However, research on energy conservation policies in developing countries is limited (Davis et al., 2014;

[1] Energy consumption, including electricity, heat, and transport, accounts for nearly three-fourths of the global greenhouse gas emissions. Agriculture, forestry, and land use account for 18.4%. Direct industrial processes and waste contribute the rest. Data on world greenhouse gas emissions are accessed at https://ourworldindata.org/emissions-by-sector.

[2] For the summary of energy efficiency policies across countries, see World Energy Council (2013) and the 2018 International Energy Efficiency Scorecard report at https://www.aceee.org/research-report/i1801. For a comprehensive information on incentives and policies to promote renewable energy and energy efficiency in the United States, access to the Database of State Incentives for Renewables and Efficiency at https://www.dsireusa.org/.

Costa and Gerard, 2021). This paper looks at an energy conservation rebate program in Vietnam, a developing country. Many developing countries are experiencing above-average growth in per capita energy consumption due to increasing population and economic development.[3] Energy conservation is critical for these countries to meet the increasing electricity demand caused by growth while limiting environmental costs from electricity generation. Also, compared to developed countries, developing countries often have lower energy efficiency, which means they may have more opportunities for reductions.[4] Furthermore, developing countries often adopt energy conservation policies like those of the U.S., but adjust them to align with their conditions and constraints. These adjustments might enhance or undermine the success of a program.[5]

Second, the paper adds to the body of research about behavioral interventions to promote resource and energy conservation. Prior studies find that the conservation behavior is heavily impacted by social comparison, information provision, and peer influence (Allcott, 2011; Ferraro and Price, 2013; Allcott and Rogers, 2014; Jessoe and Rapson, 2014; Allcott and Taubinsky, 2015; Brandon et al., 2018). This paper suggests that the Vietnamese competitive rebate program can induce electricity conservation through both the information provision and the competition channels. It also shows theoretically that a fixed-amount rebate with the competition feature can work as a marginal tax on electricity consumption among households that meet the threshold reduction. Interestingly, the tax rate depends upon each household's perceived chance of winning the rebate. I also provide some evidence for the mechanisms that explain how this competitive element works.

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[3] Data on population growth and per capita energy consumption growth are assessed by the World Bank and are available at https://data.worldbank.org/indicator/SP.POP.GROW and http://databank.worldbank.org/data/reports.aspx?source=World-Development-Indicators.

[4] Low energy efficiency in developing countries can be explained by many efficiency barriers such as informational and financial barriers (UNIDO 2011; Aznar et al. 2019). Also, Farrell et al. (2008) finds that developing countries contribute 65% of the negative-cost energy productivity opportunities to reduce greenhouse gas emissions in the globe.

[5] For example, the Vietnamese rebate program is very similar to California's statewide 20/20 electricity rebate program, as they both offer households rebates for electricity reduction compared to their consumption in the previous year. The Californian program provides a 20% electricity bill reduction to *all* households who reduces consumption by at least 20%. However, the Vietnamese program only awards rebates to a top portion of households who meet and exceed the minimum conservation threshold of 10%, primarily due to the limited funding of the program. Such an adjustment creates competition among households to get rebates, which might affect conservation decisions.

Third, the paper contributes to the growing literature showing that the effects of short-term policies can persist even after policies end.[6] The availability of long-range household-level panel data allows me to study the effects of the Vietnamese rebate program months after it ends. I also show that the persistence of the program's effect has important implications for the cost-effectiveness of such interventions.

In the first part of this paper, I construct a theoretical partial-equilibrium model to study households' responses to the presence of the rebate program. I start with a simple conservation rebate scheme that provides rebates to households whose efforts meet some threshold. Then I introduce competition by specifying that households' perceived probability of winning the rebates increases with their electricity conservation (or decreases with their electricity consumption). Households in the model maximize expected utility subject to their budget constraint. For those with some chance of winning, my theoretical findings suggest that adding this competitive element to a rebate program can encourage conservation beyond the threshold and discourage marginal increases in electricity use. If households are too far from the winning threshold, however, they are then discouraged from conserving, just as in the threshold model without competition. The model also provides predictions regarding the determinants of the program's effect on electricity conservation. In hotter weather, households compete less and thus reduce consumption less. By contrast, households reduce more if they face higher electricity prices or have a higher baseline consumption.

In the second part of this paper, I estimate the Vietnamese rebate program's overall effect on electricity consumption and empirically test predictions from the theoretical model. The primary dataset I exploit is a monthly household-level panel of electricity billing records from over 650,000 residential customers for 72 months from January 2012 to December 2017. While the program's enrollment is voluntary, it becomes available in different districts at different times. Only households in the chosen districts are eligible to enroll in the program. Power companies plan to implement the program in all districts but generally choose to introduce it first in districts with high electricity consumption. This paper assumes that, conditional on fixed effects and other controls, the timing of the program's roll-out is plausibly exogenous. I indirectly test this assumption by demonstrating that pre-treatment trends in outcomes evolve

[6] Some examples include Ferraro and Price (2013), Allcott and Rogers (2014), Ito (2015), and Costa and Gerard (2021).

similarly in areas that rolled out the program earlier and those that began the program later or did not have the program.

My empirical strategy uses three approaches. First, I use the standard difference-in-difference estimation framework to estimate the effect of the program treatment assignment to a particular district or the intent-to-treat effect, given the plausibly exogenous staggered adoption of the program. Second, to correct for endogenous household enrollment once eligible, I instrument for the actual enrollment status by using enrollment eligibility status and estimate the program's localized average treatment effect. Third, I exploit quasi-random variation in the timing of the program's roll-out and employ a difference-in-difference event-study empirical design, which also allows me to assess whether the treatment effect is persistent over time.

Empirical results suggest that the program on average reduces electricity consumption by 18%, and the effect of the program persists for at least 12 months after the program ends. My empirical work, however, cannot distinguish how much of the energy conservation is due to information provision and how much is due to the program's competition element. Also, the Vietnam data used in this study covers only the competitive program, so it cannot identify the effect of the competition element itself, relative to the a hypothetical program without competition but with the exact same total cost.

To examine the heterogeneity of the program's effect, I estimate the interaction terms between the treatment variable and a set of key factors suggested by the theoretical model. Empirical results find that a one percent increase in the air temperature results in a rise of 0.3% in electricity use among households that enrolled in the program compared to other households, and a one percent increase in the price of electricity leads to an additional 2.5% to 3.2% reduction in electricity consumption.

I also evaluate the Vietnamese rebate program's costs and benefits using estimates from the empirical model. Without consideration of the persistence of the program's effect, the program's calculated costs per unit of electricity saved and emissions abated are \$19 per MWh and \$90 per ton of CO_2 emissions, which are much smaller than those found in the literature in both developed and developing country contexts (Davis et al., 2014; Ito, 2015). After accounting for the persistent effect, the program's calculated costs are greatly reduced to \$5–6 per MWh or \$24–27 per ton of CO_2 emissions, which are below the most reliable estimates of the social cost of carbon emissions (Nordhaus, 2017; Revesz et al., 2017).

I organize the paper as follows: Section 1 provides background on the conservation rebate program in Vietnam. Section 2 describes the theoretical model and presents theoretical findings. Section 3 describes the data and descriptive statistics. Sections 4 and 5 present the empirical specifications and discuss empirical results regarding the overall effect of the Vietnamese rebate program on electricity consumption and the heterogeneity of the effect. Section 6 evaluates the persistence of the program's effect. Section 7 discusses the cost-effectiveness of the program. Section 8 presents my conclusions.

1. Background

The Vietnamese electricity rebate program was first implemented in 2010 by Vietnam Electricity (EVN), the nation's state-owned and sole electricity supplier.[7] It is still an ongoing program that awards fixed cash rebates to households with the largest percentage reduction in electricity consumption (compared to their use in the previous year).[8] EVN controls five regional power distribution corporations that have affiliated provincial power companies to distribute and sell electricity in the provinces.[9] Each provincial power company is responsible for the program's implementation according to its budget and needs. Although the program's basic design features are similar throughout the country, several features such as the roll-out period and the cash rebate structure vary by provincial power companies and their distribution zones.

Each year, provincial power companies announce their program period, which usually covers three to six months of the summer. They also set their cash rebate structure with a fixed number of total cash prizes and each prize's cash value. The fixed cash prizes usually have two tiers: the first-tiered prize has a higher cash value than the second-tiered prize.[10] Provincial power companies decide eligible distribution zones or districts within their local territory in each program's year, and only households that live in the chosen districts are eligible to enroll in the program. Power companies do not roll out the program universally at one time but introduce it in one or a few districts within their service territory at a time. They plan to implement the program

[7] The program has its official name translated into English as "Family Energy Savings Program".

[8] To date, the program has become available to about 5 million residential households. It aims to ultimately cover the whole country of more than 21 million residential households.

[9] Five regional power corporations include Northern Power Corporation (NPC), Central Power Corporation (CPC), Southern Power Corporation (SPC), Hanoi Power Corporation (HPC), and Ho Chi Minh Power Corporation (HCMPC).

[10] Some power companies offer prizes of the same fixed cash amount.

in all districts eventually but do not have the resources to do so at the same time. They generally choose districts with high electricity consumption to implement the program first. Later my empirical strategy is based on this differential timing of the program's roll-out.

To be eligible for the cash rebate, a household must enroll and then reduce its monthly electricity consumption by at least 10% for a specified number of consecutive months of the program, relative to the same months in the previous year. Power companies then rank households according to their total percentage reduction in electricity consumption during the program period. Given the fixed number of cash prizes, only a small portion of those eligible households with the largest percentage of electricity reductions get rewarded.[11] In my sample, about 20% of households that enrolled achieved the 10% reduction and became eligible, and about 2.2% of households that enrolled actually won a prize. Fixed cash prizes vary by district and year from VND 200,000 to VND 1,000,000 (or 20% to 90% of the average monthly electricity bill in my whole sample). Besides receiving the cash rebates, winning households are also invited to an award ceremony to recognize their conservation efforts. The award ceremony adds some value to the prize of winning the competition, but its dollar value cannot be quantified.

Power companies have actively promoted the program through a variety of sources. They advertise the program on the mass media, distribute information leaflets, and provide guidelines on saving electricity. They conduct training courses on electricity saving to volunteer advocates and coordinate with local authorities to advertise the program at community meetings. They also broadcast video clips with popular contents about the program and energy-saving tips in public areas and densely populated areas (such as reception areas of state administrative agencies, bus stations, and railway stations). The program has attracted praise from both national and local media for its effectiveness in changing perceptions and actions toward energy conservation. However, no study has attempted to estimate the program's causal effects, potential long-term effects, and cost-effectiveness.

2. Theoretical Framework

I begin by laying out a basic partial-equilibrium framework to study household behaviors in the presence of a competitive rebate program. I use that framework to analyze a fixed rebate

[11] If none of the enrolled households achieves the eligibility threshold, then no rebate is awarded.

scheme and show how competition and other key factors determine household energy use decisions.

2.1. Partial Equilibrium Setup

Household i gets utility from a numeraire consumption good y_i and from electricity consumption e_i (with unit price p). The household has a quasilinear utility function $u_i(e_i, y_i) = v_i(e_i) + y_i$, where v_i is an increasing and strictly concave function.[12] The household faces a budget constraint, $I_i = e_i p + y_i$, where I_i is the full income of household i.

I consider a conservation rebate program that awards rebates to households with the largest percentage reductions in electricity use compared to their "baseline" consumption $e_{i,b}$ (e.g., electricity use in the previous year). For a household to be eligible for a rebate, the program also requires that its electricity reduction meets a minimum percentage reduction threshold, $a > 0$. Equivalently, eligibility for a rebate requires that energy consumption is less than $(1 - a)e_{i,b}$. In addition, households compete for the rebates since only a limited number of eligible households get the rebates. To model this competitive rebate scheme, I assume household i forms some belief about the cumulative distribution function F_{-i} of potential energy use reductions by other households.[13]

The Vietnamese rebate program offers two-tiered prizes to households. I denote N as the number of participating households, n_1 as the number of first prizes, and n_2 as the number of second prizes. Household i wins the first prize if its electricity consumption reduction meets the threshold a and exceeds electricity consumption reductions of at least $(N - n_1)$ participating households. Eligible household i wins the second prize if its electricity consumption reduction is less than at least n_1 first-prize winners and also more than at least $(N - n_1 - n_2)$ participants.

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[12] Quasilinear utility function is a standard use in the literature (e.g., Allcott and Taubinsky (2015), Ito (2015), and Costa and Gerard (2021)).

[13] In the case of the Vietnamese rebate program, households electricity consumption is private information. Also, power companies do not publish any list of winning households or their achieved electricity consumption reductions. The lack of information faced by Vietnamese households and the large number of participants make the above assumption seem plausible because to compete, households need to make some best guess of the distribution function of electricity savings by other competing households. Although households do not have perfect information about the winning threshold, they have some information about the program in their district as well as prior programs in other districts. For example, after the program ends, power companies often make public the final total enrollment number and the average percentage electricity consumption reduction during the program's months, which is generally under 5% and always less than the eligibility threshold of 10%. Those pieces of information might affect the belief of household i about the distribution function of electricity consumption savings by others.

I formally model this competition by specifying that household i's perceived probability of winning a rebate depends on its electricity consumption reduction, its "best guess" distribution of electricity reduction by other households F_{-i}, the announced threshold a, and some exogenous factor X. If household i fails to meet the threshold a, or its electricity consumption reduction denoted by $\Delta_i^e \equiv 1 - e_i/e_{i,b} < a$, the chance of winning the first prize $\pi_{i,1}(\Delta_i^e, F_{-i}, a;\ X)$ and the chance of winning the second prize $\pi_{i,2}(\Delta_i^e, F_{-i}, a;\ X)$ are none: $\pi_{i,1} = \pi_{i,2} = 0$. Here, air temperature is an example of an exogenous factor X. Hotter weather makes it harder for household i to achieve the eligibility threshold and reduces its chance to win the rebate regardless of other households' consumption (i.e., F_{-i}).

If household i satisfies the threshold reduction or $\Delta_i^e \geq a$, the probability of household i winning the first prize is:

$$\pi_{i,1}(\Delta_i^e, F_{-i}, a;\ X) = \sum_{k=1}^{n_1} \binom{N-1}{k-1} \left(1 - F_{-i}(\Delta_i^e)\right)^{k-1} \left(F_{-i}(\Delta_i^e)\right)^{N-k}$$

and the probability of household i winning the second prize is:

$$\pi_{i,2}(\Delta_i^e, F_{-i}, a;\ X) = \sum_{k=n_1+1}^{n_1+n_2} \binom{N-1}{k-1} \left(1 - F_{-i}(\Delta_i^e)\right)^{k-1} \left(F_{-i}(\Delta_i^e)\right)^{N-k}$$

Both $\pi_{i,1}$ and $\pi_{i,2}$ are non-negative functions and are increasing in the first argument. Thus they are decreasing in e_i to capture the idea that the chance of winning a rebate decreases with electricity consumption (or, equivalently, increases with electricity conservation). Also, the chance of winning a rebate increases with household i's baseline consumption.[14] To simplify the notation, I drop subscript "i" in the rest of the paper.

The static model abstracts from the possibility that households can learn over time and adjust their behaviors. For example, after enrollment, households might find out that it is difficult to reach the threshold reduction and might be discouraged to continue competing. One way to

[14] The fact that a household's chance of winning depends on its previous consumption brings up an interesting dynamic question: whether conservation effort this year will make it more difficult to conserve further to win the cash rebate next year. My static model cannot consider this dynamic feature, but it is still an interesting empirical question. In my sample, however, the program was implemented in each district in only one year.

account for this behavior is to update their perceived probability of winning after each period t. However, the inclusion of learning over time does not alter the static model's main results: the static winning probability function fully captures the first month's effect after the enrollment (e.g., before learning might happen).

The Vietnamese rebate program also provides households with helpful information about energy conservation practices, tips, and benefits. As shown in the prior literature, information provision can affect behavior (Ferraro and Price, 2013; Allcott and Taubinsky, 2015; Brandon et al., 2018). I consider such an informational effect by assuming that each household is nudged about the benefits of energy savings and thus values electricity reduction more. This additional psychic cost of energy use can be included in the valuation of $v(e)$ in the household's utility function.[15]

2.2. Fixed Cash Rebates

This subsection considers rebates in the form of fixed cash prizes per winning household. The first and second prizes are Z_1 and Z_2 respectively. The expected value of cash prizes is $Z_1\pi_1(\Delta^e, F, a; X) + Z_2\pi_2(\Delta^e, F, a; X)$, the sum of the value of each prize times the probability of winning it. To simplify notations and without the loss of generality, let denote Z as the "average" fixed cash prize and $\pi(\Delta^e, F, a; X)$ as the "average" probability of winning Z such that $Z\pi = Z_1\pi_1 + Z_2\pi_2$, so $\partial\pi/\partial e < 0$. A household maximizes its expected utility subject to its budget constraint:[16]

$$\max_{e} \ [v(e) + (I - ep) + Z\pi(\Delta^e, F, a; X)]$$

With the absence of the program, $Z\pi(\Delta^e, F, a; X) = 0$, and the first-order condition is $v_e(e^0) = p$. Here, e^0 denotes the household's utility-maximizing electricity use with *no* conservation rebate program, and $v_e \equiv \partial v/\partial e$ is the marginal utility of electricity use. If the

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[15] Included in $v(e)$ can also be the possibility that the household enjoys the act of conservation and of playing a "game" by competing for the prize. Any utility from competition is an implicit cost of energy use.

[16] Here I do not consider that the rebate program might affect a household's discrete choice of whether or not to invest in more energy-efficient durable goods like new refrigerators or air-conditioners. Such an investment is a capital cost necessary to reduce electricity, but its benefits can be realized later and for a long period of time. Unfortunately, data on household ownership of electricity appliances are not available, and the focus of this paper is on short run examination of key determinants of the program's effect with available data.

household chooses to enroll in the program, then $Z\pi(\Delta^e, F, a; X) \geq 0$, and it chooses electricity consumption e^* to maximize its utility.

2.2.1. The Enrollment Decision

Households will choose to enroll in the program if their expected utility from enrolling and reducing consumption to meet at least the threshold reduction $(1-a)e_b$ is more than their utility without the rebate program:

$$v\big((1-a)e_b\big) - (1-a)e_b p + Z\pi(a, F, a; X)) \geq v(e^0) - e^0 p$$

Rearranging the above inequality yields the following condition:

$$\frac{v(e^0) - v\big((1-a)e_b\big)}{(e^0 - (1-a)e_b)} \leq \frac{Z\pi(a, F, a; X)}{(e^0 - (1-a)e_b)} + p \qquad (1)$$

The left-hand side of inequality (1) represents the added utility from consuming each additional electricity unit above the threshold.[17] On the right-hand side, the first term measures the forgone expected prize value for each unit of consumption that exceeds the eligibility threshold, and the second term is the price for each unit of electricity consumption. If inequality (1) holds, households choose to consume no more than the threshold, since the benefit of additional consumption is less than its cost. We can think of inequality (1) as a condition for marginal households to decide whether to enroll in the program. Intuitively, if inequality (1) holds, households have some "cash prize" incentives and are more likely to enroll in the program. Whether inequality (1) holds then depends on the parameters within it, including the magnitude of Z, the probability π of winning the cash prize, the informational effect (e.g., function $v(e)$), and the difference between the optimal electricity use with no conservation incentives (e^0) and the eligibility-threshold quantity $(1-a)e_b$.

Inequality (1) is less likely to hold when the size of the rebate prize Z is small.[18] Obviously, in that case, the rebate will not provide enough incentive for households to change

[17] Quasilinear utility function implies that the utility is measured in the unit of the numeraire good x. If one unit of x worths \$1, then the unit of the added utility or the numerator $v(e^0) - v\big((1-a)e_b\big)$ is in dollar terms. The denominator $e^0 - (1-a)e_b$ has the unit of electricity consumption, for example, kWh. Thus the whole term on the left-hand side of (1) is in \$/kWh.

[18] Note that wealthier households might value a dollar of the cash rebate Z less than poorer households might do. The quasilinear utility model does not capture such an idiosyncratic valuation of the rebate, assuming households

their behavior or to reduce their electricity use. Inequality (1) is also less likely to hold when their baseline consumption is low, perhaps because of some event in the baseline year, such as being out of town the previous summer. Even if the cash prize is large, a very low baseline use reduces the chance of winning a rebate since those households have to conserve a significant amount of electricity to win (that is, $e^0 - (1-a)e_b$ is large). Thus, households are discouraged from conserving if they are too far from the threshold for rebate eligibility. This finding suggests the "asymmetric" incentive problem of rebates that subsidize conservation but do not effectively penalize consumption as found in Ito (2015).

Figure 1 graphically illustrates the case when inequality (1) does not hold, such that the program does not provide households enough incentives to meet the threshold reduction. When a household does not meet the threshold, then it cannot win ($\pi = 0$), just as with no rebate program. Then, the original budget line (black dotted line) is tangent to the indifference curve at e^0, the utility-maximizing consumption with no rebates, and the chosen point is at $e^* = e^0$ in Figure 1. Suppose this household were to reduce electricity use to become eligible and try to win a rebate. In that case, the budget line moves up by the amount of the rebate's expected value (i.e., the size of the rebate times the probability of winning it). A positive likelihood of winning the rebate creates a kink in the household's budget constraint, because in expectation the household receives a marginal subsidy for each unit of reduction relative to the baseline.[19] The probability function π determines the shape of the new portion of the budget line when electricity use is below the eligibility threshold. Figure 1 provides an example of function π that changes linearly with e. If the new budget line lies below the indifference curve that is tangent at e^0, as in Figure 1, then the household finds no better affordable bundle than e^0. Figure 1 shows that the chosen consumption with the program, e^*, is the same as without the program, e^0, and they are both larger than the threshold: $e^* = e^0 > (1-a)e_b$.

value a \$1 rebate exactly as \$1 to spend on either x or e. However, the dollar face-value of the rebate positively correlates with households' valuation of it.

[19] A marginal subsidy to electricity reduction is equivalent to a marginal tax on electricity consumption. The tax increases the price of electricity, and as a result, the budget line gets flatter.

2.2.2. The Impact of Enrollment on Electricity Consumption

Here I consider the case where the household decides to enroll and can meet the threshold reduction: $e^* \leq (1-a)e_b$ (that is, inequality (1) holds). Then the first-order condition from the household's maximization problem yields:

$$v_e(e^*) = p - Z\pi_e(\Delta^{e^*}, F, a; X) \tag{2}$$

On the right-hand side of (2), the price of electricity is augmented by a positive amount, $-Z\pi_e(\Delta^{e^*}, F, a; X) > 0$, because the probability of winning the rebate decreases with marginal electricity consumption (i.e., $\pi_e(\Delta^{e^*}, F, a; X) \equiv \partial\pi(\Delta^{e^*}, F, a; X)/\partial e < 0$). This positive term measures the fall in the expected value of rebate and thus captures the *competition effect*. The cash rebate goes into the budget constraint, so an increase in electricity consumption reduces the household's winning probability and changes the budget line's slope. Unlike the competition effect, the *informational effect* causes the household to value electricity consumption less, but this "psychic cost" is not in its budget constraint (in dollars) and does not affect the slope of the budget line. The informational effect, however, reduces the valuation of $v(e)$ and thus can discourage energy consumption. Both the informational and competition effects lead to electricity conservation. The competitional effect depends on the size of the cash prize, baseline consumption, exogenous factor X, and winning probability.[20]

While the cash rebate is a fixed amount, interestingly, equation (2) shows that in expectation, the effect of the uncertain cash rebate is equivalent to the effect of a marginal subsidy to electricity conservation (or a marginal tax on electricity consumption).[21] Thus, for all those who attain the threshold reduction, the program can provide "symmetric" incentives to subsidize consumption reductions and penalize increases in consumption. That is, the chance of winning a rebate not only increases with *marginal* electricity conservation but also decreases

[20] The informational effect depends on a different set of factors, such as the household's feeling about how its electricity consumption might affect the environment and other people, but data on such a factor is not available.

[21] While a flat rate tax on all energy creates the same marginal incentive to reduce energy use and to avoid increase in energy use among all households, the marginal effective "tax" imposed by the competitive rebate program on electricity consumption varies across households. Precisely, the tax rate depends on a household's perceived chance of winning a rebate. That perceived probability, in turn, depends on electricity consumption e, so the size of the subsidy is not constant with e. With the competition feature, the slope of the budget contrait changes with electricity consumption. For example, a household that is barely eligible knows it is not likely to win, and so its marginal behavior does not matter as much as for a different household that thinks it is close to the final cut-off between winners and losers.

with any *marginal* increase in electricity use.[22] Equation (2) also suggests that the competitive rebate program can induce reduction beyond the threshold a for households that meet the threshold reduction in their electricity use.[23]

Figures 2A and 2B illustrate two scenarios where the rebate program can induce a household to reduce electricity consumption more than the required threshold. Figure 2A shows the case where the household, with the absence of the rebate program, would choose to consume more than the threshold: $e^0 > (1-a)e_b$. The heavy black line in Figure 2A is the new budget line with the competitive rebate program. That new budget line is tangent to a higher indifference curve at e^*, a better consumption bundle. As shown in Figure 2A, the chosen electricity consumption with the rebate program, e^*, is less than the consumption without the rebate program, e^0, and it is less than the threshold $(1-a)e_b$. The total effect of the rebate program on conservation by this household is measured by the consumption reduction from e^0 to e^*.

Figure 2B tells a different and interesting story where the rebate program can provide incentives for energy reduction by households whose consumption would be below the threshold without rebate enticements. As shown in Figure 2B, a household may have a very high baseline consumption, perhaps because of some event in the baseline year (such as a very hot previous summer, or having house guests). Thus, in the program's year, the household's optimal electricity consumption with no rebate program already satisfies eligibility-threshold quantity $e^0 < (1-a)e_b$. The heavy black line in Figure 2B is the new budget line with the rebate program, and it also is tangent to a higher indifference curve at e^*. As shown in Figure 2B, induced by the rebate program, households choose electricity consumption e^* that is less than e^0 and less than $(1-a)e_b$.

Conditions (1) and (2) are key findings from the theoretical model. Inequality (1) implies that the competitive-rebate program does not provide symmetric incentives to everybody, as

[22] Any household that knows it will not reduce energy use by at least the eligibility threshold then faces no incentive to reduce at all and no penalty on increases in energy use. Thus, when this paper claims that competitive rebates retain more symmetric incentives to reduce energy and not to increase energy, the claim pertains only to those who intend to reduce by at least the eligibility threshold. Interestingly, an implication is that a competitive-rebate program might have more symmetric incentives for even more households if it were to have a smaller eligibility threshold a, or even to have no eligibility threshold. I discuss this implication further below.

[23] In Appendix A, I consider the case of a rebate scheme with the competition feature but also with varying rebates (instead of fixed cash rebates) that depend on the level of electricity consumption. The results still show that the competition feature can induce reduction beyond the threshold for households that meet the minimum reduction requirement.

would an energy tax that encourages all reductions *and* discourages all increased energy use. It still provides no incentives to those who know they will not attain the threshold a (since they will not be eligible for the rebate). This competitive-rebate program can also be compared to a fixed-rebate program, but comparison around the threshold a is not relevant. Instead, the relevant comparison is between two programs of the same cost, a basis on which a competitive-rebate program can provide symmetric incentives that both subsidize consumption reductions and penalize increases in consumption to many more households than a fixed-threshold rebate program.[24]

Under the competitive program, many households may achieve the eligibility threshold a, but a limited program budget means that only a much smaller number n of households can receive a rebate or fixed cash prize Z by reducing electricity use by substantially more than a. Then the cost of this program is $Z \times n$. Program analysts do not know exactly which households are going to win, but suppose they have enough information about the distribution of household characteristics and preferences to be able to calculate what non-competitive fixed-and-announced threshold b would have the same costs (with n households that win a fixed cash prize Z). This b must be a much larger reduction than a. For all households that cannot reduce energy use by at least b, the fixed-threshold program provides no incentive to reduce energy use (or to avoid marginal increases in energy use). In contrast, the competitive-rebate program offers marginal incentives for all other eligible households intended to reduce by at least a, even if they do not ultimately reach the unknown greater threshold reduction to win.[25] More importantly, for the competitive-rebate program in which households do not know the winning threshold, those that reduce use by at least a do have incentives to reduce further.

The threshold a proves to be a critical parameter that determines the number of eligible households, where a lower threshold implies that more households are eligible and thus symmetry for more households. A question, then, is why to have that threshold at all. The

[24] Unfortunately, this theoretical point cannot be tested empirically in this paper, because during the sample period Vietnam had no fixed-threshold rebate program (for data that can be compared to the outcomes of the competitive rebate program). Recently, in June 2020, several Vietnamese power companies have launched a fixed-threshold rebate program similar the the one in California, offering all eligible households rebates if they meet the threshold reduction. This new program will provide an excellent natural experiment that allows me to directly compare the outcomes of a competitive rebate program and those of a non-competitive fixed-threshold rebate program in the future.

[25] The threshold to win a rebate in the competitive program is not necessarily the same as the announced fixed threshold to win the fixed-rebate with the equivalent total cost (b).

eligibility threshold can convey information about a household's true winning probability and thus affect the household's perceived chance of winning. A large threshold a can discourage household participation in the program. A very low eligibility threshold might provide households with an underestimated value of the true winning threshold, and thus they do not try as hard as they would if the threshold was larger. Also, a reasonably large threshold might save administrative costs for those who are not close to winning. How the magnitude of a impacts the effectiveness of the rebate program on conservation is an important and interesting empirical question. I attempt to address this question later in the empirical section, though data limitations somewhat limit this exercise.

2.3. Empirical Predictions

The theoretical model suggests that both information provision and competition for rebates can encourage more conservation. The model also identifies important parameters that determine the program's effects on electricity conservation, as shown in inequality (1) and equation (2). Several of these parameters can be readily observed, such as baseline consumption e_b, the size of the prize Z, threshold reduction a, electricity price p, and air temperature (an example of exogenous factor X). It is more difficult to observe or to measure other key factors, such as the function for a household's perceived probability of winning cash rebate π and the utility function from consuming electricity v.

In the empirical analysis, I quantify the program's effect on electricity consumption and test whether it depends on those key factors, with available data, in the directions predicted by the theoretical model. Presented below are the model's key predictions regarding to several key factors. Note that these factors are all determinants of the competition effect channel but not the informational effect channel. Appendix B presents mathematical proofs corresponding to each prediction.

The first prediction is that if the exogenous factor X increases the rebate-winning probability function π (i.e., $\partial\pi/\partial X>0$), it fortifies the effect of the program on electricity conservation. If factor X decreases π (i.e., $\partial\pi/\partial X<0$), it weakens the effect of the program on electricity conservation. Households are unlikely to enroll in the program or try to compete if their chance of winning the rebates is low or none. Thus, any factor that decreases households' chance of getting the rebates might discourage them from enrolling or trying to win.

Second, the effect of the program on encouraging electricity conservation increases with baseline consumption e_b. Since electricity is a necessary good, households may find it difficult to reduce electricity consumption if their baseline consumption is already low or minimal. By contrast, households with very high baseline consumptions might have more room for reduction to meet the eligibility threshold consumption; thus, they may have a better chance of receiving the rebates.[26]

Third, the effect of the program on electricity conservation increases with the size of cash prize Z and the price of electricity p. A larger cash rebate provides a greater incentive for each household to enroll in the program and to adjust its consumption to win the rebate. Electricity prices can affect everyone with or without the competitive rebate program. However, a higher electricity price implies that households are more likely to enroll in the program since conservation helps each household reduce its electricity bill and probably increase its perceived chance of winning a rebate.

3. Data

This study's primary dataset is panel data of household-level monthly electricity billing records from three provincial power companies in northern Vietnam. These companies provide billing records for all residential customers in their 27 district service territories from January 2012 through December 2017. The household billing data include monthly electricity consumption, as well as households' account numbers and addresses. The data also have the dates that accounts are opened and closed. The power companies also provide lists of households that enrolled and households that received rebates in each year of the program.

Figure 3 compares electricity consumption among households that were unassigned, assigned but not enrolled, and enrolled, where (1) the unassigned were not eligible to enroll because their districts did not have the program in the sample period, (2) the assigned but not enrolled were eligible but chose not to enroll, and (3) the enrolled were eligible and chose to

[26] Of course, households have different average or minimum consumptions due to households' particular characteristics, such as the size of households and occupations of household members. For example, all else being equal, households with a larger size generally consume more electricity at the minimum, and they are likely to have a higher baseline consumption. Thus a larger size household with a higher baseline consumption does not always imply that the household can have more room for electricity reduction than a household with a lower baseline consumption but also with a smaller household size. In the empirical estimation, I control for those household characteristics with household fixed effects.

enroll in the program. In Figure 3, households that had not yet been assigned treatment (light dashed line) generally consumed less electricity than households that had been assigned treatment (solid and heavy dashed lines). This finding is consistent with what the utilities report—they generally decide to roll out the first programs in districts with high electricity consumption. Among households assigned to treatment, those who chose to enroll in the program have lower average monthly electricity consumption for all 72 months in the sample. The pattern is also clear that consumption increases over time and peaks during the summer months from July to September. Electric utilities generally set the program duration during those summer months.

I also acquire district-level monthly administrative data from power companies on the program's costs, electricity production costs, total electricity sales, total revenue, and the total number of customers. Though the program is roughly similar across districts, some variations can be used to test several of the empirical hypotheses. In almost all districts in my sample, the minimum required reduction is 10%, but one district's reduction requirement is 15%. Most of the districts in my sample also require households to meet the eligibility-threshold quantity for three consecutive summer months, while one-fifth of districts require five and six consecutive months. The value of cash prizes also vary across districts.

Weather is an important factor that can affect the household's electricity consumption and its chance of winning a rebate. Therefore, I collecte data on monthly mean air temperature, rainfall, sunshine, and humidity at the district level from the General Statistics Office (GSO) of Vietnam.[27]

Also, I collecte monthly electricity price data from the Electricity Regulatory Authority of Vietnam. Electricity prices are regulated and follow increasing block-rate tariffs. Residential electricity block-rate tariff schedules are identical for almost all residential customers in the country.[28] Before December 2011, price adjustments took place once a year. In April 2011, a

[27] The GSO compiles their monthly data using the daily weather dataset from the Vietnam Center of Hydro-Meteorological Data. The data set includes daily average air temperature, humidity, sunshine, and rainfall, as recorded at over 170 weather stations in Vietnam. Each province has at least one weather station, but a district can have at most one weather station. Many districts do not have any weather monitor. For districts that have weather stations, mean air temperatures or humidity are measured by their weather stations. For any district that does not have a weather station, I use the average from the weather stations closest to and surrounding it

[28] Before June 2014, low income households with monthly electricity consumption of less than 50 kWh had a lower electricity rate compared all other households. After June 2014, all households face the same rates, but low income households receive a subsidy equivalent to the electricity bill of 30 kWh.

new regulation introduced market-based adjustment of retail electricity tariffs; it effectively allows a maximum of two price adjustments per year (once every six months).

The data supplied by the power companies do not include demographic or income information for each household. Unfortunately, such information is not available for each household, although averages are available at the provincial level. The GSO of Vietnam compiled and supplied the annual provincial dataset of rich information regarding household demographic characteristics, employment, and education. I use these variables as controls in regressions. Table 1 summarizes the descriptive statistics of relevant variables used in the following empirical analysis.

4. Overall Effects of the Vietnamese Rebate Program on Electricity Consumption

In this section, I estimate the overall effects of the Vietnamese rebate program on electricity consumption. Since the program enrollment is voluntary, households that choose to enroll might benefit more from energy savings than those that do not. Ignoring this possible *ex-ante* difference between the enrolled and not-enrolled would likely result in biased estimates of the program's effects. Thus, my identification strategy is based on the program's staggered adoption by locations, supporting the difference-in-difference empirical design and allowing differences in consumption levels between the enrolled and not-enrolled. Also, as households are not forced to enroll in the program but instead given a choice to opt in, I show estimates of both the intent-to-treat (ITT) and the average treatment effect on the treated (ATT).

4.1. Intent-to-treat

The plausibly exogenous roll-out of the program enables me to provide a valid estimate of the effect of the program treatment assignment to a particular district or the ITT effect (also known as the reduced form effect), using the following standard difference-in-difference estimation framework:

$$\log(e_{it}) = \phi TreatD_i \times Post_t + \gamma_i + \delta_{my} + \epsilon_{it} \tag{3}$$

where $\log(e_{it})$ is logged electricity consumption of household i at time t. The treatment assignment indicator, $TreatD_i$, equals one for households that locate in the district that ever rolled out the program. The binary variable $Post_t$ indicates whether the program has rolled out in household i's district by time period t. Thus, the interaction term $TreatD_i \times Post_t$ is a post-treatment assignment indicator that equals one if household i locates in a district that rolled out the program by time t (i.e., household i was assigned to treatment by time t). I control for household and month-by-year fixed effects, γ_i and δ_{my} respectively. The standard errors are clustered at the district level.

The difference-in-difference approach relies on assumptions of parallel trends and no anticipatory behavior. Parallel trends mean that "assigned" districts with the program and "unassigned" districts that do not have the program would have experienced the same electricity use changes without the program. Conditional on fixed effects and other controls, the staggering of the program introduction is plausibly exogenous, so the parallel trends assumption is likely to hold. The parallel trends assumption can also be indirectly tested by assessing pre-trends, as discussed in the next paragraph, though it cannot be proved. Furthermore, households do not have any prior information regarding when the program would be rolled out in their district, so the "no anticipatory behavior" assumption is likely to hold.

As an exercise to support the parallel trends assumption, I choose dates when most households enrolled in the program in the sample. For each of those dates, I compare electricity consumption among districts that introduced the program early, districts that introduced the program late, and districts that did not have the program. Figure C.1 in Appendix C summarizes the enrollment timeline and enrollment rates in the whole sample. Figure C.2 shows household electricity consumption over the timeline. In levels, electricity consumption in assigned districts (either early adopter or late adopter) is consistently higher than in unassigned districts. Before each enrollment (or roll-out) date, electricity consumption however seems to follow similar trends among those districts, providing some supporting evidence for the parallel trends assumption.

Table 2 shows the estimation results from equation (3). All regressions include household and time fixed effects. Standard errors are clustered by district. Columns (2) control exogenous weather conditions, such as mean air temperature, humidity, rainfall, and sunshine.

Estimated coefficients for the post-treatment assignment, $TreatD \times Post$, are negative and statistically significant at the usual 5% confidence level. The program assignment contributes to a reduction in electricity consumption and, on average, households assigned to treatment reduced their electricity consumption by approximately 5% more than those not assigned to treatment. The program's effects may result from utilities advertising the program and educating households about electricity conservation (the informational effect), as well as households competing to win the rebates (the competition effect).[29]

4.2. Average Treatment Effect on the Treated

The difference-in-difference approach in equation (3) yields a valid estimate of the effect of the program assignment but not the actual treatment or enrollment. As an attempt to estimate the effect of the actual enrollment, I use the same approach by estimating the following equation:

$$\log(e_{it}) = \rho TreatH_i \times Post_t + \gamma_i + \delta_{my} + \epsilon_{it}$$

where the treatment indicator, $TreatH_i$, equals one for households that ever enrolled in the program. The interaction term $TreatH_i \times Post_t$ is a post-treatment indicator that equals one if household i enrolled in the program by time period t. Household and month-by-year fixed effects are included. The standard errors are clustered by district.

The program's staggered roll-out allows for a difference-in-difference specification with individual and time-fixed effects, which mitigate the selection bias since those fixed effects can control for time-invariant individual characteristics such as house features, head of household, and household sizes. However, time-variant variables like employment or income might correlate both with electricity consumption and the households' decision to enroll. For example, households that had recently lost employment or received a pay reduction might have decided to enroll in the program because they already knew they planned to reduce consumption to weather the income loss (not only because of the program). Alternatively, households might have decided to enroll because they knew they would be absent for vacation or some other personal reason, so the consumption reductions do not actually reflect the program's effect.

Given the possible selection and confounding biases, one estimation strategy to control these factors is using an instrumental variable for the treatment dummy. Households become

[29] As shown in the theoretical framework, the program's effect can be decomposed into the informational and competition effect channels. Unfortunately, I cannot empirically estimate the effect of each separately and find how much reduction in energy use is due to the cash rebate alone or how much reduction in energy use is due to the nudge alone. A carefully designed randomized control trial might be able to test how competition for rebates affects conservation decisions. For example, power companies can randomly assign households or districts into different control and treatment groups. The control group will not have the program. For the first treatment group, power companies offer rebates to 30% of eligible households (i.e., moderate competition). For the second treatment group, half of eligible households would get rewarded. For the third treatment group, all eligible households would get rewarded (i.e., no competition). Variations in treatment among those different groups would provide a good measure of competition.

eligible to enroll in the program if (and only if) the program is implemented in their district; thus, I propose using this enrollment assignment as an instrument for the actual enrollment. The instrument variable is an indicator $TreatD_i$, which equals one if household i is located in a district that has ever rolled out the program.

Two key identifying assumptions of the instrumental variable approach are the relevance and exclusion conditions. First, the relevance condition says that the instrumental variable must directly affect the instrumented variable. Only households in districts that have the program are eligible to enroll, so the relevance condition is likely to hold. The relevance condition can also be tested from the first-stage estimation. Second, the exclusion condition states that the enrollment assignment, or the instrumental variable $TreatD_i$, has no direct effect on electricity consumption but only exerts an effect through the actual enrollment. Conditional on fixed effects, the exclusion condition is likely to be satisfied since the assignment is plausibly exogenous.[30] Furthermore, it is unlikely that households would move to eligible districts to enroll in the program or that they know beforehand which districts will have the program in the future.

Table 3 reports results from the instrumental variable (IV) approach that estimates the actual enrollment effect. It also compares those IV results to the ordinary least squared (OLS) estimates. All regressions control for household- and time-fixed effects; the only difference between columns (1) and (2) is the inclusion of other control variables. The IV estimates are the program-localized average treatment effect on "complier" households that will enroll in the program whenever it is available for them. For the Vietnamese rebate program, the complier households are enrolled households or the treated, so I can interpret the IV estimates as the average treatment effect on the treated (ATT).

As expected, the ATT is larger than the ITT. The magnitudes of the post-treatment coefficient estimates suggest that, on average, households that choose to enroll in the program reduce electricity consumption by approximately 18% more than households that do not get treated. This estimated effect is larger than the eligibility reduction levels of 10%. This result is

[30] Figure C.2 in Appendix C provides some evidence that pre-treatment trends in electricity consumption are similar in districts that do and do not have the program. A possible threat here is whether there existed other programs simultaneously in place to spur conservation, especially if some districts were "targeted" for their high energy use. To address this, I interviewed power companies to ask for all their policies during the sample period and do not find any policy that could both perfectly correlate with the staggered adoption of the rebate program and affect electricity consumption.

not unexpected, since the analytical model's findings suggest that both information provision and competition among households might induce consumption reduction well beyond the eligibility threshold. Table 3 reports first-stage regression results and OLS estimates. Table 3 also reports F statistics large enough to reject the weak instrument hypothesis. OLS estimates are negative and statistically significant but have a much smaller magnitude than the IV estimates. [31]

5. Key Determinants of the Program's Effects

The previous section shows that the program's rebate scheme may be a promising way to promote conservation, but this finding does not yet address the question of what drives the program's effect. Knowing the answer to this question would help other policymakers design a more effective conservation rebate program. The analytical results above suggest that several key factors determine the program's effect on electricity consumption, including baseline consumption e_b, the eligibility threshold fraction a, the size of the cash prize Z, the probability of winning a cash prize, and exogenous factors X (e.g., air temperature). This section presents empirical tests regarding the key driving factors of the program's effects.

Previous sections assume that the average treatment effect is the same for all households that enrolled, regardless of their different baseline consumption or various program features across districts. In this section, I consider variations in households' baseline consumption and several program features across districts, such as the required minimum reduction, the value of cash prizes, and the length of the program. I also exploit variations in monthly mean air temperature across districts and time to examine whether temperature determines the heterogeneity of the program's effect.

To examine key factors that determine the program's effects, I estimate the interaction terms between the post-treatment binary variable and the key factors. For example, the equation estimating the intent-to-treat is the following:

$$\log(e_{it}) = \phi TreatD_i \times Post_t + \beta_{TV} TreatD_i \times Post_t \times TV_{dt} + \theta TV_{dt} + \beta_{TI} TreatD_i \times Post_t \times TI_d + \gamma_i + \delta_{my} + \epsilon_{it} \quad (4)$$

where the interaction term $TreatD_i \times Post_t$ is the post-treatment assignment binary variable. Subscript d denotes district d; TV is a set of time-varying factors (e.g., mean air temperature and

[31] Results are still robust when I add more control variables or use inverse hyperbolic sine transformations of variables instead of log forms. Appendix C shows the robustness check result tables.

electricity price); and TI denotes a group of time-invariant factors (e.g., baseline consumption and target reduction). The parameters of interest are the signs and values of β_{TV} and β_{TI}, which show whether the empirical predictions hold.

Table 4 reports intent-to-treat estimates from the difference-in-difference model, and Table 5 presents results from the instrumental variable approach. These two tables show empirical tests of the theoretical predictions in previous sections.[32] In the following paragraphs, I explain the empirical results for each theoretical prediction.

First, I consider mean air temperature as one exogenous factor X in the function of the winning probability. Hotter weather would imply that it is more difficult for all households to reduce consumption to the rebate-eligibility threshold or to win a cash prize. As a result, a smaller magnitude of both the ITT and the ATT on electricity conservation is expected (first prediction in Section 2.3). The coefficient for the effect of the interaction term between post-treatment and mean air temperature $TreatH \times Post \times Temp$ on electricity use is expected to be positive (where $Temp$ is the log of monthly mean air temperature). The estimated coefficients for the above interaction term in Tables 4 and 5 are all positive and statistically significant at 5% or 1% confidence levels. This finding provides a caution for programs that offer a rebate only if household consumption is below a certain threshold. Such programs will backfire if the weather is abnormally hot during the program period, which ironically is exactly when we would want more electricity conservation.

Second, the purpose of the program is to encourage households to reduce consumption during the summer months. More than four-fifths of districts in my sample ran the program for three months, and the others ran the program for five or six months. I construct a length-of-program indicator that equals one if the district ran the program for five or six months and zero if the district ran the program for three months. Since the program generally requires households to reduce consumption at least 10% for consecutive months of its duration, an increase in the program length implies that it becomes more difficult for households to meet the eligibility requirement and win cash prizes. Thus, the probability of winning cash rebates decreases as the program lengthens. As predicted in Section 2.3, the interaction term's estimated coefficients

[32] I consider several robustness checks by adding more controls and using inverse hyperbolic sine transformation of variables instead of log forms. The results are shown in Tables C.3 – C.4 in Appendix C.

between the post-treatment binary variable and the length-of-program indicator in Tables 4 and 5 are all positive.[33]

Baseline consumption also affects the probability of households winning a cash prize. Higher baseline consumption implies that it might be easier for the household to reduce consumption to at least the threshold to win a cash rebate. Thus, the theoretical model predicts that a higher baseline consumption implies a larger ITT and ATT. I construct a variable *Baseline* as the household monthly average electricity consumption during the same program months of the previous year in log form. The estimated coefficients for the interaction term between the post-treatment indicator variable and *Baseline* are all negative and statistically significant at 10% to 1% confidence level. This result also provides evidence that rebates will not provide incentives to people who are too far from the threshold reduction or too far from the winning reduction even if they are eligible.

The theoretical model predicts that a higher cash rebate value implies a larger ATT and ITT for all households. Larger prizes provide a greater incentive for households to enroll in the program and to compete to win a rebate. The estimated coefficients for the interaction term between post-treatment and the cash prize (Z in log) have the expected negative sign but are not statistically significant. A better measure of the cash rebate's size could be weighted by income. Unfortunately, data on households' income levels are not available.

Electricity prices are also an important factor that may determine the effects of the conservation rebate program. As predicted by the theoretical model, higher electricity prices will encourage households to enroll in the program and compete for cash prices. Thus, the interaction term's coefficient between the post-treatment indicator and electricity price (in log) is expected to be negative. Adding electricity price to the right-hand side of the regression equation might cause some concern about endogeneity. Since the price is determined by the interaction between the supply and demand, unobserved variables that affect the demand can also impact the price. Month-by-year fixed effects and the air temperature control help absorb general temperatures and some unobserved variables that might affect the price. Also, electricity prices are heavily

[33] One interesting question that I have not yet been able to examine is whether the number of program months affects the persistence of the program's effect. Intuitively, if the program is only a one-month program, a persistent effect might fail to arise, since a household might just win the cash prize by happenstance and not really try very hard to change their consumption behaviors to win the rebate. The length of the program studied in this paper is at least 3 months for all districts.

regulated and subsidized by the Vietnamese government (to avoid adverse and frequent price changes and ensure price stability). As a result, consumption changes among households in the sample due to the rebate program are not likely to significantly impact electricity price. The plausibly exogenous differential timing of the program assignment also helps to mitigate the endogeneity problem.

The empirical results in Tables 4 and 5 show estimated coefficients for the interaction term between the post-treatment indicator and electricity price are all positive and statistically significant at 5% or 1% confidence level. The magnitudes of those coefficients, as shown in the third last row of columns 6 and 7 of Table 5, suggest that, all other things equal, a one percent increase in the electricity price results in an additional 2.5% to 3.2% reduction in electricity consumption among households that enrolled in the program compared to those that did not. The results suggest that the electricity rebate program might work better in periods or countries where electricity prices are higher. Another policy suggestion is to design a policy that combines a conservation rebate program and an electricity price increase.

Finally, I want to assess whether the threshold a affects the impact of the program on conservation. Although I have some variation in a in my sample, a limitation of this exercise is that almost all districts set the minimum consumption reduction a at 10%. Only one district, with more than 40,000 households, sets the threshold at 15%. A caution here is the statistical power might be low, so I interpret the results as suggestive only. I define a binary variable *Threshold* that equals one if the district required a 15% electricity consumption reduction and equals zero if the minimum reduction requirement was 10%.

On the one hand, a higher threshold reduction a implies more difficulties for a household to win a cash rebate, so households may have been less likely to enroll in the program. The ITT on electricity conservation is predicted to decrease with the threshold reduction, so I expect a positive coefficient for the interaction term between the post-treatment indicator and *Threshold*. Table 4 shows that the interaction term's estimated coefficients are all positive as predicted but not statistically significant. On the other hand, a higher threshold reduction a suggests that households that decided to enroll might have to try harder to reduce consumption beyond the higher threshold to *win* a rebate. The ATT on electricity conservation can either decrease or increase with the threshold reduction, depending on whether the threshold is high enough to discourage competition. The empirical results in Table 5 show mixed signs on the interaction term, and estimates are not statistically significant.

6. Persistence of the Program's Effect

The long household-level panel data availability allows me to investigate the program's effect months after its end. Specifically, I exploit the variations in the program's timing across locations and employ an event-study style empirical approach to tease out the program's effect. I pool information from January 2012 through December 2017 for households that enrolled or did not enroll in the program. I construct a series of indicator variables for the number of months before and after the household enrolled in the program and the program became effective. $TreatH_i \times Post_t^k$ is one if, in period t, household i enrolled k months later (or, if k is negative, household i had enrolled $-k$ months earlier).[34] The event study approach normalizes the time when households enrolled in the program to represent t=0, so the estimating equation is:

$$\log(e_{it}) = \sum_{k=-12,k\neq-1}^{18} \rho_k TreatH_i \times Post_t^k + \gamma_i + \delta_{my} + \epsilon_{it} \quad (5)$$

where $\log(e_{it})$ is logged electricity consumption of household i at time t. The indicator variable $TreatH_i \times Post_t^k$, $k = -12, \dots, -2,0,1,2, \dots, 18$ represent the enrollment event.[35] I control for household and month-by-year fixed effects, γ_i and δ_{my} respectively. The coefficients of interests are ρ_k, which capture the effect of program enrollment on a household's electricity consumption k months following its occurrence, compared to time -1 (the month before the household enrolled in the program). Note that $TreatH_i \times Post_t^{-1}$ is not included in equation (5), so coefficients on the event dummies are all compared to this omitted indicator. The standard errors are clustered at the district level.

The event-study approach relies on assumptions of parallel trends and no anticipatory behavior. Parallel trends mean that enrolled households and not-enrolled households would have experienced the same electricity use changes without the program. Conditional on fixed effects

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[34] Note that $post_t^k$ is a binary variable that indicates the number of months before and after the program became effective or was rolled out in household i's district.

[35] I have a balanced panel of T=72 time periods, so with varying event dates, I bin up endpoints to have a balanced sample in event time. In equation (5), two endpoints are -12 and 18, where $TreatH_i \times Post_t^{-12}$ is one if, in period t, household i had enrolled 12 or more months earlier, and $TreatH_i \times Post_t^{18}$ is one if, in period t, households i enrolled 18 or more months later.

and other controls, the program's roll-out timing is plausibly exogenous, so the parallel trends assumption is likely to hold.[36] No anticipatory behavior assumption is also expected to hold because households did not know beforehand when the program would be available.

Figure 4 graphically illustrates the event-study result, which is the plot of all estimated coefficients ρ^k over time. The vertical axis is the change in logged electricity consumption or estimates of coefficients ρ^k. The horizontal axis shows the number of months relative to the program's enrollment k. The solid line represents point estimates and the dashed lines indicate 95% confidence intervals. Point estimates of ρ^k on the months before enrollment are not statistically significantly different from zero, while most point estimates on the months after enrollment are negative and statistically significant.[37] These findings provide further evidence that the outcome's pre-treatment trends are similar between enrolled and not-enrolled households.[38] Negative and statistically significant estimated coefficients on the event indicators months after the enrollment also suggest that the Vietnamese short-run conservation rebate program's effects lasts for months even after the program ends.[39]

In terms of magnitude, electricity consumption reductions are the lowest during the first three months after the program takes effect, and then the reduction remains around the 5% reduction level after the program period, which is between 3-6 months. Possible explanations for this pattern and the persistence of the program's effect are (1) households have learned over time

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[36] Similar to the exercise discussed in the previous section, I choose dates when the most households enrolled in the program. For each of those dates, I track and compare electricity consumption between households that enrolled in the program, household that were assigned but chose not to enroll when the program was available in their district, and households that were never assigned to treatment during the sample period. Figure C.3 in Appendix C shows household electricity consumption over the timeline. Consumption before the enrollment dates is similar between enrolled households and unenrolled households.

[37] Point estimates at t=8 and t=11 are all negative but less precise.

[38] However, the similar pre-trends might not be enough because some might worry that other policies also occur at the same time as the rebate program, which will contaminate the identification. The variations in the timing of the rebate program across locations allow me to tease out the effect of the rebate program, unless those other policies correlate with the rebate program both in terms of geography and in terms of time. I do not know any such other policy after discussing with power companies about their other policies during the sample period.

[39] The recent and emerging literature on event studies with staggered adoption shows that the two-way fixed effects difference-in-differences estimators can be biased with the presence of heterogeneous treatment effects either across groups or over time (Athey and Imbens, 2018; Goodman-Bacon, 2018; Sun and Abraham, 2020; Callaway and Sant'Anna, 2020; Baker *et al.*, 2021). The bias is attributable to the use of already treated units as effective controls for later-treated units. For robustness check, I implement Callaway and Sant'Anna (2020) estimator that corrects potential biases in two-way fixed effects difference-in-differences estimates. The results show that changes in electricity consumption are close to zero before the program enrollment but have a downward sloping trend right after enrollment.

to adjust their behaviors to conserve electricity more effectively, and (2) households invested in new energy efficient appliances that they continue to use for months after the investment.[40] The persistence of the effect has an important implication for the program's cost-effectiveness, which I discuss in the next section.

7. Discussions of the Program's Costs and Benefits

Following prior literature, I evaluate the cost-effectiveness of the Vietnamese rebate program using two cost ratios: (1) the program's cost per unit of electricity saved and (2) the program's cost per ton of CO_2 emissions avoided. First, I use my estimates of the program's effect on electricity reduction to calculate the total electricity saved. Then, the total CO_2 emissions avoided equals the total electricity saved multiplied by the carbon emission intensity factor for the electricity generation sector in Vietnam, which is 2.5 kilogram CO_2 per kilogram oil equivalent or 0.215 metric ton of CO_2 per MWh.[41] Second, the program's total cost includes reward payments to households (i.e., *direct* costs) and the expenses associated with promoting the program (i.e., *indirect* costs), which are both in the utility cost administrative data. Since only a small portion of eligible households win cash rebates, these reward payments only account for about 17% of the program's total cost. The total payment to all households in the sample is 0.9 billion Vietnamese Dong (VND), or \$38,796 (USD).[42] The program's promotion costs for all provinces in the sample is 4.3 billion VND (or \$186,905 USD).

Table 6 reports the program's cost ratios (per MWh, or per ton of CO_2). Panel A shows calculations based on the ITT point estimate of electricity reduction; Panel B presents

[40] Compared to developing countries like the U.S., household ownership of electric appliances such as air conditioners is much less common in developing countries like Vietnam. For example, in 2016, the penetration of air conditioners in Vietnam was less than 20% while in the U.S. it was 90% (data can be accessed at https://asia.nikkei.com/Business/Daikin-plans-air-conditioner-factory-in-Vietnam and https://www.statista.com/statistics/911064/worldwide-air-conditioning-penetration-rate-country/). However, the hotter weather and rising income will spur new demand for heavy use of air conditioners in developing countries (Wolfram et al., 2012). This paper cannot assess whether the rebate program induces new purchases of energy-efficient appliances, since data on household ownership of electric appliances is not available. Nevertheless, evidence that the program's effect continues after its period might suggest an important possibility that a conservation program in a developing country can last longer if it encourages households to purchase more energy-efficient appliances.

[41] https://ens.dk/sites/ens.dk/files/Globalcooperation/Official_docs/Vietnam/vietnam-energy-outlook-report-2017-eng.pdf.

[42] For rough calculation, I use the exchange rate of 23,000 VND per U.S. dollar. All reward payments and promotion costs are reported in the program year's money, so I assume a discount rate of 5% to calculate the values of those costs in 2017 dollars.

calculations based on the ATT point estimate. The first column of Table 6 reports cost ratios under the assumption that the program is only effective during its length (which is often three to six summer months). Given the empirical evidence that the program's effects continues even after it ends, columns (2)–(4) in Table 6 present cost ratios under assumptions that the program is effective after it ends, by an additional three months, six months, or twelve months. The calculated costs per megawatt hour (MWh) saved or per ton of CO_2 avoided are reduced greatly when I account for the effect's suggested persistence. The costs per ton of CO_2 emissions in column (4) are \$24 and \$27, which are only a third of those in column (1) and below most reliable social cost of carbon estimates (Nordhaus, 2017; Revesz et al., 2017).

These computed cost ratios are much smaller than most available estimates from energy-efficiency or conservation programs in the literature. For example, Ito (2015) studies California's statewide "20/20" electricity rebate program, which provides households a 20% electricity bill reduction if they reduce consumption by at least 20% compared to consumption in the previous year. He finds the program cost per ton of carbon dioxide emissions is \$390. Davis *et al*. (2014) evaluate the cost-effectiveness of an appliance replacement program in Mexico and find the program cost per ton of carbon dioxide emissions is \$547. Unfortunately, no estimate of any other energy conservation programs in Vietnam is available for comparison.

The ultimate question is whether the program is welfare-enhancing. Since the electricity price is heavily regulated and subsidized by the Vietnamese government, funding for the program comes from all electricity ratepayers and taxpayers. Therefore, part of the program's dollar cost is a pure transfer to rebate winners from other ratepayers.[43] Whether the program enhances welfare depends much on comparing the social cost of funding the program using tax dollars and the avoided social cost of electricity generation. The social cost of the taxpayer-funded portion of the program is determined by that dollar amount times the marginal cost of public funds (MCPF). The social cost of electricity generation is the sum of the dollar cost of generation and the negative externality due to emissions from electricity production. It is hard to measure whether the program is welfare-enhancing because I do not have a reliable estimate of the MCPF in Vietnam. A higher marginal cost of public funds might imply that the program is more likely to result in a positive net social cost and thus is less likely to enhance welfare.

[43] In fact, if the portion of the funding from other ratepayers means an increase in the general price per kWh, then it can have a social cost less than the dollar cost if the higher price per KWH itself results in less use of electricity.

Calculations below provide suggestive information to policymakers on the possible welfare implication of the rebate program in Vietnam.

I first compare the dollar costs of the program and the electricity generation avoided. The program's total dollar cost, which sums reward payments and advertising costs as shown in Table 6, is \$225,701. I calculate the dollar cost of electricity generation avoided by multiplying the total electricity saved, as shown in Table 6 and the marginal cost of electricity generation provided by the Vietnamese electric utility, which is \$35 per MWh. The avoided cost of electricity generation using the ITT estimate is \$457,482 under the assumption that the program only reduces consumption during its length.[44] Under the assumption that the effect can persist 12 months after the program ends, the dollar cost of electricity generation saved is \$1,509,285, which is more than six times the program's dollar cost.[45] If electricity utilities are private firms, then it might be worthwhile to them to implement the program since the dollar or private benefits are greater than the private costs.

Next, I consider the social cost and benefit of the program. I calculate the program's social benefit by adding the dollar cost of electricity generation saved and the avoided social cost of carbon dioxide emissions, which equals the social cost of carbon per metric ton (SCC) times the total emissions avoided. I use the SCC=\$40 assumption and the ITT estimate.[46] Thus, the calculated social benefit is \$569,872 under the assumption that the program's effect only lasts during its length, which should be considered a lower bound of the program's social benefit for several reasons. It does not account for the benefits from persistence beyond the program length, the benefits of non-carbon emissions reduction, and the avoidance of power shortages. The social cost of the program is the taxpayer-funded portion of the program times the MCPF. The portion of the program paid by tax dollars and the MCPF are both not readily available. Therefore for

[44] All dollar values are in 2017 dollars.

[45] Note that the program's dollar costs should also include the costs borne by households that reduced their electricity use, such as the extra cost of their investment in more energy-efficient household appliances. Unfortunately, I have neither these cost data nor a feasible systematic approach to assess these costs, so I have to omit them from the cost measure. However, assuming that households are risk-neutral and optimize their behaviors, the costs borne by households that chose to reduce their electricity consumption are well-justified by the benefits they got from, for example, their reduced electricity bills. Thus the electricity reduction decision or effort is welfare-enhancing to them.

[46] The social cost of \$40 per metric ton of CO_2 is the most updated estimate to date by Greenstone and his group at the Energy Policy Institute at the University of Chicago (EPIC) https://epic.uchicago.edu/area-of-focus/social-cost-of-carbon/.

rough calculations I assume that taxpayers fund all the program costs, and I define and calculate a threshold MCPF that barely equates the social benefit and the social cost of the program and makes the program worthwhile.[47] Dividing the calculated social benefit by the program's dollar cost yields the threshold MCPF=2.52. If the actual marginal cost of public funds is less than the threshold marginal cost of public funds, the program might enhance social welfare. The calculated threshold MCPF is slightly above the range of most MCPF estimates in the literature, which is between 0.5 and 2.5 in OECD (The Organization for Economic Co-operation and Development) countries, European Union members, and African countries (Auriol & Warlters, 2012; Barrios et al., 2013; Kleven & Kreiner, 2006).

8. Conclusion

The Vietnamese rebate program has an interesting design that encourages competition among individual households trying to conserve enough energy to win a cash prize. This paper examines the effect of such a competitive rebate program on electricity use. The empirical results suggest that the program reduces electricity consumption by 18%, and the effects of the program persists for at least a year after its end. The program's costs per unit of electricity saved and per ton of emissions abated are smaller than most available estimates in the literature even before accounting for the program's persistent effects.

I theoretically show that, relative to a fixed-threshold program, competition for rebates can effectively encourage electricity conservation for more households, among those that can meet the threshold reduction. Also, the competitive rebate program can induce conservation beyond the fixed threshold. Unfortunately, I cannot empirically test or separate the effect of competition due to the lack of a reliable measure of competition among households during the sample period. In the case of the Vietnamese rebate program, households have almost no information about how many people would enroll, the percentage of enrolled households that would meet the eligibility threshold, or the percentage of eligible households that would get awards.

The paper also identifies key factors that may determine the likelihood that households enroll in the conservation rebate program and reduce consumption, such as air temperature,

[47] Refer to footnote 39, if the program cost is partly funded by ratepayers, the calculated MCPF is expected to be larger.

baseline consumption, and electricity pricing. These findings are important for policymakers who want to design a successful conservation rebate policy. For example, both the theoretical and empirical models find that a higher price for electricity encourages more households to enroll and compete for the rebates. Thus, this result implies that combining a conservation rebate policy with an electricity price increase might improve its effectiveness. The findings also inform policymakers regarding whether to introduce the program in specific communities with certain conditions such as certain climatic zones or electricity use patterns. For instance, the paper finds that the effect of the Vietnamese rebate program on electricity conservation decreases with air temperature, and thus if such a program were implemented in areas with abnormal heat during the program's months, it might backfire.

Finally, this paper provides an analysis of an interesting program in a developing country, an analysis that might be useful to other developing countries seeking to implement something similar.

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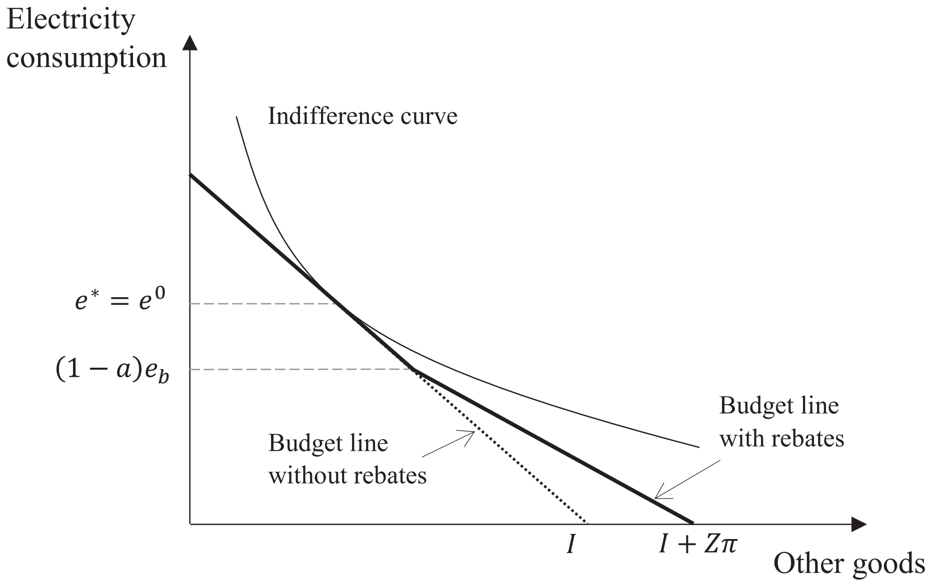
FIGURES

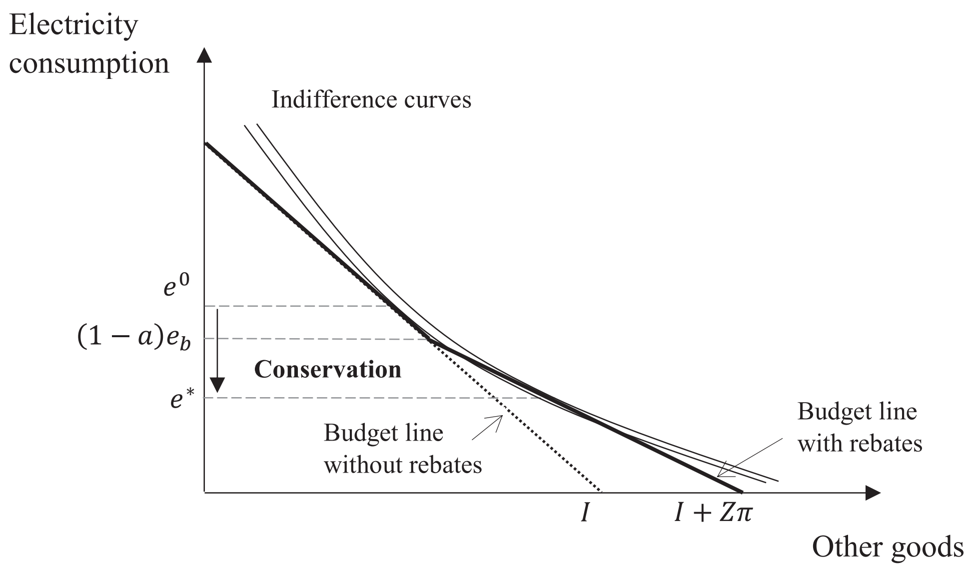
Figure 1:
Far from the threshold, households choose as if no rebate ($e^* = e^0 > (1-a)e_b$)



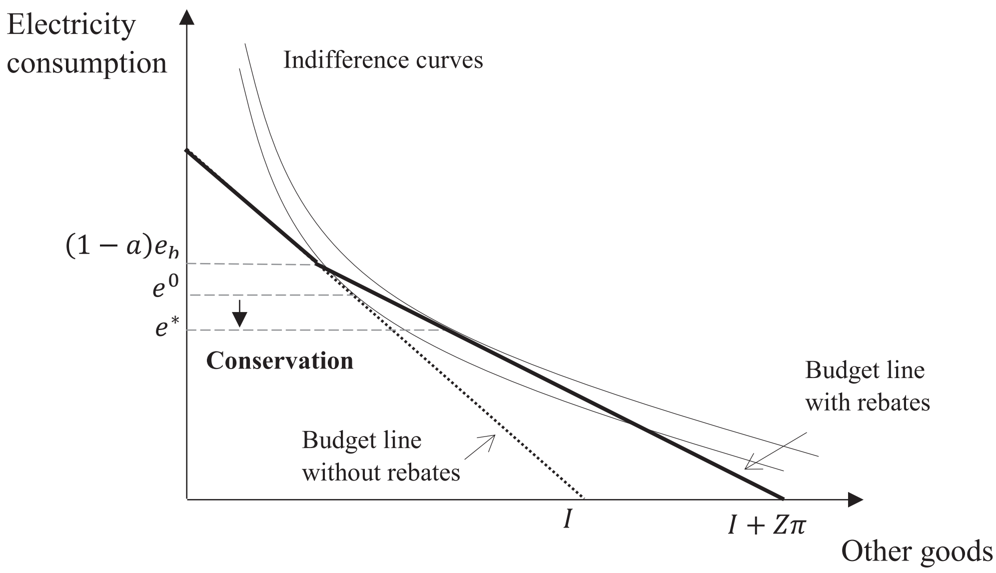
Notes: This figure illustrates the case where the program fails to induce the household to reduce electricity use. Consumption remains the same with or without the program ($e^* = e^0$). The probability function π determines the shape of the new portion of the budget line when electricity consumption is below the eligibility threshold. This figure provides an example of function π that changes linearly with e. A positive probability of winning the rebate creates a kink in the household's budget constraint as if it receives a marginal subsidy for each unit of reduction relative to the baseline e_b.

Figure 2: Households consume e^* less than $(1-a)e_b$

Scenario A: $e^0 > (1-a)e_b$

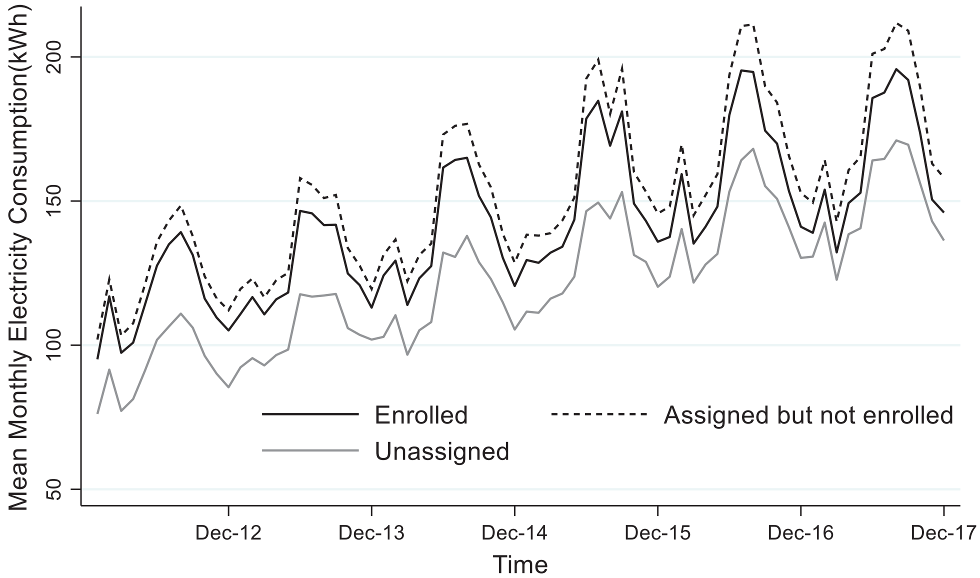


Scenario B: $e^0 < (1-a)e_b$



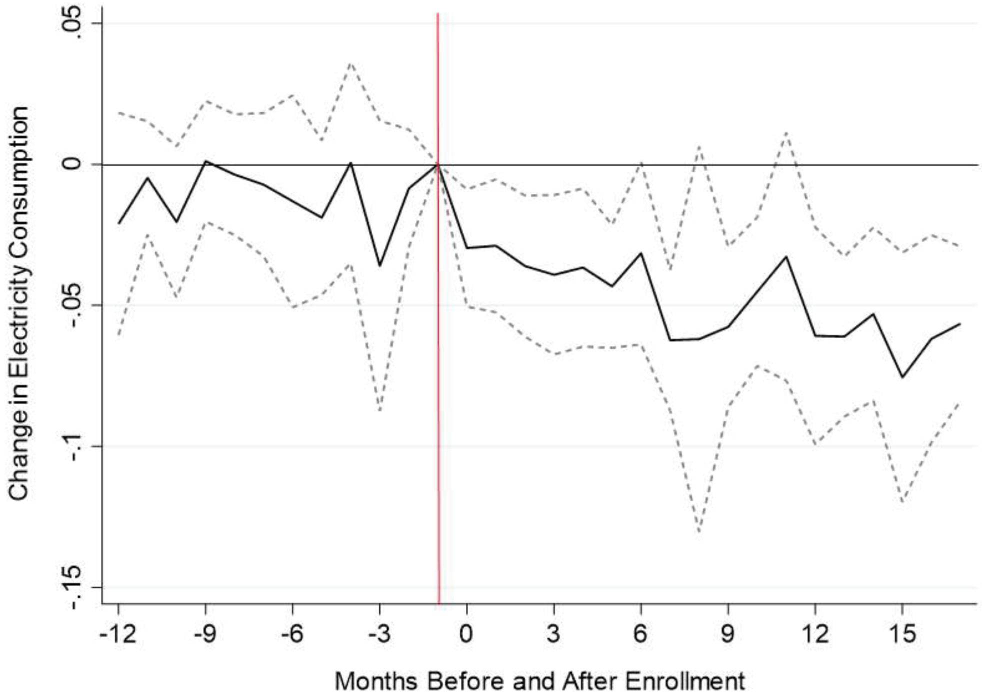
Notes: Figure 2 illustrates two scenarios where the program induces households to reduce electricity consumption. Conservation is measured by the difference between household's electricity consumption without the program (e^0) and with the program (e^*). The probability function π determines the shape of the new portion of the budget line when electricity consumption is below the eligibility threshold. This figure provides an example of function π that changes linearly with e. A positive probability of winning the rebate creates a kink in the household's budget constraint as if it receives a marginal subsidy for each unit of reduction relative to the baseline e_b.

Figure 3: Electricity Consumption by Treatment Status



Notes: This figure compares electricity consumption among enrolled, assigned but not enrolled, and unassigned households. I define in the text three groups of households: enrolled households that enrolled in the program (black dashed line), assigned but not enrolled households that were eligible but chose not to enroll (black solid line), and unassigned households whose districts did not have the program in the sample period (grey line).

Figure 4: The Program's Effect on Electricity Consumption over Time



Notes: This figure illustrates the event-study regression result equation (5). The vertical axis is the change in logged electricity consumption. The horizontal axis shows the number of months relative to the program's enrollment. The solid line represents point estimates and the dashed line indicates 95% confidence intervals. The standard errors are clustered at the district level. Controls include households and year fixed effects.

TABLES:

Table 1: Summary Statistics

	Mean	S.D.	Min.	Max.	N
Monthly Household-Level Variables					
Electricity Consumption (kWh)	140	153	1	4000	45,565,601
Treated Household (0/1)	0.14	0.35	0	1	45,565,601
Monthly District-Level Variables					
Treated District (0/1)	0.48	0.50	0	1	1,944
Prize Tier 1 (000's VND)	393	330	0	1,000	1,944
Prize Tier 2 (000's VND)	240	159	0	500	1,944
Mean Air Temperature (°C)	23.1	5.2	9.5	30.8	1,944
Humidity (%)	82.4	4.2	65	94	1,944
Sunshine (Hours)	115	55.3	2.6	234	1,944
Rainfall (Mm Station)	140	130	0	766	1,944
Monthly Province-Level Variables					
Electricity Price Block 1 (000's VND/kWh)	2.15	0.12	1.88	2.34	216
Electricity Price Block 2 (000's VND/kWh)	2.36	0.16	2.01	2.62	216
Electricity Price Block 3 (000's VND/kWh)	2.44	0.17	2.06	2.7	216
Annual Province-Level Variables					
Birth Rate (per 1000 People)	18.4	2.9	13	23	18
Death Rate (per 1000 People)	8.6	2.5	5	14	18
In-migration (per 1000 People)	6.8	5.7	1	19	18
Out-migration (per 1000 People)	6.1	3.2	3	16	18
Net migration (per 1000 People)	0.8	7.0	-11	13	18
Cost of Living Index (%)	90.6	4.3	83	97	18
Industrial Production Index (%)	115	26.9	43	175	18
Average Population (000's People)	1,172	557	515	1,853	18
Population Density (People/km^2)	861	583	77	1,477	18
Number of Passengers Traffic (Million/km2)	644	577	76	1,638	18
Literate (%)	93.5	6.4	81.5	98.6	18

Table 2: Intent-to-treat Effect of the Rebate Program on Electricity Consumption

	(1)	(2)
TreatD×Post	-0.046**	-0.047**
	(0.019)	(0.020)
Controls	No	Yes
Fixed Effects	Yes	Yes
Observation	45,565,601	45,565,601
Adj R-squared	0.710	0.710

Notes: This table reports estimates of the intent-to-treat effect of the rebate program. All regressions include household and time-fixed effects. Standard errors (in parentheses) are clustered by district. *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

Table 3: OLS and IV Estimates of the Effect of the Program on Electricity Consumption

	(1)	(2)
Instrumental Variables:		
TreatH×Post	-0.178**	-0.180**
	(0.079)	(0.080)
First-stage Regression		
TreatD×Post	0.260***	0.260***
	(0.049)	(0.050)
Ordinary Least Squares:		
TreatH×Post	-0.048***	-0.048***
	(0.017)	(0.017)
Controls	No	Yes
Fixed Effects	Yes	Yes
Clusters	27	27
Observations	45,565,601	45,565,601
Kleibergen-Paap Wald rk F	28.13	28.24
Adj R-squared	0.710	0.710

Notes: This table reports and compares regression results from Ordinary Least Squares (OLS) and Instrumental Variable (IV) approaches. The dependent variable is electricity consumption in log form. All regressions include household and time-fixed effects. Standard errors (in parentheses) are clustered by district. *** Significant at the

Table 4: Heterogeneity of the Intent-to-treat Effect

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
TreatD×Post	-0.3252***	0.1636	-0.2743***	-0.2801***	-0.2867***	0.1302	0.6920**
	(0.110)	(0.156)	(0.092)	(0.088)	(0.096)	(0.109)	(0.249)
TreatD×Post×Temp	0.0722***	0.0818***	0.0729***	0.0719***	0.0702***	0.0734***	0.0814***
	(0.026)	(0.026)	(0.025)	(0.026)	(0.026)	(0.026)	(0.027)
TreatD×Post×Baseline		-0.1107***					-0.110***
		(0.036)					(0.036)
TreatD×Post×Threshold			-0.022				-0.0007
			(0.028)				(0.024)
TreatD×Post×Prize				-0.001			-0.0006
				(0.001)			(0.001)
TreatD×Post×Program Length					0.016		0.045*
					(0.020)		(0.024)
TreatD×Post×Electricity Price						-0.5138**	-0.6116***
						(0.168)	(0.185)
Observation	45,565,601	45,534,655	45,565,601	45,565,601	45,565,601	45,565,601	45,534,655
Adj R-squared	0.710	0.711	0.710	0.710	0.710	0.710	0.711

Notes: This table reports the heterogeneity of the program's intent-to-treat effect. The dependent variable is electricity consumption in log form. All regressions include household and time-fixed effects. Standard errors (in parentheses) are clustered by district.

*** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

Table 5: Heterogeneity of the Average Treatment Effect on the Treated

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
TreatH×Post	-1.2015**	1.2181	-1.2026**	-1.1880***	-1.290**	0.7557	3.6748**
	(0.456)	(1.109)	(0.462)	(0.440)	(0.492)	(0.587)	(1.672)
TreatH×Post×Temp	0.3223**	0.3333**	0.3146**	0.3226**	0.3092**	0.3234**	0.3211**
	(0.127)	(0.130)	(0.123)	(0.128)	(0.127)	(0.129)	(0.133)
TreatH×Post×Baseline		-0.5116*					-0.5116*
		(0.264)					(0.258)
TreatH×Post×Threshold			0.083				-0.003
			(0.121)				(0.083)
TreatH×Post×Prize				-0.0025			-0.0038
				(0.006)			(0.004)
TreatH×Post×Program Length					0.263		0.287*
					(0.166)		(0.146)
TreatH×Post×Electricity Price						-2.453**	-3.174**
						(1.013)	(1.156)
Observations	45,565,601	45,534,655	45,565,601	45,565,601	45,565,601	45,565,601	45,534,655
Kleibergen-Paap Wald rk F	19.83	13.21	21.969	13.27	14.39	32.23	20.30

Notes: This table reports IV estimates of the program's heterogeneous effects. The dependent variable is electricity consumption in log form. All regressions include household and time-fixed effects. Standard errors (in parentheses) are clustered by district. *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

Table 6: Cost-effectiveness of the Program

	Program months (1)	3 months after (2)	6 months after (3)	12 months after (4)
Panel A: Based on ITT estimate				
Electricity saved (MWh)	11,683	17,634	22,505	38,539
Avoided emissions (metric tons)	2,512	3,791	4,839	8,286
Reward payments ($)	38,796	38,796	38,796	38,796
Promotion costs ($)	186,905	186,905	186,905	186,905
Cost per MWh ($)	19.32	12.80	10.03	5.86
Cost per ton of CO_2 emissions avoided ($)	89.85	59.54	46.64	27.24
Panel B: Based on ATT estimate				
Electricity saved (MWh)	12,725	20,400	25,681	43,295
Avoided emissions (metric ton)	2,736	4,386	5,521	9,308
Reward payments ($)	38,796	38,796	38,796	38,796
Promotion costs ($)	186,905	186,905	186,905	186,905
Cost per MWh ($)	17.74	11.06	8.79	5.21
Cost per ton of CO_2 emissions avoided ($)	82.50	51.46	40.88	24.25

Notes: This table reports the program's cost and benefit measures. Column (1) reports costs and benefits under the scenario that the program is only effective during its length. Columns (2) – (4) assume the program is effective 3, 6, and 12 months after it ends.

Appendices to be Placed Online

Appendix A: Varying Conservation Rebates

Section 2.2 in the text studies the fixed rebate structure. Here, I consider varying rebates that depend on the level of conservation effort, or equivalently, the level of electricity consumption. In particular, a household that wins a rebate is entitled to d percent discount on its electricity bills. The household's optimizing problem is to maximize their expected utility subject to the budget constraint:

$$\max_{e}[v(e) + I - ep + epd\pi(\Delta^e, F, a; X)] \tag{A.1}$$

Again, as for the fixed cash rebate scheme, I consider the household's enrollment decision and the effect of enrollment on electricity consumption. Similar to inequality (1), the household will choose to enroll in the program if the expected utility from consuming e^0 is less than the expected utility from consuming at the threshold consumption level, $(1-a)e_b$:

$$v((1-a)e_b) - e_b(1-a)p\big(1 - d\pi(a, F, a; X)\big) \geq v(e^0) - e^0 p$$

$$\frac{v(e^0) - v((1-a)e_b)}{(e^0 - (1-a)e_b)} \leq \frac{d(1-a)e_b p\pi(a, F, a; X)}{(e^0 - (1-a)e_b)} + p \tag{A.2}$$

Inequality (A.2) depends on the probability of winning the rebates π, the difference between the optimal electricity consumption with the absence of conservation incentives e^0, the threshold consumption $(1-a)e_b$, the shape of the indifference curve, and the magnitude of the rebates (which are determined by the level of electricity consumption and the percentage discount d). Under this consideration, the varying rebate scheme results are quite similar to those from the fixed cash rebate scheme, as inequalities (1) and (A.2) are much the same.

The second consideration is when the household decides to enroll in the program and can meet the threshold reduction, or $e^* \leq (1-a)e_b$. Then, the first-order condition of the household"s optimizing problem (A.1) yields:

$$v_e(e^*) = p - e^* pd\pi_e\big(\Delta^{e^*}, F, a; X\big) - pd\pi\big(\Delta^{e^*}, F, a; X\big) \tag{A.3}$$

where the second term of (A.3) with the negative sign is positive: $-e^* pd\pi_e\big(\Delta^{e^*}, F, a; X\big) > 0$. The third term of (A.3) with the negative sign is the negative of the expected value of electricity bill discounts, so $-pd\pi\big(\Delta^{e^*}, F, a; X\big) < 0$. Equation (A.3) suggests that competition for the rebates induces consumption reduction, while varying rebates that depend on the consumption level induce households to consume more electricity. Intuitively, households who get rebates that vary with the level of electricity consumption in effect face lower electricity prices, and thus households increase their electricity consumption through the substitution effect. The competition also encourages households to reduce their electricity consumption to increase their chance of getting a rebate. In either the case of fixed cash rebates or varying rebates, competition itself helps to induce conservation.

Appendix B: Proofs of Empirical Predictions

This appendix presents mathematical proofs corresponding to predictions in Section 2.3 in the text. All proofs are straightforward and based on total differentiation of both sides of inequality (1) in the text with respect to key factors, including exogenous factor X, baseline consumption e_b, size of prize Z, and electricity price p. Note that if inequality (1) holds, households will not reduce their consumption and thus the program will not be effective in reducing electricity use.

For the first prediction regarding the exogenous factor X, I can show that the right hand side of inequality (1) increases in π as $Z > 0$ and $(e^0 - (1-a)e_b) > 0$. Thus, inequality (1) is more likely to hold when the probability of winning the rebates π decreases.

For prediction regarding baseline consumption. First, I totally differentiate the right hand side of (1) with respect to e_b, and the differentiation yields:

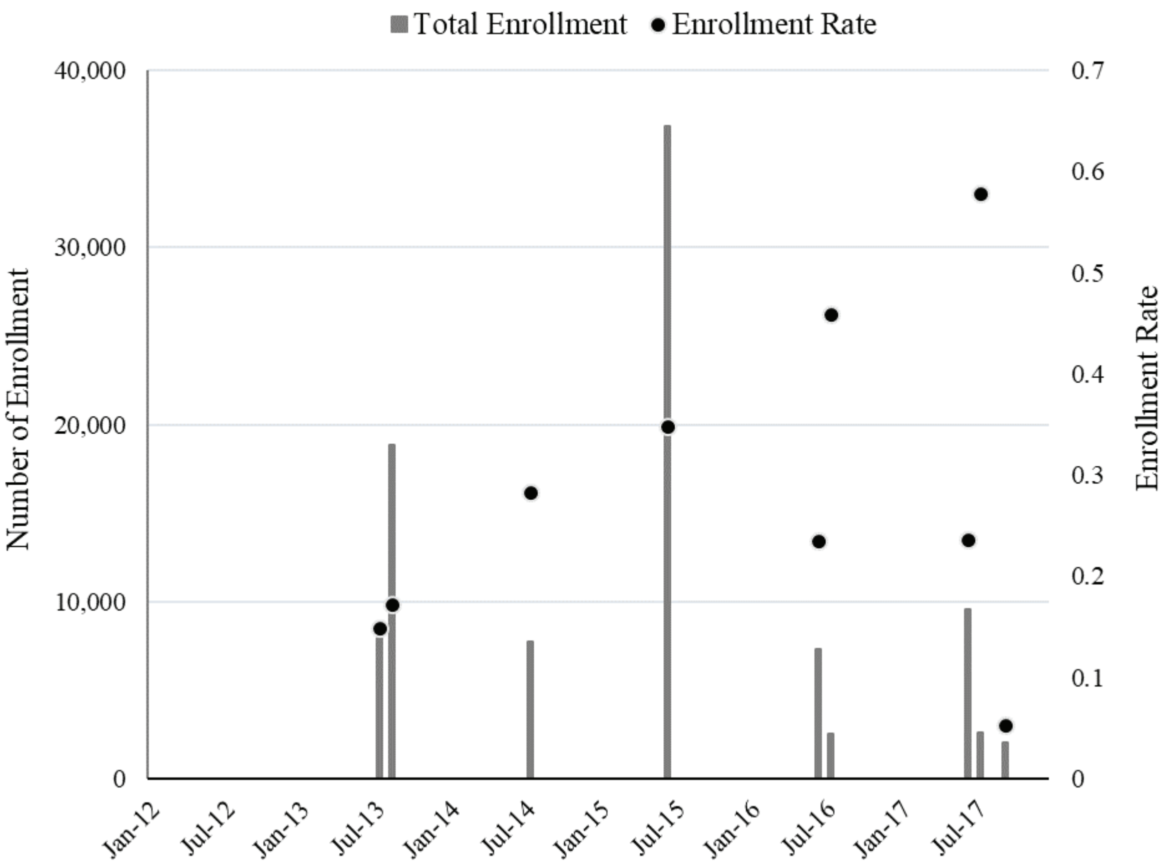
$$\frac{Z\pi(a, F, a; X)(1-a)}{(e^0 - (1-a)e_b)^2} \geq 0,$$

which implies that the right hand side of (1) increases with e_b. Given the assumption that function $v(e)$ is increasing and strictly concave, it can be easily shown that the left hand side of (1) decreases with e_b. Thus, inequality (1) is less likely to hold when e_b is larger.

Finally, it is ready to see that the right hand side of (1) increases with both Z and p, and thus inequality (1) is less likely to hold when Z and p are larger.

Appendix C: Evidence for Parallel Trends Assumption and Robustness Check Results

Figure C.1: Enrollment Timeline and Enrollment Rate



Notes: This figure shows the number of enrollment and the enrollment rate over time during the sample period from January 2012 to December 2017. The gray columns show the number of enrollment. The black dots represent the enrollment rate.

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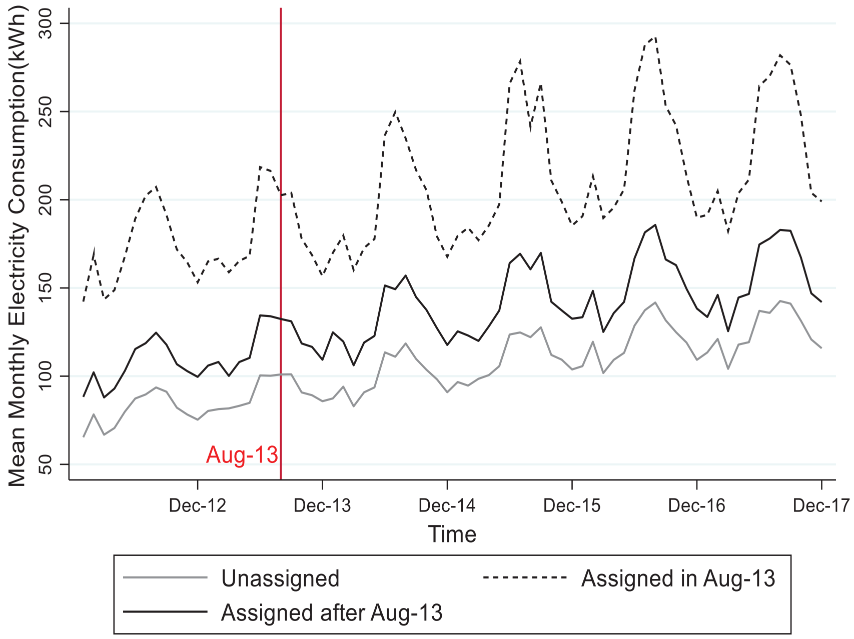
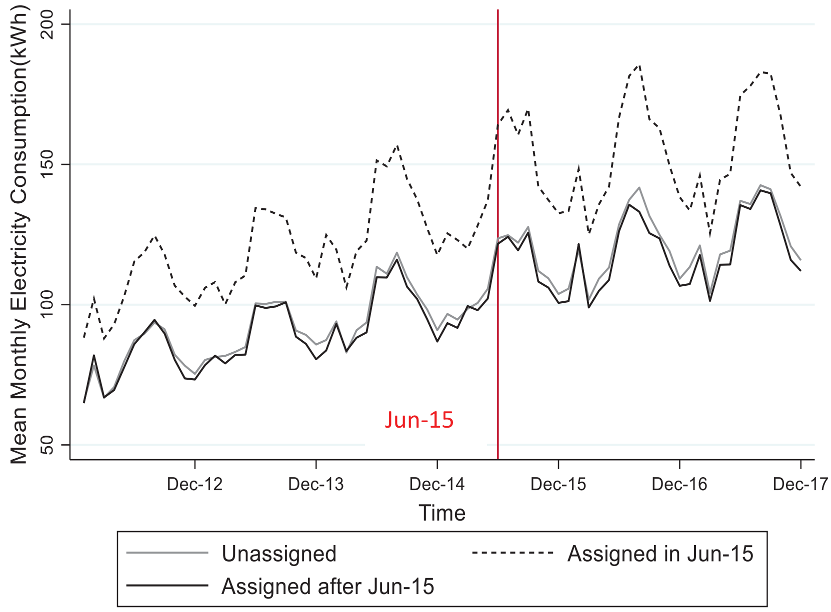
Figure C.2: Comparing Electricity Consumption among Assigned and Unassigned Districts

A. Program started in August 2013



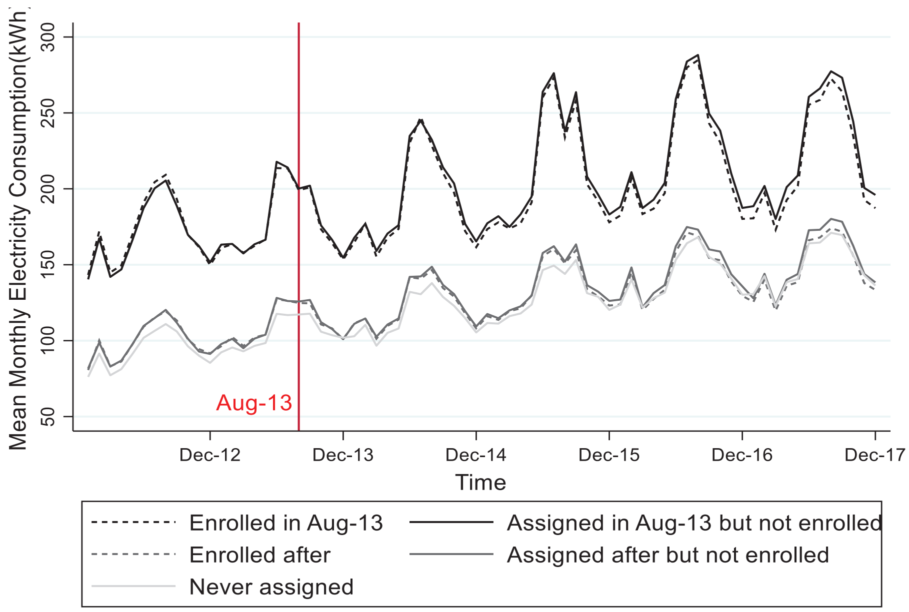
B. Program started in June 2015



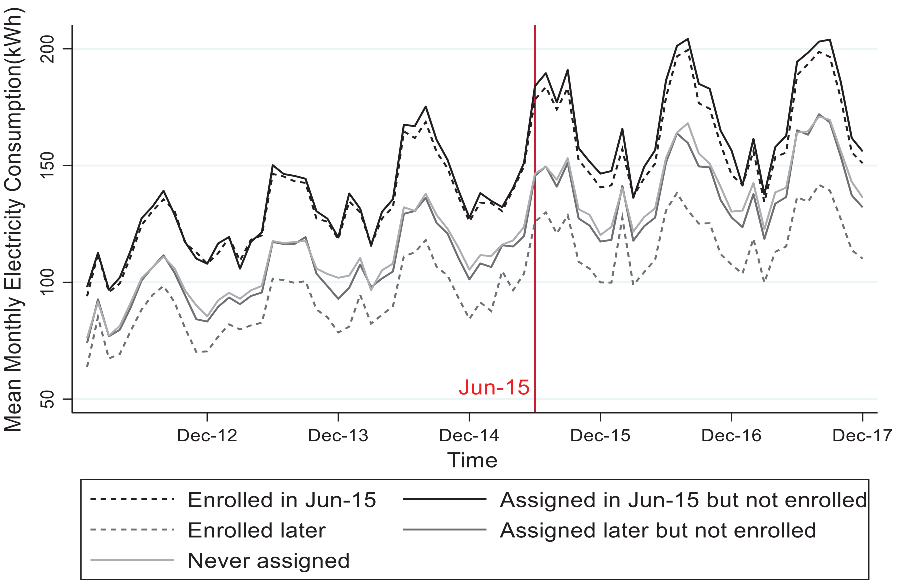
Notes: The red vertical axis shows the specified enrollment date (i.e., when the program was rolled out). For example, the enrollment date in part A of this figure is August 2013. The black dashed line represents mean monthly electricity consumption in "assigned" districts that rolled out the program at the specified date. The black solid line shows mean monthly electricity consumption in assigned districts that rolled out the program after the specified date. The gray solid line presents electricity consumption in "unassigned" districts that did not have the program during the entire sample period.

Figure C.3: Comparing Electricity Consumption Between Enrolled and Not-Enrolled Households

A. Program started in August 2013



B. Program started in June 2015



Notes: The red vertical axis shows the specified enrollment date. For example, the specified enrollment date in part A of this figure is August 2013. The black dashed line represents consumption of enrolled households at a specified date. The black solid line shows consumption of the assigned but not enrolled households who chose not to enroll when the program was available in their district at the specified date. The darker gray dashed line represents consumption of households who were not assigned by the specified date but later were assigned and enrolled. The darker gray solid line shows consumptions of households who were not assigned by the specified date but later were assigned and did not choose to enroll. The light gray solid line presents electricity consumption of households who were never assigned to treatment or their district never had the program during the entire sample period.

Table C.1: Robustness Check for the ITT Effect on Electricity Consumption

	(1)	(2)	(3)
TreatD×Post	-.046**	-0.047**	-0.048***
	(0.019)	(0.020)	(0.016)
Controls	No	Yes	Yes
Fixed Effects	Yes	Yes	Yes
Observation	45,565,601	45,565,601	45,565,601
Adj R-squared	0.711	0.711	0.711

Notes: This table reports estimates of the intent-to-treat effect of the rebate program. The dependent variable is the inverse hyperbolic sin transformation of monthly electricity consumption. All regressions include household and time-fixed effects. Column (1) do not include any other control variables. Column (2) controls for monthly district-level weather variables, and Column (3) includes both weather variables and annual provincial control variables. Standard errors (in parentheses) are clustered by district. *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

Table C.2: Robustness Check for the ATT Effect on Electricity Consumption

	(1)	(2)	(3)
Instrumental Variables:			
TreatH×Post	-0.178**	-0.180**	-0.179***
	(0.079)	(0.080)	(0.065)
First-stage Regression			
TreatD×Post	0.259***	0.260***	0.268***
	(0.049)	(0.049)	(0.050)
Ordinary Least Squares:			
TreatH×Post	-0.046**	-0.049***	-0.049**
	(0.019)	(0.019)	(0.017)
Controls	No	Yes	Yes
Fixed Effects	Yes	Yes	Yes
Clusters	27	27	27
Observations	45,565,601	45,565,601	45,565,601
Kleibergen-Paap Wald rk F	28.13	28.24	27.82
Adj R-squared	0.710	0.711	0.711

Notes: This table reports and compares regression results from Ordinary Least Squares (OLS) and Instrumental Variable (IV) approaches. The dependent variable is the inverse hyperbolic sin transformation of monthly electricity consumption. All regressions include household and time-fixed effects. Column (1) do not include any other control variables. Column (2) controls for monthly district-level weather variables, and Column (3) includes both weather variables and annual provincial control variables. Standard errors (in parentheses) are clustered by district. *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10%

Table C.3: Robustness Check for the Heterogeneity of the ITT Effect

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
TreatH×Post	-0.2747*** (0.092)	0.2192*** (0.159)	-0.2742*** (0.092)	-0.2801*** (0.096)	-0.287*** (0.096)	0.1311 (0.108)	0.6909*** (0.249)
TreatH×Post×Temp	0.0721*** (0.026)	0.0816*** (0.026)	0.0729*** (0.025)	0.0719*** (0.026)	0.0719*** (0.026)	0.0734*** (0.026)	0.0814*** (0.027)
TreatH×Post×Baseline		-0.1104*** (0.036)					-0.110*** (0.036)
TreatH×Post×Threshold			-0.022 (0.028)				-0.0007 (0.024)
TreatH×Post×Prize				0.0009 (0.001)			-0.0006 (0.001)
TreatH×Post×Program Length					0.0489 (0.039)		0.045* (0.024)
TreatH×Post×Electricity Price						-0.5149*** (0.168)	-0.6125*** (0.185)
Observation	45,565,601	45,534,655	45,565,601	45,565,601	45,565,601	45,565,601	45,534,655
Adj R-squared	0.711	0.712	0.711	0.711	0.711	0.711	0.713

Notes: This table reports the heterogeneity of the program's intent-to-treat effect. The dependent variable is the inverse hyperbolic sin transformation of electricity consumption. All regressions include household and time-fixed effects. Standard errors (in parentheses) are clustered by district.
*** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.

Table C.4: Robustness Check for the Heterogeneity of ATT Effect

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
TreatH×Post	-1.2014**	1.2105	-1.2024**	-1.1880**	-1.289**	0.7607	3.6700**
	(0.457)	(1.106)	(0.462)	(0.440)	(0.493)	(0.586)	(1.668)
TreatH×Post×Temp	0.3223**	0.3332**	0.3147**	0.3225**	0.3092**	0.3234**	0.3212**
	(0.127)	(0.130)	(0.123)	(0.128)	(0.127)	(0.129)	(0.133)
TreatH×Post×Baseline		-0.5099*					-0.5098*
		(0.264)					(0.258)
TreatH×Post×Threshold			0.082				-0.0049
			(0.121)				(0.083)
TreatH×Post×Prize				-0.0025			-0.0038
				(0.006)			(0.004)
TreatH×Post×Program Length					0.262		0.287*
					(0.166)		(0.146)
TreatH×Post×Electricity Price						-2.459**	-3.178**
						(1.012)	(1.156)
Observations	45,565,601	45,534,655	45,565,601	45,565,601	45,565,601	45,565,601	45,534,655
Kleibergen-Paap Wald rk F	13.563	13.21	15.016	13.57	13.86	10.43	14.17

Notes: This table reports IV estimates of the program's heterogeneous effects. The dependent variable is the inverse hyperbolic sin transformation of electricity consumption. All regressions include household and time-fixed effects. Standard errors (in parentheses) are clustered by district. *** Significant at the 1% level. ** Significant at the 5% level. * Significant at the 10% level.