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Using Price Incentives to Bound Welfare from Pay-as-You-Go Solar Electricity

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Abstract

Non-price barriers suppress demand at the adoption margin, complicating efforts to quantify the welfare effects of rural electrification. This paper studies demand for pay-as-you-go (PAYGo) solar electricity using a randomized experiment with 800 existing PAYGo customers in Kenya and Rwanda post-adoption. The experiment randomly assigns incentives that lower the effective price of usage for consumers who meet monthly purchase thresholds. Although average demand is unchanged, consumers with the highest pre-experimental demand increase their purchases by 6–7% in response to the incentive. These responses are used to estimate a lower bound on consumer surplus from PAYGo solar. The study finds large gains for high-demand consumers, but benefits deteriorate substantially for low-demand consumers. Combining these estimates with evidence from the literature on the environmental externalities of solar home systems, the marginal value of public funds of PAYGo solar subsidies is at most 1.7 in Kenya and 2 in Rwanda.

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1 Introduction

Providing universal access to electricity requires electrifying increasingly remote and low-income households. However, these households typically have low demand for electricity (e.g., Lee, Miguel, and Wolfram 2020, Grimm, Lenz, et al. 2020, Burgess et al. 2025). It is unclear whether this low observed demand for electricity accurately reflects the welfare consumers gain from electrification. For instance, credit constraints could bind for high upfront connection costs or appliance purchases, depressing demand even if consumers have a high value for electrification. Consumers may also face uncertainty about low power quality and poor ongoing maintenance, or simply face bureaucratic hurdles to connecting. All of these non-price factors shape observed demand, making it difficult to accurately assess the welfare effects of rural electrification. Understanding the true value consumers place on electricity is critical for determining whether the high costs of rural electrification are a justifiable and efficient use of public resources.

Pay as you go (PAYGo) solar home systems address many of the barriers that prevent households from adopting electricity. PAYGo contracts spread out payments over time. Consumers make a small down payment to have a solar home system (SHS) installed, which includes solar panels and all of the basic appliances consumers can use with the system. Consumers pay off the system by purchasing access time: time when they can use the electricity generated by the system. When a consumer's access time runs out, the solar company remotely locks them out of the system until they purchase more. Remote lockout provides a low-cost enforcement mechanism that enables off-grid companies to contract with rural, potentially low-income consumers. The contract also includes after-sales service. Low population density in rural areas and limited demand among low-income households make basic solar home systems the least-cost option for many unelectrified areas (International Energy Agency 2017). Such systems do not incur the costs of expanding the grid and are sufficient to provide the limited energy services that many low-income households demand in the short- to medium-term. Studying demand for electricity using the ongoing payments for PAYGo solar home systems therefore reduces many concerns about using observed demand to estimate welfare from electrification.

I bound demand for PAYGo solar using incentives offered by a solar company to 800 randomly selected, existing customers in Kenya and Rwanda. The incentives change the effective price paid for access time by giving customers free days at the end of each month if their total monthly purchases are high enough. By randomizing the amount of free time provided, I observe consumer responses to different effective prices, essentially tracing out a demand curve. I can therefore use estimated average treatment effects and counterfactual demand in the control group to estimate bounds on consumer surplus from electricity.

On average, consumers in Kenya and Rwanda do not significantly increase demand in response to the incentive. However, consumers with the highest pre-experimental demand in both countries modestly increase purchases by 6-7%. To use consumer responses to the incentives to bound consumer surplus from PAYGo solar, I need to ensure that they reflect consumer responses to price changes rather than other attributes of the incentives. I use survey data to demonstrate that liquidity constraints are not driving my results. Detailed usage data show that although consumers engage in minor amounts of intertemporal substitution, the magnitudes are not large enough to explain small responses to the incentives. I cannot rule out that consumers may respond to the information contained in the incentives, but I show that such responses would bias my estimates of consumer surplus downward. Finally, I show that the empirical patterns in treatment effects between districts with a high proportion versus a low proportion of treated consumers are not consistent with spillovers between households. I thus argue that consumer responses to the incentives are a credible revealed preference measure that I can use to derive a lower bound on consumer surplus.

My results suggest that high-demand consumers have relatively inelastic demand for electricity. In Kenya, my lower bound on consumer surplus indicates that high-demand consumers benefit by at least $19 per month. PAYGo solar is relatively more expensive in Rwanda, so my lower bound of consumer surplus for high-demand consumers is just $4. However, demand drops steeply for consumers with moderate to low usage. This results in my lower bound on *average* consumer surplus falling in line with existing estimates in both countries, but my estimates of consumer surplus for high-demand consumers being substantially higher.

Although I lack experimental variation to estimate producer surplus, I calculate ranges of producer surplus under different assumptions about the amount of access time the average consumer would need to purchase for the firm to break even. My most conservative estimates imply negative producer surplus over the life of a PAYGo contract of up to $29 in Kenya and $34 in Rwanda.

Even accounting for negative producer surplus and using conservative lower bounds on consumer surplus, I find that PAYGo solar increases social welfare by over $300 per SHS in Kenya and $40 per SHS in Rwanda.

Despite being more affordable than grid expansion, PAYGo solar home systems remain expensive relative to traditional energy sources. Consumers at the 75th percentile of energy expenditures in rural Rwanda spend just RWF 200 per week on energy services that could be replaced by a basic solar home system, while purchasing a week of PAYGo access time costs roughly RWF 900.[1] The gap is smaller but still substantial in rural Kenya: around KES 110 per week for traditional energy sources relative to KES 140 for a week of access time. Such gaps in affordability have prompted governments throughout Africa to consider subsidizing solar home systems.[2] I therefore consider what my results imply about the value of subsidies for PAYGo solar. I calculate the marginal value of public funds (MVPF) for a PAYGo solar subsidy using my lower bounds on consumer surplus, estimates from the literature on the externalities from solar home systems, my back of the envelope calculation of producer surplus, and information about tax rates and spending on alternative energy sources (Hendren and Sprung-Keyser 2020, Hahn et al. 2024). I find that the MVPF is at most 1.7 in Kenya and 2 in Rwanda, but accounting for negative producer surplus brings it down to 0.8 in Kenya and 0.6 in Rwanda. These estimates imply that the MVPF for solar subsidies fall somewhat below other redistributive policies in both countries.

My results contribute evidence on the welfare impacts of rural electrification. Much of the literature on electricity access has focused on estimating the impacts of electrification on a range of economic outcomes, but results have been heavily mixed. Grimm, Munyehirwe, et al. (2016) and Lenz et al. (2017) both find some positive impacts from electrification in Rwanda in the form of reduced energy expenditures, health improvements, and reduced use of dry cell batteries, but Bensch, Kluve, and Peters (2011) find no positive impacts on income or energy expenditures. In Tanzania, Chaplin et al. (2017) find that electrification increases income generating activities and per capita consumption but has negative impacts on health. Conversely, Lipscomb, A. Mushfiq Mobarak,

[1] Although part of this gap is due to the appliance financing included in a PAYGo contract, it is worth noting that the appliances included in a basic solar home system typically only include light bulbs, phone chargers, and radios or torches. Apart from light bulbs, many non-electrified households pay for disposable radio/torch batteries and phone charging services.

[2] See for instance Togo (Mumbere 2019), Nigeria (Africa Oil+Gas Report 2025), Kenya (Energy 4 Impact 2025), and Rwanda (Rwanda Ministry of Infrastructure 2020). Note that subsidies in Kenya and Rwanda did not start until after my experimental period.

and Barham (2013) and Dinkelman (2011) both find that electrification leads to improvements in economic development in Brazil and South Africa, respectively. Walle et al. (2015) and Khandker et al. (2014) find positive impacts of electrification on consumption and Burlig and Preonas (2024) document positive impacts for large enough villages in India, but no impacts for smaller villages. Parsing these results is difficult due to differences in settings, the outcomes being measured, and different timescales between electrification and measurement.

Revealed preference measures of consumer surplus address many concerns about approaches that use selected economic outcomes as proxies for welfare impacts. Grimm, Lenz, et al. (2020), Lee, Miguel, and Wolfram (2020), and Burgess et al. (2025) use price variation to derive revealed preference estimates of the welfare impacts of electrification. In general, they find that consumer demand for rural electrification is low and relatively elastic, implying low impacts on consumer surplus. However, all three examine electricity adoption, which may be shaped by multiple non-price factors that complicate interpretations for welfare. My study addresses these concerns because I study demand post-adoption, when consumers are using and enjoying the benefits of electrification rather than simply anticipating them. As such, consumers have already learned about the benefits and reliability of the technology. Although consumers in my sample make a small down payment to adopt a solar home system, the variation I study comes from ongoing payments for use. These payments are extremely small relative to the adoption costs studied in much of the literature, addressing concerns about credit constraints. My setting is also one where consumers enjoy after-sales support for ongoing maintenance and where appliances are included with the system. I therefore contribute a lower bound on the consumer surplus benefits of rural electrification by leveraging variation on the usage rather than adoption margin and by studying a context that reduces concerns about other non-price factors that may complicate measurement of consumer surplus.

Finally, I make a modest contribution by documenting patterns in consumers' use of electricity, providing novel, granular insights into the energy consumption behavior of low-income households. I show that consumers engage in limited intertemporal substitution of electricity even when they have rechargeable appliances. This has implications for discussions of reliability. Consumers in my setting receive multiple SMS reminders about impending remote lockouts but still do not engage in substantial intertemporal substitution. This indicates that there is limited scope for intertemporal substitution among consumers with low demand for electricity, implying that unpredictable outages likely have significant welfare costs even for households using small quantities of electricity. My findings align with those in Cissé (2025), who finds substantial increases in willingness to pay for more reliable electricity, as well as Alberini, Steinbuks, and Timilsina (2020) and Gertler, Lee, and A Mushfiq Mobarak (2017).

Designing effective and efficient policies to achieve universal electrification hinges on a clear understanding of consumer demand. By presenting novel experimental evidence from the usage margin of PAYGo demand, I find that the welfare gains from PAYGo solar are substantial but the marginal value of public funds to expand access further is relatively low. Such evidence provides a key parameter for calibrating subsidies and offers a new perspective on the value of basic, modern energy for rural, low-income households.

2 Background

The PAYGo systems I study include solar panels, a battery for storing power, and all of the appliances to be used with the system except mobile phones.[3] The smallest solar home systems include three light bulbs, a phone charger, and the choice of an additional light bulb, a rechargeable torch, or a rechargeable radio. Consumers can include additional appliances for a higher price. The batteries included in the system are large enough to make the systems a reliable source of electricity in spite of their relatively small generation capacity.

Consumers make a small down payment to have the solar home system installed. The solar company adds up the total value of the system and the appliances plus a flat fee, then converts the total value of the contract into a daily price for solar access. Once a household has a solar home system installed, the consumer prepays for solar access time using mobile money. Access time is simply time when the consumer can use the electricity being generated by the solar home system or stored in the battery.

Access time runs down continuously, regardless of system use. Consumers cannot opt to turn their system off and save access time for later use, transfer money out of their solar account, or transfer access time to other solar consumers. When a consumer runs out of access time, the solar

[3]Mobile phones can be charged using the solar home system, but the phones are not typically included in the package of appliances that the consumer purchases from the solar company.

company remotely switches off their system until they make another payment. When the system is switched off, the consumer cannot access any of the electricity being generated by the system or electricity stored in the battery. Prolonged periods of non-payment lead to repossession. In general, consumers can purchase access time in any increment.[4]

On average, consumers use fifty watt hours (wH) on days when they have purchased access to their solar home systems in Kenya and forty-five wH in Rwanda.[5] In Kenya, the median purchase size among the sample of consumers in my experiment is six days of access time, in Rwanda it is seven days. Nearly half of consumers in both countries purchase solar access on 95% or more of days prior to the experiment. Around 20% purchase access for 85%-95% of days, and the remaining 30% purchase less than 85% of days.

Data from a phone survey of PAYGo consumers in Rwanda suggests that PAYGo consumers positively self-select on wealth (see Figure A1). Thus, although PAYGo contracts make solar home systems more financially accessible than they would be if consumers had to buy them outright, they still serve a relatively wealthy segment of the rural population. This is consistent with the cost of PAYGo access time compared to traditional energy sources, and aligns with evidence from Benin (Barry and Creti 2020) and East Africa (Collings and Munyehirwe 2016, Muchunku et al. 2018, Groenewoudt, Romijn, and Alkemade 2020). Energy services replaceable by a solar home system comprise just 0.7% of total expenditures for the median rural household without electricity access in Rwanda. Paying for just 5 days a week of access time for the most basic solar home system would comprise nearly 11% of total expenditures (National Institute of Statistics of Rwanda December 2017).

Selection may be less extreme in Kenya, although I lack analogous phone survey data to test this directly. However, replaceable expenditures comprise just 0.5% of total expenditures for the median rural, unconnected household, and purchasing 5 days a week of access time would only increase the share of expenditures to 3% (Kenya National Bureau of Statistics 2016).

At the time of my experiment, the governments of Kenya and Rwanda were not directly subsidizing solar home systems. My results therefore speak to demand among consumers able to

[4]The one exception to incremental purchases is that once consumers' systems are switched off, they need to purchase at least one week to have the system switched back on.

[5]Appliances used with the solar home system are efficient, so consumer benefit from fifty wH more than would be expected were they to use less efficient appliances.

purchase solar home systems at market prices. Lower-usage consumers in my sample provide a rough sense of how demand may change for consumers on the margins of electrification, who may be induced to adopt a solar home system under a subsidy regime.

3 Experimental Design

I collaborated with the solar company to offer incentives for access time purchases to randomly selected customers for 7–8 months in 2018–2019. My core analysis concerns incentives structured as a monthly reward. If consumers bought at least four weeks over the course of a month, they would receive either 1 or 2 days of access time for free, with the bonus level being determined randomly.[6] One concern about the treatment is that the incentives may simply provide information about how to remain in good standing with the solar company. For consumers who are uncertain about the risk of repossession, the incentives may change behavior purely through this information channel rather than a price channel. To address such concerns, the solar company also implemented a simple information treatment where randomly selected consumers received a phone call from the solar company informing them how many days a month they need to purchase to be considered a "good customer".

In both countries, pre-experimental mean and median demand were around 20–21 days.[7] Consumers faced a schedule of incentives, with incentives increasing in the number of weeks purchased (see Figure A3). In practice, few consumers qualified for anything above the minimum number of bonus days.

In total, the company offered 400 consumers the monthly reward in both Rwanda and Kenya. It implemented the information treatment with an additional 400 consumers. My control group includes 5198 randomly selected consumers in Rwanda and 3800 randomly selected consumers in Kenya. I stratified the sample based on a consumers' utilization rate at the start of the experiment: the proportion of days a consumer has had access to their solar home system. I created five strata based on pre-experimental demand: 65% and below, 65%–75%, 75%–85%, 85%–95%, and

[6] The solar company simultaneously piloted other incentives with higher qualifying thresholds and incentives structured as bulk discounts on individual purchases of access time. These incentives are not informative for the consumer surplus exercise. For completeness, I present the pre-registered results on all incentive types in Appendix E.

[7] Figure A2 shows the distribution of mean days of access time purchased in a month during the 4 months prior to the experiment in Rwanda and during the 5 months prior to the experiment in Kenya.

above 95%. In both countries, the stratification ensures that I oversample consumers with low pre-experimental demand. However, since I stratified my entire sample, consumers in each stratum face the same probability of treatment. Random assignment generally yields balance along a number of pre-experimental covariates, though there is slight imbalance in TV ownership in both countries, in the daily rate in Rwanda, and in pre-experimental demand in Kenya (see Table A1 and Table A2). Given that my primary specification uses panel data with consumer-level fixed effects, imbalance on these covariates should not bias my results.

A representative from the solar company called each consumer who had been selected for either the incentives or the information treatment in the two weeks prior to the start of the experiment. Once the experiment started, consumers in the incentive treatment received weekly SMS messages with detailed reminders about the incentives. The experiment ran from July, 2018 through February, 2019 in Rwanda and from August, 2018 through February, 2019 in Kenya.[8]

3.1 Data

Throughout my analysis I primarily rely on administrative data obtained from the solar company to examine consumer responses to the incentives and information. The administrative data start in March, 2018 and continue through the end of the experiment in February, 2019, providing a 4-month pre-period in Rwanda and a 5-month pre-period in Kenya. I observe all payments for solar access time, bonus days earned, the pre-experimental utilization rate, the daily rate, daily totals of watt hours used, and the appliances included in each consumer's solar home system.

For descriptive purposes, I also leverage a phone survey conducted after the end of the experiment with a random subset of consumers in Rwanda in March 2019. The phone survey was conducted by an independent team of enumerators, who called 800 respondents in the control group and all treated consumers. Response rates were low at just 61.86%.[9] Given the relatively low response rates, I interpret results using the phone survey data with caution.

[8]Consumers could still earn the bonus in February even though there were only 28 days: the bonus days were simply added to their account to use in March. I show in Table A3 that average treatment effects are robust to excluding February.

[9]Enumerators reported that it was more difficult to reach consumers in more remote areas where the cellular network is less reliable. I also see that response rates are increasing in pre-experimental demand, with consumers in the 95%–100% stratum being 6.9pp more likely to respond to the phone survey than consumers in the 0%–65% stratum.

3.2 Empirical Strategy

I estimate reduced form average treatment effects using the specification

$$DaysPurchased_{it} = \alpha + \beta_1 Incentive_{it} + \beta_2 Info_{it} + \gamma_t + \gamma_i + \epsilon_{it}. \tag{1}$$

$DaysPurchased_{it}$ is the number of days of access time purchased by consumer i in month t, not inclusive of bonus days. $Incentive_{it}$ is a dummy variable equal to one if consumer i in month t was assigned to the monthly reward treatment and $Info_{it}$ is a dummy variable equal to one if they were assigned to the information treatment. Note that both treatment indicators are zero during the pre-period, switch on in the first month of treatment, and remain switched on during all subsequent months of the experiment (i.e., treatment never turns "off" for any consumer until the experiment concludes). γ_t is a month fixed effect and γ_i is a consumer fixed effect. Although not necessary for identification, γ_i and γ_t allow me to leverage the pre-period to increase the precision of my estimates.[10] I further estimate heterogeneous average treatment effects by strata using the specification

$$DaysPurchased_{it} = \alpha + \sum_j \beta_{1j} Incentives_{it} \times S_{ij} + \sum_j \beta_{1j} Info_{it} \times S_{ij} + \gamma_t + \gamma_i + \epsilon_{it}, \tag{2}$$

where j indexes strata and S_{ij} are strata dummies.

Average treatment effects measure the effect of each treatment on the amount of access time purchased. I can further leverage the structure of the incentives to learn more about consumer demand for solar with a few additional assumptions. In the next section, I lay out a framework that uses the structure of the monthly reward and the randomized bonus level to bound price elasticities and, by extension, consumer surplus from PAYGo solar electricity.

4 Bounding consumer surplus

Assume there are three types of consumers. The first type does not respond to the incentive at any bonus level offered in the experiment, so I call consumers of this type "non-responders". The

[10] Note that my results are similar if I use strata by month fixed effects since the proportional size of the strata are the same in the treatment and control groups.

second type only responds to the high bonus, but does not respond to the low bonus. I call this type "high-value responders." The third type responds to the low bonus, so I call these consumers "low-value responders." In this section, I show that I can calculate a lower bound on the price elasticity of demand for non-responders and high-value responders. For low-value responders, I can calculate an upper bound.

To lay out my framework, I consider consumers in the highest stratum in Kenya. Counterfactual demand for these consumers during the months of the experiment is 25.32 days per month, the mean demand in the control group. Panel A of Figure 1 illustrates the consumer's problem when offered an incentive that gives two days free if the consumer buys 28 days in a month. The consumer faces a normalized marginal price of 1 for every day that they buy up to the twenty-eighth day. The marginal price of days 29 and 30 goes to zero, then jumps back up to one for 31 days. Demand is 25.32 days absent the incentive. Therefore, consumers' willingness to pay for more than 25.32 days must be below the normalized price of one. If the consumer chooses to increase the number of days purchased to 28 to qualify for the incentive, they lose consumer surplus equal to area A but gain consumer surplus equal to area B. I denote the consumer's willingness to pay for 28 and 30 days absent the incentive as P_{28} and P_{30}, respectively, and counterfactual demand as Q_c.

Non-responders do not change their behavior even when offered a high bonus. It follows that area $A > B$ for non-responders, or, assuming linear demand, [11]

$$14 - \frac{Q_c}{2} > P_{30} + P_{28}(15 - \frac{Q_c}{2}). \tag{3}$$

I can write the slope of the demand curve s in multiple ways:

$$s = \frac{P_{30} - 1}{30 - Q_c} = \frac{P_{28} - 1}{28 - Q_c}.$$

Re-arranging, I come to the expression

$$(15 - \frac{Q_c}{2})P_{28} = (14 - \frac{Q_c}{2})P_{30} + 1. \tag{4}$$

[11] See Appendix B for a more detailed explanation.

Substituting Equation 4 into Equation 3, the consumer does not respond to the incentive if

$$14 - \frac{Q_c}{2} > (15 - \frac{Q_c}{2})P_{30} + 1, \text{ or}$$

$$\frac{13 - \frac{Q_c}{2}}{15 - \frac{Q_c}{2}} > P_{30}. \tag{5}$$

The expression in Equation 5 indicates that consumers who are willing to pay exactly $\frac{13-\frac{Q_c}{2}}{15-\frac{Q_c}{2}}$ for the thirtieth day of solar access would be indifferent between buying twenty-eight days to qualify for the incentive or staying with counterfactual demand Q_c. When $P_{30} = \frac{13-\frac{Q_c}{2}}{15-\frac{Q_c}{2}}$, it implies that the price elasticity at the counterfactual point of demand that I observe is

$$\epsilon_{NR} = \frac{450 - 30Q_c + \frac{Q_c^2}{2}}{-2}\frac{1}{Q_c}.$$

Given the direction of the inequality in Equation 5, ϵ_{NR} is a lower bound on the price elasticity of demand for solar electricity among non-responders: their demand may be less elastic than ϵ_{NR} but is not more elastic. Similarly, the linear slope of the demand curve that I calculate is an upper bound for non-responders. Assuming that the demand curve is weakly convex everywhere, using a linear demand curve to estimate consumer surplus is conservative.

Next, I consider low-value responders, illustrated in Panel B of Figure 1. These are the consumers who respond even when only offered the low bonus, indicating that the consumer surplus they gain from receiving one free day is greater than the consumer surplus they lose by increasing their purchases to qualify for the incentive. I can follow the same process to solve for the price at which low-value responders are indifferent between increasing their purchases to earn the bonus or not, then solve for an upper bound on the slope, which I denote s_{LVR} and a lower bound on the price elasticity denoted ϵ_{LVR}.

Finally, I consider high-value responders who do not respond when offered the low bonus but do respond when offered the high bonus, illustrated in Panel C of Figure 1. These consumers must have a price elasticity that falls below that of non-responders but above that of low-value responders:

$$\epsilon_{LVR} \leq \epsilon_{HVR} \leq \epsilon_{NR}.$$

Similarly, high-value responders must have consumer surplus that is less than that of non-responders but more than that of low-value responders.

The final step in bounding consumer surplus is estimating the proportion of consumers who are non-responders, high-value responders, and low-value responders. I estimate the average treatment effect for the incentive with a 1-day bonus among consumers in each strata. I assume that consumers do not change their behavior unless they qualify for the incentive.[12] It follows that the proportion of low-value responders is

$$p_{LVR} = \frac{ATE_{LowBonus}}{28 - Q_c}.$$

In other words, the average treatment effect for the 1-day bonus scaled by the number of days required for the average consumer to reach the qualifying threshold relative to counterfactual demand. Similarly, I calculate the proportion of high-value responders by estimating the average treatment effect of the incentive with a 2-day bonus, rescaling it by the implied change in demand, and removing the proportion of low-value responders:

$$p_{HVR} = \frac{ATE_{HighBonus}}{28 - Q_c} - p_{LVR}.$$

It follows that the proportion of non-responders is

$$p_{NR} = 1 - p_{LVR} - p_{HVR}.$$ [13]

I first present reduced-form results on the impact of the incentives and information on demand for solar access time. I then consider non-price factors that may influence consumer responses to the incentives in a manner that could bias my estimated bounds. Finally, I use consumer responses to bound effects from PAYGo solar on consumer surplus in each country and combine these estimates with back of the envelope calculations on costs and externalities to bound welfare.

[12] It is possible that consumers partially respond to the incentives in practice (i.e., increasing demand by a small amount but not enough to reach the incentive after uncertainty about need for electricity gets resolved over the course of the month). If so, the ATEs I estimate for the low and high bonus will be biased upward relative to a world in which consumers only change demand if they qualify for the incentive. This will result in my bounds being overly conservative, as I show for the analogous case of responses to information in appendix subsection B.2.

[13] In practice some estimated average treatment effects are negative, possibly due to a discouragement effect for low-demand consumers. I assume that the price mechanism of the incentives cannot cause a reduction in demand, so I replace such coefficients with zero when estimating the proportion of low-value, high-value, and non-responders.

5 Results

Columns (1) and (3) of Table 1 show average treatment effects pooling across randomized incentive levels and strata. In both countries, neither treatment causes significant changes in demand. I can reject that incentives increase demand by one or more days per month in either country, 5-6% of the control group mean. For the information treatment, I can reject that the average effect is 1.5 days or more. Columns (2) and (4) continue pooling across strata but break out effects for each incentive level. Again, I cannot reject that either incentive level leads to significant changes in demand relative to the control group. Effects are not significantly different between the two incentive levels.

The null effects in Table 1 may mask heterogeneity: it is possible that only consumers with counterfactual demand near the threshold will respond to the incentive. I estimate heterogeneous treatment effects by stratum and incentive level, shown in Figure 2. Black diamonds show the level of demand among the control group during the months of the experiment. Black points show average treatment effects for the low incentive, dark grey points show effects for the high incentive, and light grey points show effects for the information treatment.

In Kenya, incentives only lead to a significant increase in demand among consumers in the top stratum. These consumers increase monthly purchases by 2 days on average, or nearly 8%, when offered the high incentive. When offered the low incentive, they increase monthly purchases by 1 day, though the increase is not statistically significant. These effects may be partially driven by the information implied in the incentive offer: I cannot reject that the incentives and information treatments have the same effect for consumers in the top stratum, and information leads to significantly higher demand than for consumers in the control group. Information does not significantly change demand for consumers in other strata, but the incentive appears to be discouraging for some lower-demand consumers, reducing demand. This is particularly true for the higher incentive, consistent with consumers feeling frustrated that a desirable bonus is out of reach.

I observe similar patterns in Rwanda. Incentives significantly increase demand only for consumers in the top stratum. These consumers increase days purchased in a month by 1.5 days on average when offered the high incentive and 1.9 days when offered the low incentive, a 6.5%–8.3% increase. As in Kenya, the incentive appears to have a discouraging effect for some lower-demand consumers, particularly for the high incentive, although both information and the low incentive increase demand in the lowest strata. Unlike in Kenya, information does not significantly increase demand among consumers in the top stratum, though I cannot reject that information has the same effect as incentives because the effect is estimated imprecisely.

Taken together, my reduced form results suggest that consumers who are closest to the incentive's threshold respond most strongly to the promise of a monthly reward, as expected. However, the incentive appears to discourage consumers who are far from the qualifying threshold, leading to significant reductions in demand among some consumers in lower strata in both countries. In translating my results into measures of consumer surplus from PAYGo solar electricity, I deal with discouragement effects by assuming that PAYGo solar is a normal good. This effectively imposes a floor of zero for any consumer responses to changes in *price*, setting aside the structure of the incentive.

Before turning to welfare, it is important to consider non-price factors that could shape consumer responses to the incentives relative to an equivalent per-unit change in price. First, incentives may require that consumers forego liquidity. I lack complete information on consumers' access to liquidity; however, in the March 2019 phone survey I ask whether consumers have ever borrowed to pay for solar. I use responses to this question to estimate heterogeneous treatment effects based on self-reported access to liquidity to pay for solar. I cannot reject that the overall average treatment effect for consumers reporting access to liquidity is zero (see Table A4). These results are in line with Lang (2025b), who randomizes access to liquidity among a similar population in Rwanda and finds few changes in demand.

Second, consumers may gain variable utility from solar access. For instance, if a consumer is away from home on market days then it may be optimal for them to forgo solar access on those days. The non-storeable nature of access time could lead to smaller consumer responses to the incentives than to an equivalent, per-day price discount.

I cannot directly observe the utility that consumers gain from having access to electricity, but I observe daily totals of watt hours used when systems are switched on.[14] I first perform a descriptive analysis to explore the importance of intertemporal substitution. Consumers may charge all of their

[14]Figure A4 shows the watt hours used by a random subset of consumers when their systems are switched on prior to the start of the experiment.

appliances if they know that their system will be switched off the following day as a way to store electricity, leading to a spike in electricity use. If consumers engage in significant inter-temporal substitution, it may make the incentive less appealing. I find minimal evidence of intertemporal substitution, equal to less than 2% of use (see Figure A7 in Appendix C).

I also examine heterogeneous treatment effects based on pre-experimental variance in use. Consumers with highly variable demand may not respond to the incentives because they anticipate days with little to no usage.[15] I find no evidence of this in Kenya. In Rwanda, consumers with highly variable demand respond to the incentives less than those with less variable demand, but the difference is only marginally significant (see Figure A8).

Third, I consider the implications of my results on the information treatment. If any of the observed increases in demand in response to the incentives are partially a response to information rather than a change in price, then I will overestimate the effect of a price reduction. Any effect from information will lead me to overestimate the proportion of low-value responders and high-value responders and to underestimate the proportion of non-responders.[16] Since non-responders have the least elastic demand, this results in my estimated lower bound on consumer surplus being overly conservative.

Finally, I assess whether my results could reflect spillovers between treated households. Since the randomization was at the household level, it is possible that some low-demand households reduce their own payments for solar and instead pay neighbors to use theirs, effectively helping their neighbor reach the qualifying threshold for bonus days by pooling their demand. Although this is possible in theory, in practice it is important to note that a limited number of energy services provided by the solar home system can be effectively pooled across households. People may charge rechargeable appliances outside their home, but they will not enjoy electric light in their home unless they purchase access time. Nevertheless, I evaluate spillovers empirically in Rwanda.[17]

The most granular information I have about customer locations in Rwanda is the district of residence. Only a small proportion of customers in each district are offered the incentive, ranging from 1.6%–8.7%, with a median of 5.3% (see Figure A5). There were only around 50,000 active

[15] In practice, I use the standard deviation in wH used rather than a binary indicator for using any wH since consumers may choose to still consume a small quantity of electricity on a day when they have already paid for access, even if they have virtually no need for electricity.

[16] I show mathematically how this upward bias will affect my bounds on consumer surplus in subsection B.2.

[17] I lack sufficiently granular data about customer locations in Kenya to perform the same analysis there.

customers with the solar company at the time of the experiment out of more than 2 million rural Rwandese households. Combined, these two facts suggest that it is unlikely that a large number of households in the experiment were engaging in inter-household electricity trading.

I assess spillovers by estimating treatment effects heterogeneously by whether a customer resides in a district with an above- versus below-median proportion of treated customers. If there are spillovers, I should observe that in districts with an above-median proportion of treated customers, effects are more negative for consumers with low pre-experimental demand and more positive for those with high pre-experimental demand, relative to consumers living in districts with a below-median proportion of treated consumers. The assumption is that the districts with a larger proportion of treated consumers are more likely to see households pooling demand. I find no significant differences in effects between above- versus below-median districts for the low incentive (see Figure A6, top panel). I do observe that consumers living in above-median districts in the second stratum have significantly lower treatment effects than those living in below-median districts, but effects are not commensurately higher for high-demand consumers (see Figure A6). I therefore conclude that spillovers are not a first-order concern in the interpretation of results.

Taken together, my results suggest that the incentive primarily influenced consumer behavior through the price mechanism modeled in section 4. I therefore proceed to estimate bounds on price elasticities and consumer surplus using the results of the experiment.

5.1 Welfare

Recall from section 4 that I arrive at a lower bound on consumer surplus by adding up the lower bounds for non-responders and high-value responders and assuming zero consumer surplus for low-value responders. I calculate a more generous estimate of consumer surplus by assuming that low-value responders and high-value responders both receive their upper bound of consumer surplus and that non-responders receive their lower bound. I repeat all of my calculations in Rwanda and Kenya for consumers in the 95%–100% stratum, the 85%–95% stratum, and for all consumers combined in the 0%–85% strata.[18]

Table 2 shows the results. In both Kenya and Rwanda, I can reject that the lower bound on the

[18] I combine the bottom three strata in part because they represent a much smaller fraction of all consumers than the top two strata, but also because bounds mechanically become increasingly uninformative as counterfactual demand moves further away from the qualifying threshold.

price elasticity of demand is equal to zero for all types of consumers at a 5% level of significance. In Kenya, I can reject that my lower bound on the price elasticity of demand is equal to one for all consumers in the top two strata. I estimate that high-demand consumers enjoy a minimum of \$19.46 in consumer surplus each month. This declines to \$3.66 per consumer per month for consumers in the 85%–95% stratum and only \$0.87 for consumers in the 0%–85% strata. Taking a weighted average, I estimate that consumer surplus in Kenya is at least \$10.65 per household per month. Under the slightly less conservative assumption of constant elasticity demand rather than linear demand, my lower bound on consumer surplus is \$13.09 per household per month (see Table A5 in subsection B.1).

Consumer surplus from solar is substantially lower in Rwanda. The difference between the two countries is due to more elastic demand in Rwanda as well as lower counterfactual demand for all strata, consistent with PAYGo solar being relatively more expensive than traditional fuels. I find that the lower bound on consumer surplus is \$3.85 per month in the top stratum, \$1.03 per month for consumers in the 85%–95% stratum, and \$0.24 for consumers in the 0%–85% strata. My weighted lower bound for Rwanda is \$1.94 per household per month. Using constant elasticity demand, it increases to \$5.52 per household per month (see Table A5).

Next, I compare my results to two studies of electricity adoption in Rwanda and Kenya. Extensive margin adoption decisions should fully reflect intensive margin demand (Dubin and McFadden 1984). In practice, demand on the adoption margin may differ from intensive margin demand due to credit constraints, incomplete information, uncertainty about service quality, and bureaucratic hurdles, among other factors.[19]

Grimm, Lenz, et al. (2020) study demand for solar home systems in Rwanda that are purchased upfront as entire units rather than on PAYGo contracts. I assume that the life of a solar home system is three years and that households have a discount rate of 15%. I thus compare the total amount that consumers in my sample would be willing to pay for the solar home system based on my bounds on monthly consumer surplus discounted to the time of adoption. My weighted average lower bound is \$179, similar to the *maximum* willingness to pay in Grimm, Lenz, et al. (2020) of \$176. My estimates imply that PAYGo consumers in the top strata would be willing to pay

[19] Note that although the daily payment builds equity in the system in addition to providing immediate access to electricity, in practice the lifespan of the systems is somewhat limited. Thus, I assume that households view payments for access time primarily as expenditures for current electricity rather than long-term asset accumulation.

at least \$256. It is important to note that Grimm, Lenz, et al. (2020) informed participants that the market price for a solar home system purchased outright was \$120–\$180, likely biasing their results downward. My results imply large gains in consumer surplus from solar home systems for consumers at the top of the distribution of demand.

In Kenya, I compare my estimates to those in Lee, Miguel, and Wolfram (2020), who randomize the price of grid access in rural communities. The technologies being studied are admittedly extremely different: grid connections have a much higher upfront cost than a PAYGo solar contract, but can also bear much larger loads. I follow Lee, Miguel, and Wolfram (2020) in assuming that grid connections have an asset life of 30 years and that households have a 15% discount rate. Given the limited capacity of solar home systems relative to the grid, I only consider their estimates for consumers with the lowest electricity consumption, and calculate the benefits for just the first three years of a grid connection to account for the potentially short lifespan of a solar home system. This ranges from \$34–\$101 at prevailing electricity prices in Lee, Miguel, and Wolfram (2020) compared to my weighted average lower bound of \$112, again implying larger gains in consumer surplus particularly for high-demand consumers.

I lack the detailed cost data required to precisely estimate producer surplus. However, I know the overall amount that must be repaid in the PAYGo solar contracts in each country: \$208.05 in Rwanda and \$317.55 in Kenya. I assume that the firm is profit maximizing in its choice of contract and prices, which would mean that the price I observe reflects the lending costs the company expects to bear from consumers who choose not to purchase access time every day as well as the anticipated costs of repossession among consumers who default on their contracts.[20] Based on qualitative conversations with the solar company, I assume that the firm can break even if the average utilization rate is 70% - 80%. Assuming that the firm has an annual discount rate of 15%, the break-even net present value of the contract is \$167.32–\$173.03 in Rwanda and \$255.38–\$264.10 in Kenya.

Figure 3 shows lower bounds on willingness to pay for the total PAYGo solar contract based on my estimates in Table 2 for consumers in the top, second-highest, and bottom three strata in each country. The horizontal grey lines show the break-even price for the firm under the 70% and 80%

[20]This assumption is somewhat strong, as many PAYGo companies at the time were still working to learn about the right prices and contracts to use.

utilization assumptions. Stars indicate the effective price that the firm receives from consumers in each stratum based on demand in the control group during the experiment where they fall below the lower bounds I estimate on demand. I assume a 15% annual discount rate. Producer surplus is negative for consumers in the lowest strata regardless of the break-even assumption made in both countries, and is negative for all consumers under the 80% break-even assumption in Rwanda. Taking a weighted average, producer surplus is -\$18.10 to -\$28.82 per customer in Kenya and -\$28.08 to -\$33.79 in Rwanda (see subsection B.3 for detailed calculations).

My estimated lower bounds on consumer surplus from PAYGo solar indicate larger gains for consumers than those estimated from extensive margin price variation. Even allowing for negative producer surplus, I do not find that expanding electrification using PAYGo solar home systems is welfare-reducing as Lee, Miguel, and Wolfram (2020) find for on-grid expansion in Kenya. The difference reflects both higher consumer surplus in my setting but also smaller losses in producer surplus given the low cost of SHS relative to the cost of a grid connection. Going beyond private consumer and producer surplus, Fetter and Phillips (2019) estimate that each solar home system in East Africa has annual benefits of \$13.70 in reduced carbon emissions (\$35.97 over three years with a 15% discount rate). Combining all of these estimates implies a lower bound on welfare gains of \$312–\$321 per SHS in Kenya and \$43–\$48 per SHS in Rwanda.[21]

Although my estimates indicate that PAYGo solar meaningfully improves welfare, they still indicate that sizable subsidies would be needed to expand the number of households adopting solar home systems. Extrapolating my lower bounds in Figure 3 implies that a Pigouvian subsidy covering the positive externalities from reduced carbon emissions would increase adoption by just 8% in Kenya and 20% in Rwanda. However, in this case my lower bounds on elasticities pose a limitation. My results show that demand is more elastic among consumers in lower strata compared to those in higher strata, so it is likely that extrapolating my estimates beyond the sample in question understates the benefits of a potential subsidy.

An alternative way to assess the public spending needed to increase access is the marginal value of public funds (MVPF) (Hendren and Sprung-Keyser 2020). Hahn et al. (2024) extend the

[21]My calculations assume that the price consumers face is the effective price consumers in each strata pay based on demand in the control group when it falls below my lower bound on WTP, and my lower bound on WTP when it does not. This leads to a weighted average consumer surplus of \$40 per SHS in Rwanda and \$303 per SHS in Kenya over the full course of the PAYGo contract. I combine these estimates with estimated externalities of \$36 and my producer surplus calculations to arrive at my lower bound on total welfare.

foundational MVPF framework for goods with environmental externalities. Using their framework and assuming perfect competition, the MVPF can be written as

$$\frac{1 + \frac{V}{p}(-\epsilon)}{1 + \frac{\tau}{p}(-\epsilon),}$$

where p is the unsubsidized price of the good, V is the marginal social benefit from consuming an additional unit of the good, τ is the marginal net cost to the government, and ϵ is the good's price elasticity. I use the lower bound on my elasticity of demand for the lowest strata (-2.69 in Kenya and -5.2 in Rwanda) and the value of reduced carbon emissions as the value V to assess the MVPF for expanding access to PAYGo solar. I set the price as the effective price that consumers in the lowest strata pay over the course of the PAYGo contract: \$237 in Kenya and \$151 in Rwanda.

Assessing the literature suggests that there are limited fiscal externalities from solar home systems. Rom, Günther, and Harrison (2017) finds no evidence of sustained increases in study time for children when households are provided with solar lighting, a finding confirmed by Stojanovski et al. (2021) in Zambia and Kudo, Shonchoy, and Takahashi (2014) in Bangladesh. Given that most solar home systems provide only basic energy services, increased educational attainment is one of the only channels that could lead to future growth. The other is averted climate damages to productivity; however, given the overall contribution of both countries to global carbon emissions and the relatively small reduction in emissions due to solar home systems, it is unlikely that these will lead to any meaningful fiscal externality. Apart from the direct cost of a \$1 subsidy, I assume that each government would incur around \$0.07 in administrative costs based on the findings in Aker et al. (2016).[22] I further calculate the change in expected tax revenues in each country by taking the difference between taxes paid on the solar home system and taxes paid on the alternative energy sources that the solar home system is replacing.[23]

Under these assumptions, I calculate that the MVPF is 1.7 in Kenya and 2 in Rwanda if I do not account for the negative producer surplus I find in my back of the envelope calculation. If I instead assume that the government would additionally need to compensate solar firms for the

[22] Seven percent is the estimated administrative cost for a subsidy delivered via mobile phones in Niger in Aker et al. (2016), which seems feasible in the PAYGo context since all customers make mobile solar payments.

[23] See subsection B.4 for details on the full MVPF calculation. Note that value added tax rates are the same for solar home systems and alternative energy sources in Kenya, so the change in tax revenues is positive because households tend to spend more on solar home systems than on alternative energy sources. In Rwanda, the change is negative because solar home systems are tax-exempt.

loss in producer surplus from taking on more low-demand customers, I arrive at an MVPF of 0.8 in Kenya and 0.6 in Rwanda.[24] For context, public spending on pre-primary education in low-income countries has an MVPF between 1.8 and 3.1 (World Bank 2022). Additional environmental or health externalities from solar home systems would need to be around \$7 to reach this range without accounting for negative producer surplus. Accounting for negative producer surplus, additional externalities would need to be around \$140 to be comparable to the MVPF for pre-primary education.[25] Thus, although solar home systems have positive welfare benefits, the value of public spending to increase access is small relative to alternative uses of public funds.

6 Conclusion

Consumer responses to price incentives for PAYGo solar access time are small in both Kenya and Rwanda, suggesting that consumer demand for electricity is relatively inelastic. I incorporate consumer responses to the incentives into a simple model of demand for electricity to estimate a lower bound on consumer surplus from solar. My lower bound suggests larger welfare gains from rural electrification than other estimates in the literature that leverage extensive margin price variation, particularly for high-demand consumers. Nevertheless, my results imply that the marginal value of public funds spent to further expand access to solar home systems is relatively low.

Many open questions remain about demand for electricity among rural, low income populations who comprise the majority of unelectrified households worldwide. Although my results speak to intensive margin price elasticities, my sample is limited to PAYGo adopters. We require elasticity estimates across the full income distribution to design optimal pathways for universal electrification. Relatedly, we will need estimates of extensive margin elasticities in a range of settings, as well as studies examining the interplay between intensive and extensive margin prices.[26] There is little evidence on income elasticities on both margins of demand, a critical input for designing well-targeted

[24] Note that using lower bounds on the price elasticity of demand means that my estimates of MVPF are lower bounds when less than one and upper bounds when greater than one.

[25] It is worth noting that households could also fail to internalize some of the private benefits from solar home systems. If so, consumer under-valuation of the benefits combined with additional externalities would need to reach \$140 to be comparable to the MVPF for pre-primary education.

[26] Lang (2025a) provides evidence on extensive margin responses to intensive margin price changes in Togo but lacks cross-cutting variation in extensive margin prices.

subsidies that can achieve higher cost-effectiveness than blanket approaches. Planning for universal electrification requires more detailed knowledge of the full distribution of demand, particularly among consumers whose incomes remain too low to currently adopt any form of electricity.

Finally, consumer decisions about electricity do not occur in a vacuum. Future work should consider how households make choices about electricity in the context of shifting prices for other types of energy services and other essential commodities as well as major productivity shocks. It should also consider longer time horizons, over which consumers may upgrade to larger systems that can support more appliances or switch to a grid connection as grid access continues to expand. Situating consumer demand for electricity in complex, real-world environments will help build policies that expand energy access in fiscally and environmentally sustainable ways.

During the preparation of this work the author used Google Gemini in order to debug code and check the paper for errors in spelling and grammar. After using this tool/service, the author reviewed and edited the content as needed and takes full responsibility for the content of the published article.

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7 Tables and figures

Table 1: Pooled Average Treatment Effects from Incentives and Information

	Dependent variable: Days Bought in a Month			
	Kenya		Rwanda	
	(1)	(2)	(3)	(4)
Information	0.401	0.401	0.202	0.202
	(0.46)	(0.46)	(0.569)	(0.569)
Incentive	-0.459		-0.117	
	(0.483)		(0.463)	
Low Incentive		0.322		0.026
		(0.61)		(0.595)
High Incentive		-1.241		-0.261
		(0.712)		(0.673)
Control Mean	19.445	19.445	16.392	16.392
Observations	43188	43188	71976	71976
Consumer FEs	X	X	X	X
Month FEs	X	X	X	X

Notes: Average treatment effects on the number of days purchased in a month for incentives and the information treatment, pooling across the sample stratification. Note that days purchased does not include bonus days earned from the incentive. I cluster all standard errors at the level of the consumer.

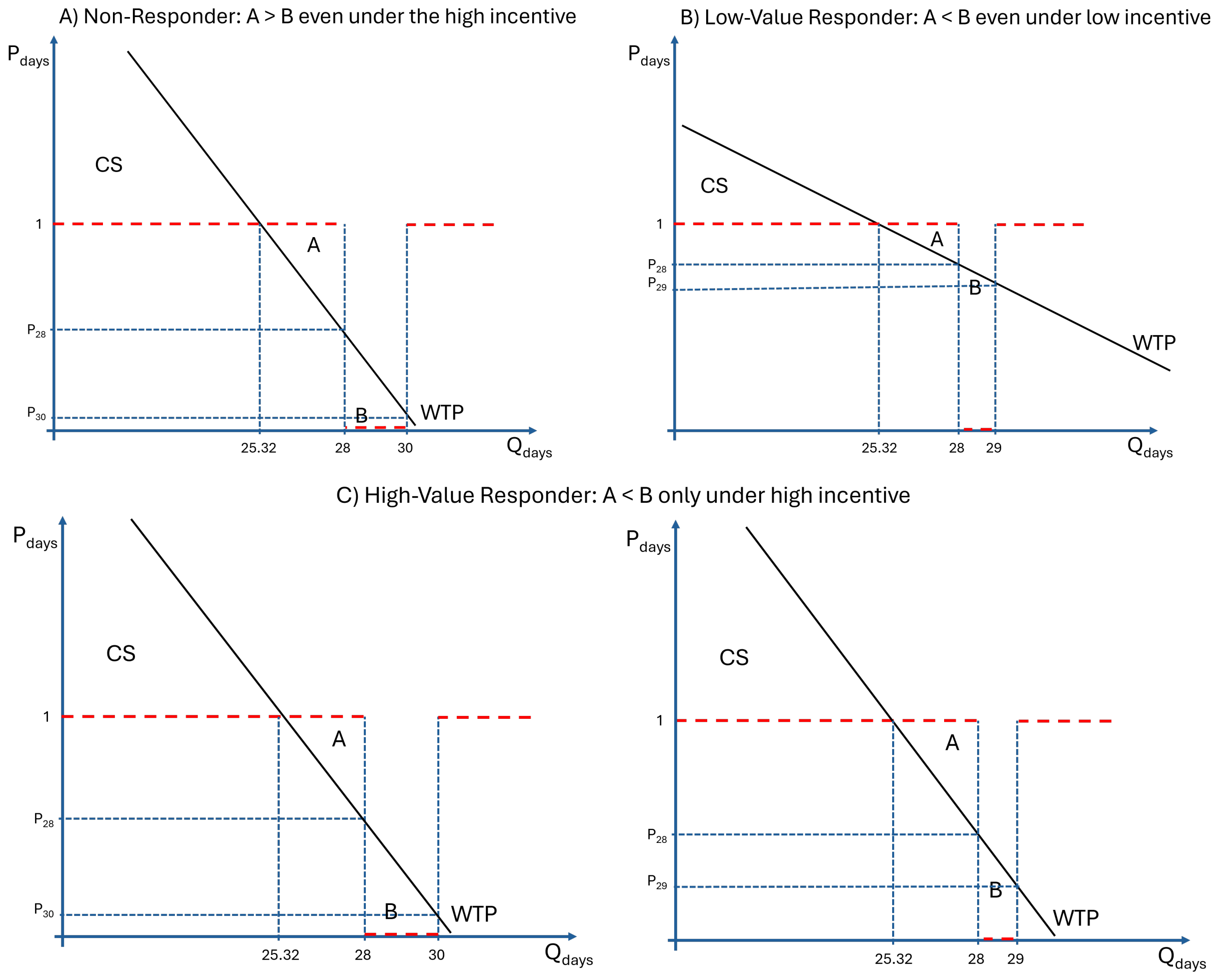
Table 2: Bounds on Elasticities and Consumer Surplus

Kenya									
Bin	Weight	ϵ_{NR} (1)	CS_{NR} (2)	$\%NR$ (3)	ϵ_{HVR} (4)	CS_{HVR} (5)	$\%HVR$ (6)	CS (LB) (7)	CS (8)
95%–100%	0.497	-0.18	20.85	0.57	-0.21	17.77	0.42	19.46	20.84
		(-0.23,-0.14)	(16.05,27.83)	(-0.44,1)	(-0.28,-0.15)	(12.84,25.77)	(-0.76,1.33)	(-0.42,25.88)	(14.8,27.39)
85%–95%	0.195	-0.82	3.81	1	-1.27	2.45	-0.06	3.66	3.73
		(-0.94,-0.71)	(3.23,4.48)	(0.98,1)	(-1.48,-1.08)	(2.05,2.94)	(-0.32,0)	(2.84,4.38)	(3.05,4.41)
0%–85%	0.308	-2.69	0.89	1	-4.6	0.52	-0.04	0.87	0.88
		(-2.9,-2.48)	(0.81,0.98)	(1,1)	(-4.99,-4.23)	(0.47,0.58)	(-0.15,0)	(0.77,0.97)	(0.78,0.98)

Rwanda									
Bin	Weight	ϵ_{NR} (1)	CS_{NR} (2)	$\%NR$ (3)	ϵ_{HVR} (4)	CS_{HVR} (5)	$\%HVR$ (6)	CS (LB) (7)	CS (8)
95%–100%	0.479	-0.56	3.85	1	-0.82	2.6	0	3.85	3.85
		(-0.66,-0.47)	(3.19,4.7)	(0.77,1)	(-0.99,-0.67)	(2.11,3.26)	(-0.31,0.2)	(2.8,4.65)	(3.09,4.67)
85%–95%	0.201	-1.57	1.14	0.87	-2.59	0.69	0.06	1.03	1.11
		(-1.75,-1.4)	(0.99,1.3)	(0.63,1)	(-2.92,-2.3)	(0.6,0.8)	(-0.21,0.33)	(0.81,1.23)	(0.94,1.28)
0%–85%	0.321	-5.2	0.24	1	-9.18	0.14	-0.01	0.24	0.24
		(-5.52,-4.89)	(0.22,0.26)	(0.92,1)	(-9.77,-8.61)	(0.13,0.15)	(-0.11,0.06)	(0.21,0.26)	(0.22,0.26)

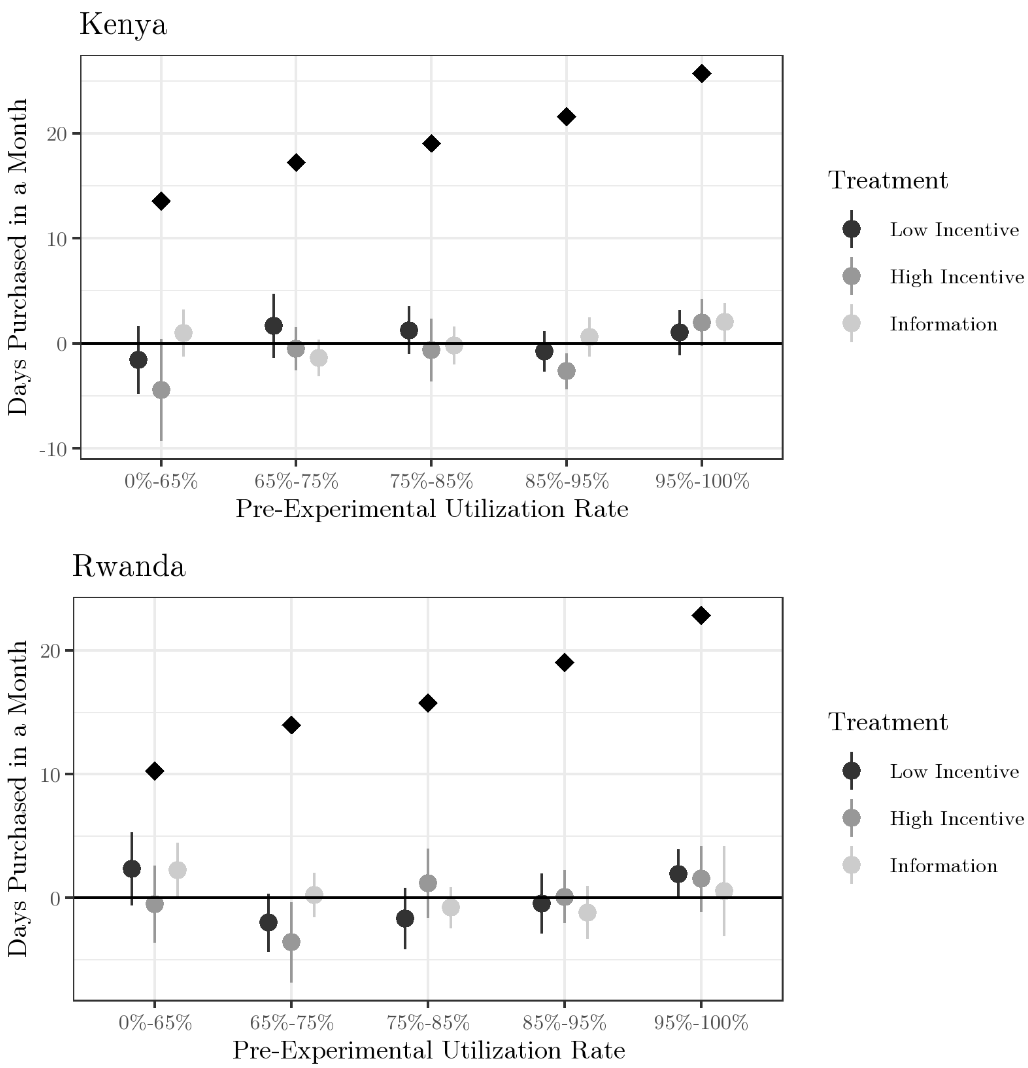
Notes: Bounds on price elasticities and consumer surplus (in USD). Column (1) is a lower bound on the price elasticity for non-responders, which is also the upper bound on the price elasticity for high-value responders. Column (2) is a lower bound on consumer surplus for non-responders and the upper bound on consumer surplus for high-value responders. Column (3) is the estimated proportion of non-responders. Column (4) is a lower bound on the price elasticity for high-value responders, which is also an upper bound on the price elasticity for low-value responders. Column (6) is a lower bound on consumer surplus for high-value responders and an upper bound on consumer surplus for low-value responders. Column (6) is the estimated proportion of high-value responders. Column (7) is a lower bound on consumer surplus for the strata, which assumes that non-responders and high-value responders only enjoy their lower bound of consumer surplus and low-value responders get zero consumer surplus. Column (8) calculates consumer surplus by assuming that low-value and high-value responders both enjoy their upper bound of consumer surplus and non-responders enjoy their lower bound. I report bootstrapped 95% confidence intervals in parentheses, which account for uncertainty in the estimated parameters.

Figure 1: The Monthly Reward for Different Consumer Types



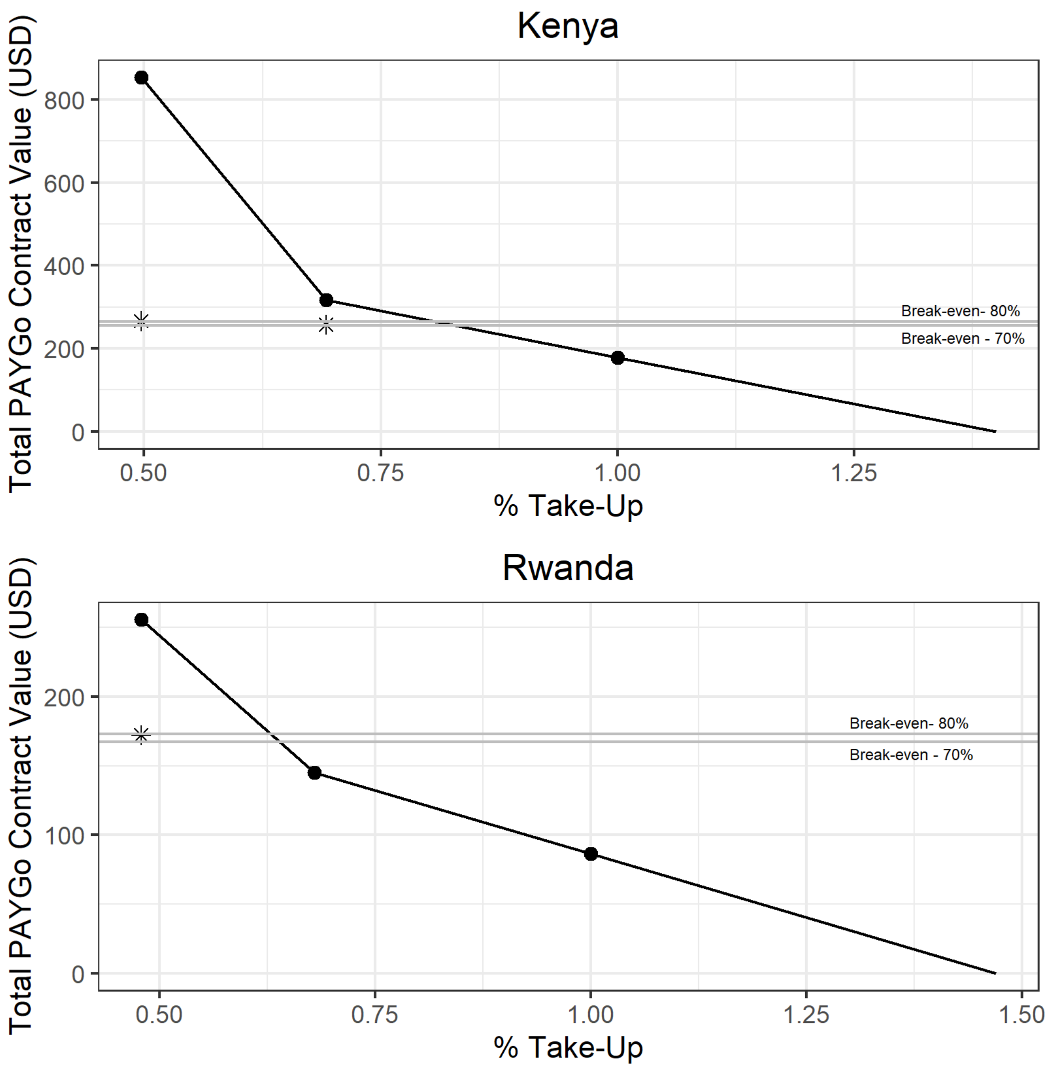
Note: Visual representation of the incentive structure that allows me to bound the price elasticity of demand for different types of consumers. The marginal price of one additional day of access time goes to zero at the qualifying threshold of 28 days. Under the high bonus, the marginal price stays at zero up to thirty days, then returns to a (normalized) price of 1. Under the low bonus, the marginal price stays at zero up to twenty-nine days. 25.32 days per month is the counterfactual demand in the control group for consumers in the highest stratum in Kenya, indicating that the average consumer will not qualify for bonus days without increasing their monthly solar purchases. It is only optimal for a consumer to increase their purchases to the qualifying threshold if the loss of consumer surplus (area A) is less than the consumer surplus gained from the bonus days (area B). Panel (A) illustrates a consumer who will never respond because $A > B$ even under the high bonus amount. Panel (B) illustrates a consumer who always responds because $A < B$ even under a low bonus amount. Panel (C) illustrates a consumer who responds under the high bonus, but not the low bonus.

Figure 2: Heterogeneous ATEs by Pre-Experimental Demand



Note: Estimated heterogeneous average treatment effects on days purchased in a month by levels of pre-experimental demand and by the randomly assigned incentive level. The pre-experimental utilization rate is the proportion of time consumers have access to solar prior to the start of the experiment, over their entire tenure with the solar firm. Bars are 95% confidence intervals, calculated using standard errors clustered at the level of the individual consumer. Black diamonds show average monthly purchases among consumers in the control group in each strata.

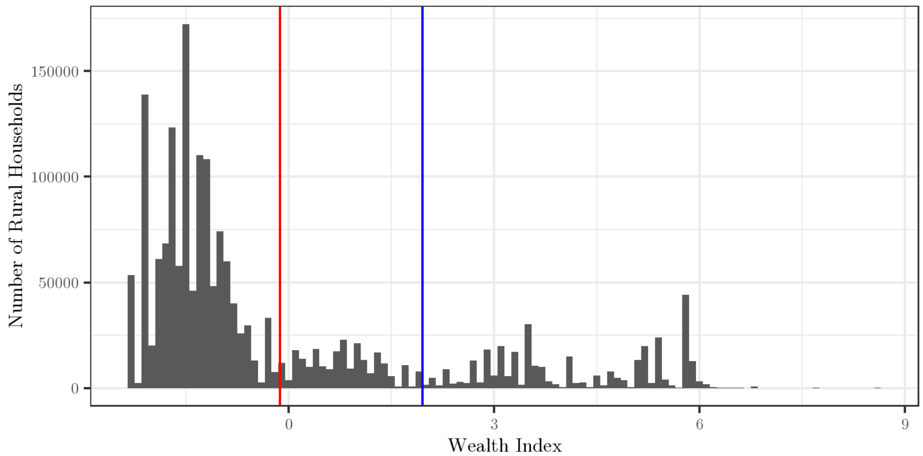
Figure 3: Cross-Sectional Demand for PAYGo Solar



Note: Black points show total willingness to pay for a PAYGo solar contract based on the estimates of consumer surplus derived in Table 2. All calculations assume a 3-year lifespan for the solar home system and a 15% discount rate. I normalize the population of PAYGo adopters to 1. The leftmost points represent estimates for consumers in the top stratum of the experiment, the middle points estimates for consumers in the second stratum, and the rightmost points estimates for consumers in the bottom three strata. Grey lines show break-even PAYGo contract prices under two different assumptions about what level of utilization is required for the solar company to break even: 70% or 80%. Asteriks show the amount of revenue the solar company is receiving from consumers in each stratum based on demand in the control group during the experiment, where it falls below my lower bound. I assume a linear slope out of sample, which is conservative for estimating adoption as long as demand is weakly convex.

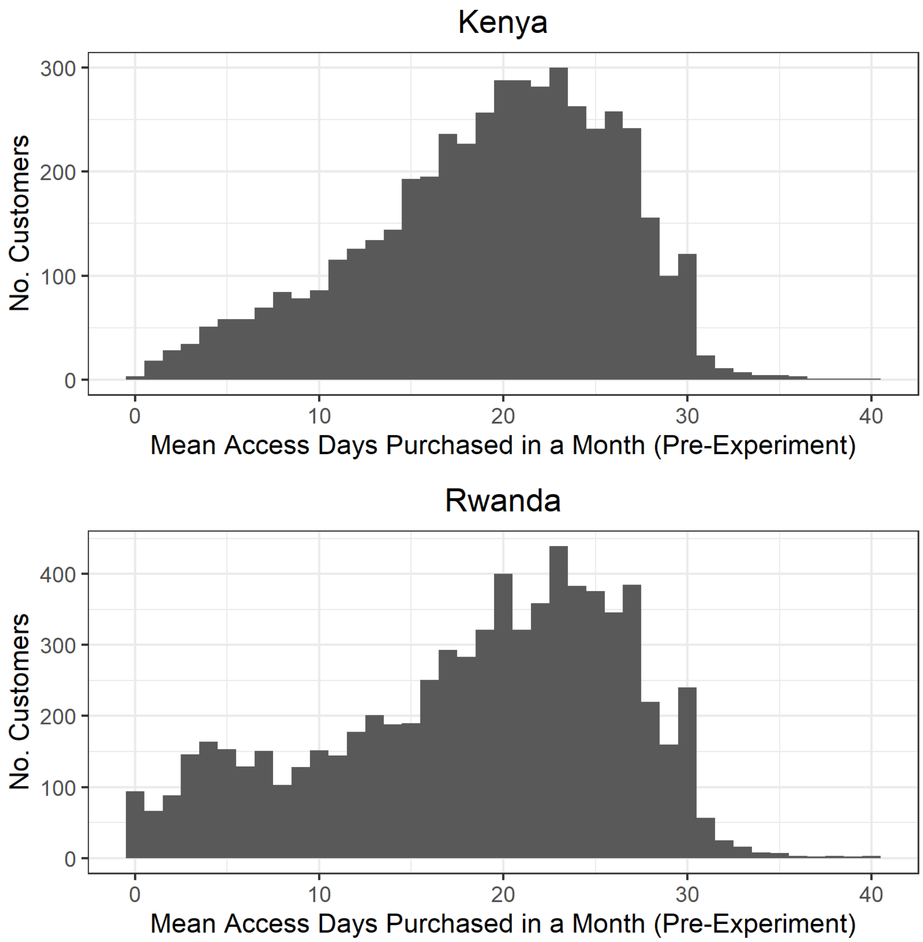
A Appendix A - Supplemental tables and figures

Figure A1: Comparison of PAYGo Consumers to the Average Rural Household



Note: Reproduced from Lang (2025b), Figure A8. "The figure shows the distribution of wealth indices for a nationally representative sample of rural households in Rwanda using data from the 2016–2017 Integrated Household Living Conditions Survey (National Institute of Statistics of Rwanda December 2017). The red line is mean wealth for the nationally representative sample. The blue line is mean wealth for PAYGo consumers who responded to the phone survey. The wealth index uses ubudehe category (a government-assigned category designed to summarize the socio-economic status of a household), roof material, wall material, floor material, primary source of electricity (if any), primary source of light, whether or not the household is connected to the national grid, and weekly energy expenditures."

Figure A2: Pre-Experimental Distribution of Demand among Sampled Consumers



Note: Distribution of mean days of access time purchased in a month during the pre-period in Kenya and Rwanda among consumers in my sample.

Figure A3: Schedule of Incentives

Low Bonus			High Bonus		
Weeks to buy	**Bonus days**	**% discount**	**Weeks to buy**	**Bonus days**	**% discount**
4	1	3.57%	4	2	7.14%
5	3	8.57%	5	4	11.43%
6	5	11.90%	6	6	14.29%
8	7	12.50%	8	10	17.86%
12	12	14.29%	12	20	23.81%

Note: Bonus amounts and weeks of prepaid access time consumers were required to purchase over the course of a calendar month to earn the bonus.

Table A1: Balance - Kenya

	Control	Info	MR	p-value
Days Bought per Month	19.33	19.911	18.819	0.08
	(6.812)	(6.768)	(7.261)	
Mean Payment Size (Days)	7.872	8.202	7.749	0.54
	(6.182)	(6.762)	(6.466)	
Num. Payments per Month	3.786	3.964	3.931	0.41
	(3.103)	(3.317)	(3.346)	
Daily Rate	32.973	32.082	34.058	0.18
	(15.294)	(14.315)	(15.467)	
Mean wH Used	1020.7	1029.205	1001.132	0.95
	(1316.512)	(1177.352)	(911.826)	
Own Lights	0.382	0.365	0.347	0.39
	(0.486)	(0.482)	(0.477)	
Own TV	0.237	0.207	0.28	0.08
	(0.426)	(0.406)	(0.45)	
Own Radio	0.394	0.397	0.429	0.47
	(0.489)	(0.49)	(0.496)	
Own Torch	0.246	0.244	0.231	0.83
	(0.431)	(0.43)	(0.422)	
N	2800	399	400	

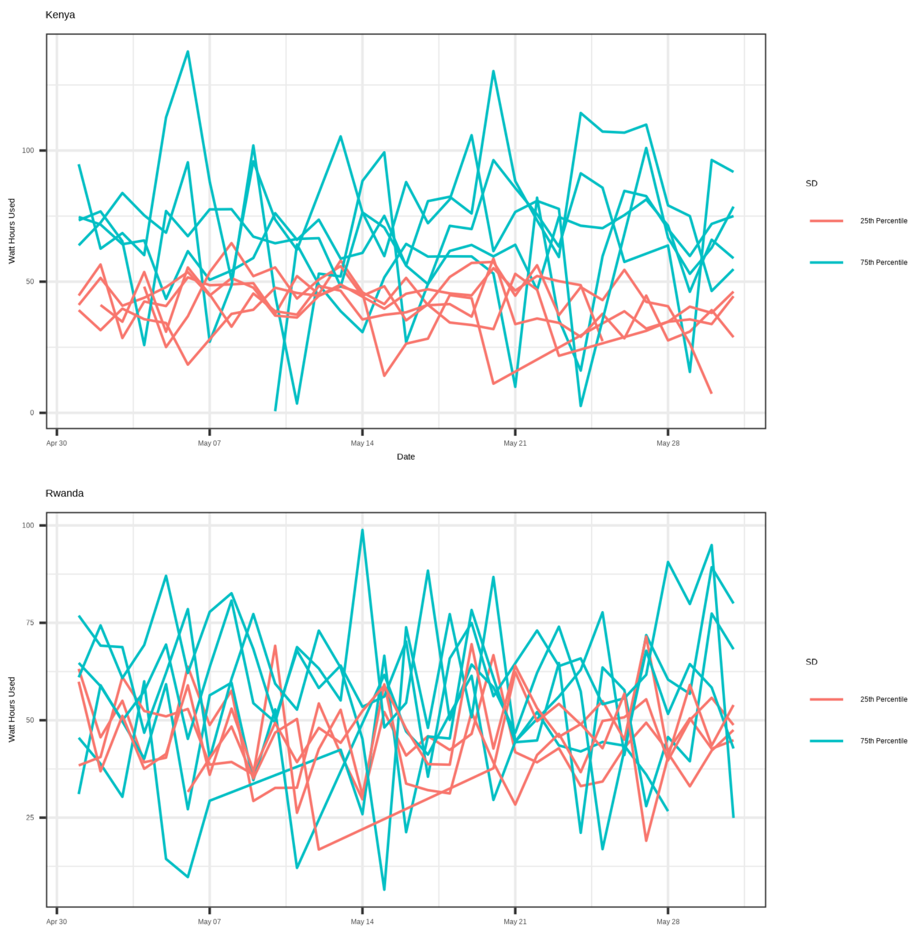
Notes: Mean pre-period covariates by treatment group. Standard deviations are in parentheses. Column 2 refers to the information treatment and column 3 refers to the monthly reward. Column 4 reports p-values associated with F-tests of joint equality between the three groups.

Table A2: Balance - Rwanda

	Control	Info	MR	p-value
Days Bought per Month	18.486	19.155	18.258	0.25
	(8.376)	(8.168)	(8.187)	
Mean Payment Size (Days)	9.798	10.058	9.913	0.75
	(7.169)	(7.403)	(7.186)	
Num. Payments per Month	2.449	2.465	2.453	0.99
	(1.831)	(1.694)	(1.867)	
Daily Rate	192.619	199.36	201.167	0.04
	(79.291)	(88.418)	(87.63)	
Mean wH Used	859.098	923.12	838.169	0.53
	(1214.097)	(1122.086)	(778.306)	
Own Lights	0.537	0.557	0.555	0.67
	(0.499)	(0.498)	(0.498)	
Own TV	0.097	0.11	0.135	0.07
	(0.296)	(0.314)	(0.342)	
Own Radio	0.307	0.31	0.27	0.36
	(0.461)	(0.463)	(0.444)	
Own Torch	0.196	0.209	0.207	0.76
	(0.397)	(0.407)	(0.406)	
N	5198	400	400	

Notes: Mean pre-period covariates by treatment group. Standard deviations are in parentheses. Column 2 refers to the information treatment, column 3 refers to the monthly reward. Column 4 reports p-values associated with F-tests of joint equality between the four groups.

Figure A4: Variance in Watt Hours Used Between Consumers



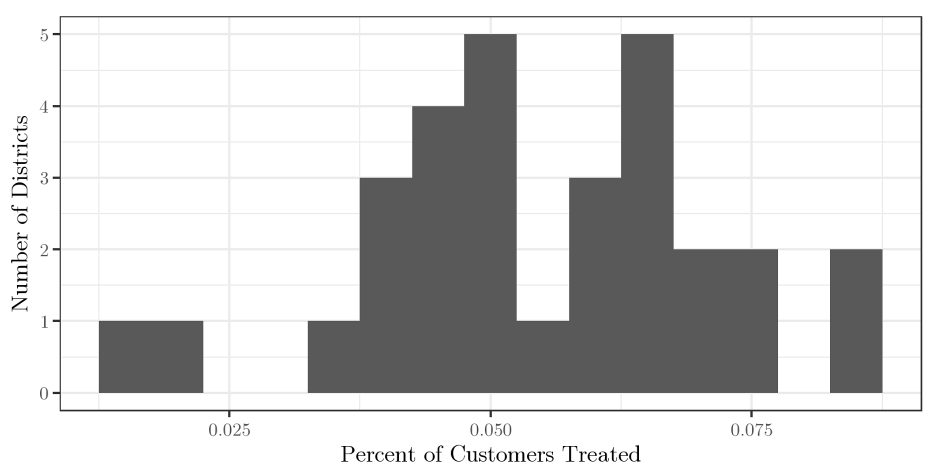
Note: Plots show the watt hours used when systems are switched on for a random subset of high-demand consumers in May, 2018. Pre-experimental mean use falls within 2 units of the median for all selected consumers. Blue lines represent consumers with a pre-experimental standard deviation in use that falls near the 75th percentile. Orange lines represent consumers with a pre-experimental standard deviation in use that falls near the 25th percentile.

Table A3: Pooled Average Treatment Effects from Incentives and Information, Excluding February

	Dependent variable: Days Bought in a Month			
	Kenya		Rwanda	
	(1)	(2)	(3)	(4)
Information	0.141	0.141	0.093	0.093
	(0.464)	(0.464)	(0.573)	(0.573)
Incentive	-0.4		-0.054	
	(0.483)		(0.478)	
Low Incentive		0.381		0.031
		(0.613)		(0.62)
High Incentive		-1.182		-0.14
		(0.712)		(0.69)
Control Mean	19.445	19.445	16.392	16.392
Observations	39589	39589	65978	65978
Consumer FEs	X	X	X	X
Month FEs	X	X	X	X

Notes: Average treatment effects on the number of days purchased in a month for incentives and the information treatment, pooling across the sample stratification. Note that days purchased does not include bonus days earned from the incentive. I cluster all standard errors at the level of the consumer.

Figure A5: Distribution of the Proportion of Treated Consumers within Districts in Rwanda



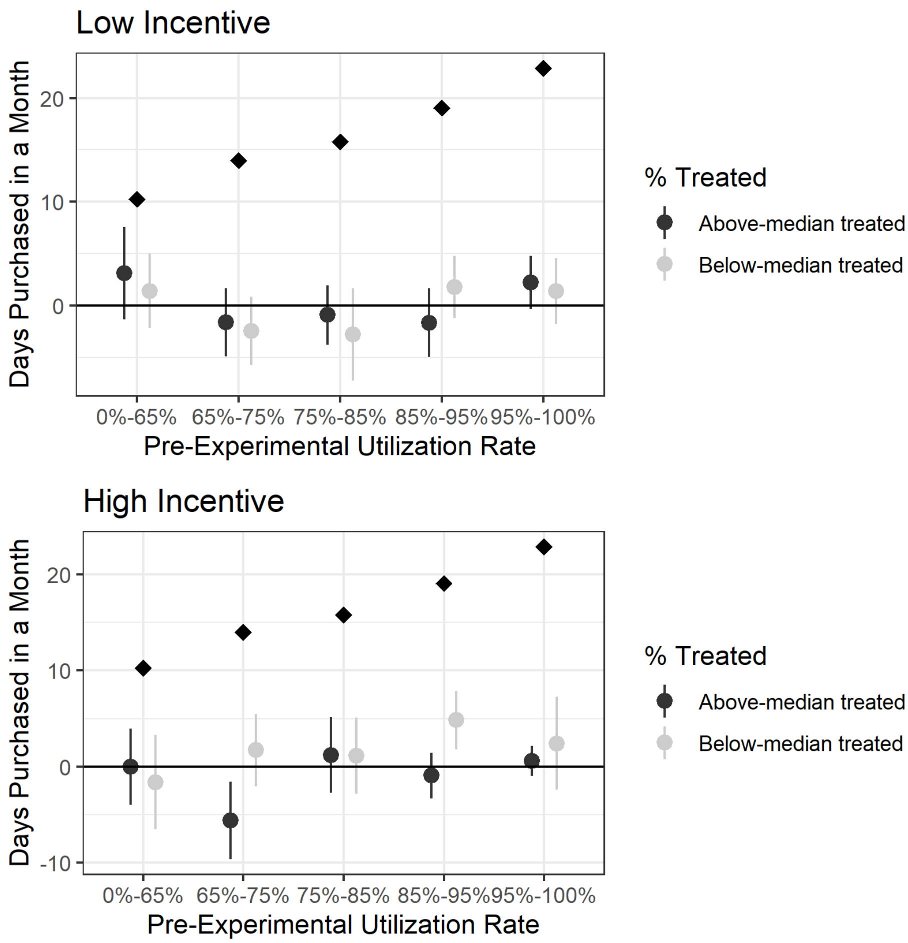
Note: Distribution of the proportion of customers of the solar company who were offered the incentive within each district of Rwanda (the smallest geographic unit for which I have information about customers) The median is 0.053 and the mean is 0.055.

Table A4: Heterogeneous Average Treatment Effects by Access to Liquidity

	Dependent variable: Days Bought in a Month	
	(1)	(2)
Information	-0.807	
	(1.132)	
Incentive	0.034	-0.456
	(0.969)	(1.006)
Information x Liquidity	2.412	
	(1.273)	
Incentive x Liquidity	0.825	0.825
	(1.062)	(1.062)
Observations	10379	6828
Adjusted R^2	0.237	0.217
Consumer FEs	X	X
Month FEs	X	X

Notes: “Liquidity” is a dummy variable equal to one if the consumer responds yes to the phone survey question “Have you ever borrowed to pay for solar?” Data come from a phone survey conducted in March 2019, after the experiment ended. Column (1) estimate effects relative to consumers in the control group. Column (2) estimates effects for the incentive relative to the information treatment and omits consumers in the control group due to differences in response rates between treated and control consumers. I cluster standard errors at the level of the individual consumer.

Figure A6: Heterogeneity by the proportion of consumers treated in a district



Note: Heterogeneous average treatment effects for the low incentive (top panel) and high incentive (bottom panel) by strata and the percentage of customers treated in each treated customer's district. Black dots show effects for consumers in districts with an above-median percentage of consumers treated (over 5.3%). Grey dots show effects for consumers in districts with a below-median percentage of consumers treated (less than or equal to 5.3%). The pre-experimental utilization rate is the proportion of time consumers have access to solar prior to the start of the experiment, over their entire tenure with the solar firm. Bars are 95% confidence intervals, calculated using standard errors clustered at the level of the individual consumer. Black dots show average monthly purchases among consumers in the control group in each strata.

B Appendix B - Additional details for bounding consumer surplus

From Figure 1, non-responders have $A > B$.

$$A = \frac{1}{2}(28 - Q_c)(1 - p_{28}) = (14 - \frac{Q_c}{2})(1 - p_{28}).$$

$$B = P_{30}(30 - 28) + \frac{1}{2}(30 - 28)(P_{28} - P_{30}) = P_{30} + P_{28}.$$

It follows that for non-responders,

$$(14 - \frac{Q_c}{2})(1 - p_{28}) > P_{30} + P_{28}.$$

Rearranging to combine all of the P_{28} terms gives

$$(14 - \frac{Q_c}{2}) - P_{28}(14 - \frac{Q_c}{2}) - P_{28} > P_{30},$$

or

$$(14 - \frac{Q_c}{2}) - P_{28}(15 - \frac{Q_c}{2}) > P_{30}.$$

Rearranging a final time yields Equation 3:

$$(14 - \frac{Q_c}{2}) > P_{30} + P_{28}(15 - \frac{Q_c}{2}).$$

Assuming linear demand, the slope is equal to

$$s = \frac{P_{30} - 1}{30 - Q_c} = \frac{P_{28} - 1}{28 - Q_c}.$$

Rearranging gives

$$(P_{30} - 1)(28 - Q_c) = (P_{28} - 1)(30 - Q_c),$$

which, after expanding, is

$$28P_{30} - 28 - Q_cP_{30} + Q_c = 30P_{28} - 30 - Q_cP_{28} + Q_c.$$

Simplifying, this becomes

$$P_{30}(28 - Q_c) + 2 = P_{28}(30 - Q_c).$$

Multiplying both sides by 1/2 yields Equation 4:

$$P_{30}(14 - \frac{Q_c}{2}) + 1 = P_{28}(15 - \frac{Q_c}{2}).$$

Substituting Equation 4 into Equation 3 gives

$$(14 - \frac{Q_c}{2}) > P_{30} + P_{30}(14 - \frac{Q_c}{2}) + 1.$$

Rearranging yields Equation 5:

$$13 - \frac{Q_c}{2} > P_{30}(15 - \frac{Q_c}{2}) \text{ , or}$$

$$\frac{13 - \frac{Q_c}{2}}{15 - \frac{Q_c}{2}} > P_{30}.$$

Finally, I bound the price elasticity of demand between 30 days and counterfactual demand, meaning that the change in quantity demanded is $30 - Q_c$ and the change in price is $P_{30} - 1$. When a consumer is indifferent between qualifying for the high bonus and staying with counterfactual demand,

$$P_{30} = \frac{13 - \frac{Q_c}{2}}{15 - \frac{Q_c}{2}}.$$

I can therefore write the price elasticity as

$$\frac{30 - Q_c}{Q_c} \frac{1}{\frac{13 - \frac{Q_c}{2}}{15 - \frac{Q_c}{2}} - 1} = \frac{30 - Q_c}{Q_c} \frac{1}{\frac{13 - \frac{Q_c}{2} - 15 + \frac{Q_c}{2}}{15 - \frac{Q_c}{2}}}.$$

Simplifying, this becomes

$$\frac{30 - Q_c}{Q_c} \frac{15 - \frac{Q_c}{2}}{-2}.$$

Expanding, I arrive at the lower bound on the price elasticity of demand for non-responders:

$$\epsilon_{NR} = \frac{450 - 30Q_c + \frac{Q_c^2}{2}}{-2} \frac{1}{Q_c}.$$

B.1 Alternative estimates using constant elasticity demand

I can alternatively bound price elasticities and consumer surplus assuming that demand is constant elasticity rather than linear. With constant elasticity demand, area A from Figure 1 can be written as

$$A = \int_{Q_c}^{28} \left(1 - \frac{Q}{Q_c}^{-1/\epsilon}\right) dQ.$$

Re-arranging, this becomes

$$A = (28 - Q_c) - \int_{Q_c}^{28} \left(\frac{Q}{Q_c}^{-1/\epsilon}\right) dQ = (28 - Q_c) - \frac{Q_c^{1/\epsilon}}{1 - \frac{1}{\epsilon}} (28^{1-1/\epsilon} - Q_c^{1-1/\epsilon}), \text{ or}$$

$$A = (28 - Q_c) - \frac{\epsilon Q_c^{1/\epsilon}}{\epsilon - 1} (28^{\frac{\epsilon-1}{\epsilon}} - Q_c^{\frac{\epsilon-1}{\epsilon}}).$$

For non-responders, the relevant area B is the area below the demand curve between 28 and 30 days. This is

$$B = \int_{28}^{30} \left(\frac{Q}{Q_c}^{-1/\epsilon}\right) dQ.$$

Following analogous steps as for area A, this can be written as

$$B = \frac{\epsilon Q_c^{1/\epsilon}}{\epsilon - 1} (30^{\frac{\epsilon-1}{\epsilon}} - 28^{\frac{\epsilon-1}{\epsilon}}).$$

I numerically solve for the elasticity at which $A = B$. This elasticity is a lower bound on the elasticity for non-responders.

For low-value responders, the relevant area B is the area below the demand curve between 28 and 29 days, or

$$B = \frac{\epsilon Q_c^{1/\epsilon}}{\epsilon - 1}(29^{\frac{\epsilon-1}{\epsilon}} - 28^{\frac{\epsilon-1}{\epsilon}}).$$

I similarly solve numerically for the elasticity at which $A = B$ for low-value responders. This is an upper bound on the elasticity for low-value responders and a lower bound on the elasticity for high-value responders. The procedure for estimating the proportion of low-value, high-value, and non-responders remains the same as for the case of linear demand.

Table A5 shows the results. In Kenya, the bounds on elasticities for consumers in the top two strata are within the confidence intervals of my bounds using linear demand. For the lowest demand consumers, assuming constant elasticity demand results in significantly less elastic bounds than those implied by linear demand, and commensurately higher bounds on consumer surplus. My estimates move slightly more in Rwanda, leading to a lower bound on consumer surplus for consumers in the top stratum that is more than double my estimate using linear demand and a lower bound on consumer surplus for consumers in the second-highest stratum that is four times higher. However, in absolute terms these changes are small because consumer surplus from PAYGo solar in Rwanda is low.

Table A5: Bounds on Elasticities and Consumer Surplus using Constant Elasticity Demand

Kenya									
Bin	Weight	ϵ_{NR} (1)	CS_{NR} (2)	$\%NR$ (3)	ϵ_{HVR} (4)	CS_{HVR} (5)	$\%HVR$ (6)	CS (LB) (7)	CS (8)
95%–100%	0.497	-0.11	20.22	0.57	-0.16	19.46	0.42	19.81	20.21
		(-0.15,-0.08)	(19.22,21.17)	(-0.42,1)	(-0.22,-0.11)	(18.14,20.7)	(-0.76,1.3)	(-0.77,20.87)	(18.83,21.1)
85%–95%	0.195	-0.61	11.43	1	-1.04	8.38	-0.06	10.92	11.24
		(-0.7,-0.53)	(10.42,12.44)	(0.98,1)	(-1.21,-0.89)	(7.31,9.52)	(-0.32,0)	(8.62,12.23)	(9.93,12.29)
0%–85%	0.308	-1.95	3.68	1	-3.56	1.81	-0.04	3.61	3.61
		(-2.1,-1.82)	(3.33,4.06)	(1,1)	(-3.83,-3.3)	(1.62,2.04)	(-0.15,0)	(3.21,4.01)	(3.2,4.01)

Rwanda									
Bin	Weight	ϵ_{NR} (1)	CS_{NR} (2)	$\%NR$ (3)	ϵ_{HVR} (4)	CS_{HVR} (5)	$\%HVR$ (6)	CS (LB) (7)	CS (8)
95%–100%	0.479	-0.41	9.21	1	-0.68	7.51	0	9.21	9.21
		(-0.48,-0.34)	(8.49,9.99)	(0.77,1)	(-0.81,-0.55)	(6.65,8.48)	(-0.3,0.2)	(6.79,9.95)	(8.34,9.97)
85%–95%	0.201	-1.17	4.41	0.87	-2.09	2.56	0.06	4	4.28
		(-1.3,-1.05)	(3.94,4.92)	(0.62,1)	(-2.33,-1.85)	(2.2,2.99)	(-0.22,0.33)	(3.16,4.67)	(3.67,4.84)
0%–85%	0.321	-3.53	0.96	1	-6.58	0.45	-0.01	0.96	0.95
		(-3.72,-3.34)	(0.88,1.05)	(0.92,1)	(-6.94,-6.22)	(0.41,0.49)	(-0.11,0.06)	(0.85,1.04)	(0.86,1.04)

Notes: Bounds on price elasticities and consumer surplus (in USD). Column (1) is a lower bound on the price elasticity for non-responders, which is also the upper bound on the price elasticity for high-value responders. Column (2) is a lower bound on consumer surplus for non-responders and the upper bound on consumer surplus for high-value responders. Column (3) is the estimated proportion of non-responders. Column (4) is a lower bound on the price elasticity for high-value responders, which is also an upper bound on the price elasticity for low-value responders. Column (6) is a lower bound on consumer surplus for high-value responders and an upper bound on consumer surplus for low-value responders. Column (6) is the estimated proportion of high-value responders. Column (7) is a lower bound on consumer surplus for the strata, which assumes that non-responders and high-value responders only enjoy their lower bound of consumer surplus and low-value responders get zero consumer surplus. Column (8) calculates consumer surplus by assuming that low-value and high-value responders both enjoy their upper bound of consumer surplus and non-responders enjoy their lower bound. I report bootstrapped 95% confidence intervals in parentheses, which account for uncertainty in the estimated parameters. All consumer surplus calculations assume a choke price at 4 times the median daily rate.

B.2 Bias from consumers responding to information rather than price

This section considers the implications of consumers responding to the information in the incentives rather than purely the change in price, given that I cannot reject that the incentives have the same effect as information for consumers in most strata. Let $ATE_{HighBonus}$ be the average treatment effect resulting purely from the change in price for the high bonus, and $ATE_{LowBonus}$ be the effect from the change in price for the low bonus. Denote with i the effect resulting from information. Assume that $i \geq 0$ and that i is the same regardless of the bonus level since the qualifying threshold is the same for both incentives.[27] Therefore, the average treatment effects that I observe empirically are

$$\widehat{ATE_{LowBonus}} = ATE_{LowBonus} + i$$
$$\widehat{ATE_{HighBonus}} = ATE_{HighBonus} + i.$$

It follows that my estimated proportion of low-value responders is biased upward:

$$p_{LVR} = \frac{\widehat{ATE_{LowBonus}}}{28 - Q_c} = \frac{ATE_{LowBonus} + i}{28 - Q_c}.$$

The proportion of high-value responders remains the same.

$$p_{HVR} = \frac{\widehat{ATE_{HighBonus}}}{28 - Q_c} - p_{LVR} = \frac{ATE_{HighBonus} + i}{28 - Q_c} - \frac{ATE_{LowBonus} + i}{28 - Q_c}, \text{ so}$$

$$p_{HVR} = \frac{ATE_{HighBonus} - ATE_{LowBonus}}{28 - Q_c}.$$

However, the upward bias in the proportion of low-value responders mechanically results in a downward bias in the proportion of non-responders.

$$p_{NR} = 1 - p_{HVR} - p_{LVR} = 1 - \frac{ATE_{HighBonus} - ATE_{LowBonus}}{28 - Q_c} - \frac{ATE_{LowBonus} + i}{28 - Q_c}.$$

Simplifying, this yields

$$p_{NR} = 1 - \frac{ATE_{HighBonus} - i}{28 - Q_c}.$$

Intuitively, non-responders have the least elastic demand and low-value responders have the most elastic demand. This means that overestimating the proportion of low-value responders and underestimating the proportion of non-responders biases my bounds on consumer surplus downward, making them conservative.

Note that the same logic applies to partial responses to the incentives (i.e., increasing demand by a small amount but not enough to reach the incentive after uncertainty about need for electricity gets resolved over the course of the month). Such a partial response would imply that

$$\widehat{ATE_{LowBonus}} = ATE_{LowBonus} + p_l \text{ and that}$$

$$\widehat{ATE_{HighBonus}} = ATE_{HighBonus} + p_h,$$

where p_l and p_h denote the component of the average treatment effect that comes from consumers

[27]This is without loss of generality. Different levels of information bias for high versus low incentives lead to overestimating the proportion of both high- and low-value responders and underestimating the proportion of non-responders, which still leads to an overall downward bias in my bounds on consumer surplus.

partially responding to the incentives. Again, this will result in overestimates of the proportion of low-value and high-value responders and underestimates of the proportion of non-responders, biasing my bounds on consumer surplus downward.

B.3 Producer surplus calculation

Since I lack detailed price data, my estimates of producer surplus are based on the total value of the PAYGo contract and the assumption that the solar firm is profit maximizing, implying that the choice of contract and prices reflect the lending costs the company faces, the costs of repossession, etc. On average, the total amount paid for the PAYGo contract is \$208.05 in Rwanda and \$317.55 in Kenya. Based on conversations with the solar company, the average utilization rate would need to be 70% - 80% for the company to break even. Assuming a 15% annual discount rate, this implies that the net present value of the contract in Rwanda is

$$\sum_{t=0}^{2} 55.48/1.15^t + \frac{41.61}{1.15^3} = 173.03 \text{ if 80\% utilization is necessary for the firm to break even, or}$$

$$\sum_{t=0}^{3} 48.55/1.15^t + \frac{13.85}{1.15^4} = 167.32 \text{ if 70\% utilization is necessary for the firm to break even.}$$

In Kenya, the net present value of the contract is

$$\sum_{t=0}^{2} 84.68/1.15^t + \frac{63.51}{1.15^3} = 264.10 \text{ if 80\% utilization is necessary for the firm to break even, or}$$

$$\sum_{t=0}^{3} 74.10/1.15^t + \frac{21.15}{1.15^4} = 255.38 \text{ if 70\% utilization is necessary for the firm to break even.}$$

I evaluate producer surplus by comparing these estimated break-even levels to observed demand in the control group during my experiment when they fall below my estimated lower bound on demand and to my lower bound on demand otherwise. This ensures that I arrive at a conservative lower bound on producer surplus. Table A6 shows the results. In Kenya, the 70% break-even assumption implies that the firm is falling short of breaking even, with producer surplus of -\$27. Under the 80% assumption, the firm has producer surplus of -\$17. Results are similar in Rwanda, with producer surplus of -\$28 under the 70% assumption and -\$34 under the 80% assumption.

B.4 Marginal value of public funds (MVPF) assumptions

I use the framework presented by Hahn et al. (2024) to calculate the MVPF of subsidizing solar home systems. Assuming perfect competition, the MVPF can be written as

$$\frac{1 + \frac{V}{p}(-\epsilon)}{1 + \frac{\tau}{p}(-\epsilon),}$$

where p is the unsubsidized price of the good, V is the marginal social benefit from consuming an additional unit of the good, τ is the marginal net cost to the government, and ϵ is the good's price elasticity.

First consider the numerator. I use the lower bound on my elasticity of demand for the lowest

Table A6: Producer Surplus Calculation

Strata	Utilization Rate	Implied NPV of Contract	PS Lower Bound - 70% Assumption	PS Lower Bound - 80% Assumption
		Kenya		
95%–100%	0.83	266.18	10.80	2.08
85%–95%	0.72	257.06	1.68	-7.04
0%–85%	0.54	236.71	-77.26	-85.98
Weighted average			-18.10	-26.82
		Rwanda		
95%–100%	0.76	172.18	4.86	-0.85
85%–95%	0.69	166.69	-22.09	-27.80
0%–85%	0.50	150.92	-80.89	-86.60
Weighted average			-28.08	-33.79

Note: Strata are based on utilization rates before the experiment started. Utilization rate is the proportion of time consumers in the control group purchased solar access during the experiment. The 70% assumption for producer surplus assumes that the firm breaks even if the average utilization rate is 70%, which implies a net present value (NPV) of the contract of \$167.32 in Rwanda and \$255.38 in Kenya. The 80% assumption assumes that the firm breaks even only if the average utilization rate is 80%, or if the NPV of the contract is \$173.03 in Rwanda and \$264.10 in Kenya. All NPV calculations assume a 15% annual discount rate. The final two columns show producer surplus calculated using the lower bounds I estimate experimentally in Table 2 when they are lower than the demand observed in the control group, and demand in the control group when by lower bounds are higher.

strata to assess the MVPF for expanding access to PAYGo solar. This is -2.69 in Kenya and -5.2 in Rwanda. This is the relevant elasticity assuming that the consumers induced to adopt solar as a result of the subsidy are most similar to the lowest-demand consumers who have self-selected into PAYGo contracts. I use the value of reduced carbon emissions as the value of V (\$36), although it is worth noting that this may understate the social benefits of solar home systems since it does not account for any improvements in local environmental quality. I set the price p as the effective price that consumers in the lowest strata pay over the course of the PAYGo contract: \$237 in Kenya and \$151 in Rwanda. It follows that the numerator of the MVPF is

$$1 + \frac{36}{237}(2.69) = 1.41 \text{ in Kenya and}$$

$$1 + \frac{36}{151}(5.2) = 2.24 \text{ in Rwanda.}$$

Next I turn to the fiscal externalities from subsidies for PAYGo solar. There is little evidence that the energy services provided by a solar home system have significant impacts on income or education (Rom, Günther, and Harrison 2017, Stojanovski et al. 2021, Kudo, Shonchoy, and Takahashi 2014). Although Hahn et al. (2024) include fiscal benefits from reduced climate damages in their estimates, given the overall contribution of both countries to global carbon emissions and the relatively small reduction in emissions due to solar home systems, it is unlikely that climate benefits will lead to any meaningful fiscal externality in my setting. I therefore only consider the administrative costs of the subsidy and direct changes to value-added tax revenues. Based on Aker et al. (2016), I assume that there are around \$0.07 in administrative costs per dollar spent on the subsidy. Seven percent is the estimated administrative cost for a subsidy delivered via mobile phones in Niger in Aker et al. (2016), which seems feasible in the PAYGo context since all customers make mobile solar payments.

The change in value-added tax revenues for solar versus alternative energy sources like kerosene and disposable batteries is

$$\Delta_{tax} = (TaxRate_{solar} * Spending_{solar}) - (TaxRate_{nonsolar} * Spending_{nonsolar}).$$

In Kenya, the value added tax for solar products is the same as for all alternative energy sources. As stated in Section 2, the median rural household in Kenya spends 0.5% of their income on energy services that can be replaced by a solar home system, approximately \$101 over the course of the PAYGo contract based on utilization rates among consumers in the lowest strata, compared to \$237 for the solar home system (Kenya National Bureau of Statistics 2016). The value added tax rate in Kenya is 16%, so the change in tax revenues in Kenya is

$$\Delta_{tax} = 0.16(237 - 101) = \$21.76.$$

In Rwanda, solar home systems are exempt from taxes while other energy sources are taxed at 18%. The median rural household in Rwanda spends approximately \$10 on energy services that are replaceable with a basic SHS over the course of the PAYGo contract. Therefore, the change in tax revenues in Rwanda is

$$\Delta_{tax} = -0.18(10) = -\$1.80.$$

Taking administrative costs and changes in tax revenues together, the denominator of the MVPF is

$$1 + 0.07 + \frac{-21.76}{237}(2.69) = 0.82 \text{ in Kenya and}$$

$$1 + 0.07 + \frac{1.8}{151}(5.2) = 1.13 \text{ in Rwanda.}$$

Under these assumptions, I calculate that the MVPF is 1.7 in Kenya and 2 in Rwanda.

I present the MVPF under an additional set of assumptions that accounts for the negative producer surplus my back of the envelope calculations find, particularly for the lowest-demand consumers. These estimates assume that the government would need to compensate the solar firm to take on more low-demand customers by adding the lost producer surplus to τ as an additional part of the subsidy. Under these assumptions, the demoninator of the MVPF is

$$1 + 0.07 + \frac{-21.76 + 77}{237}(2.69) = 1.69 \text{ in Kenya and}$$

$$1 + 0.07 + \frac{1.8 + 81}{151}(5.2) = 3.92 \text{ in Rwanda.}$$

This implies an MVPF of 0.83 in Kenya and 0.57 in Rwanda.

C Appendix C - Variable demand for electricity

I formally look for evidence of intertemporal substitution using the following two specifications, estimated separately for consumers with and without rechargeable appliances on all days when consumers' systems are switched on

$$wH_{it} = \alpha + \delta_1 Purchase_{it} + \delta_2 SwitchOn_{it} + \gamma_i + \gamma_t + \epsilon_{it}, \tag{6}$$

$$wH_{it} = \alpha + \eta_1 PriorOff_{it} + \gamma_i + \gamma_t + \mu_{it}. \tag{7}$$

wH_{it} is the number of watt hours consumed by consumer i on day t. $Purchase_{it}$ is a dummy variable equal to one if consumer i purchases additional access time on day t when their system is already switched on, which I will term a "continuation purchase." $SwitchOn_{it}$ is a dummy variable equal to one if consumer i makes a purchase on day t that switches their system on after a period of remote lockout. $PriorOff_{it}$ is a dummy variable equal to one on days prior to consumer i being remotely locked out. γ_i and γ_t are consumer and day fixed effects.

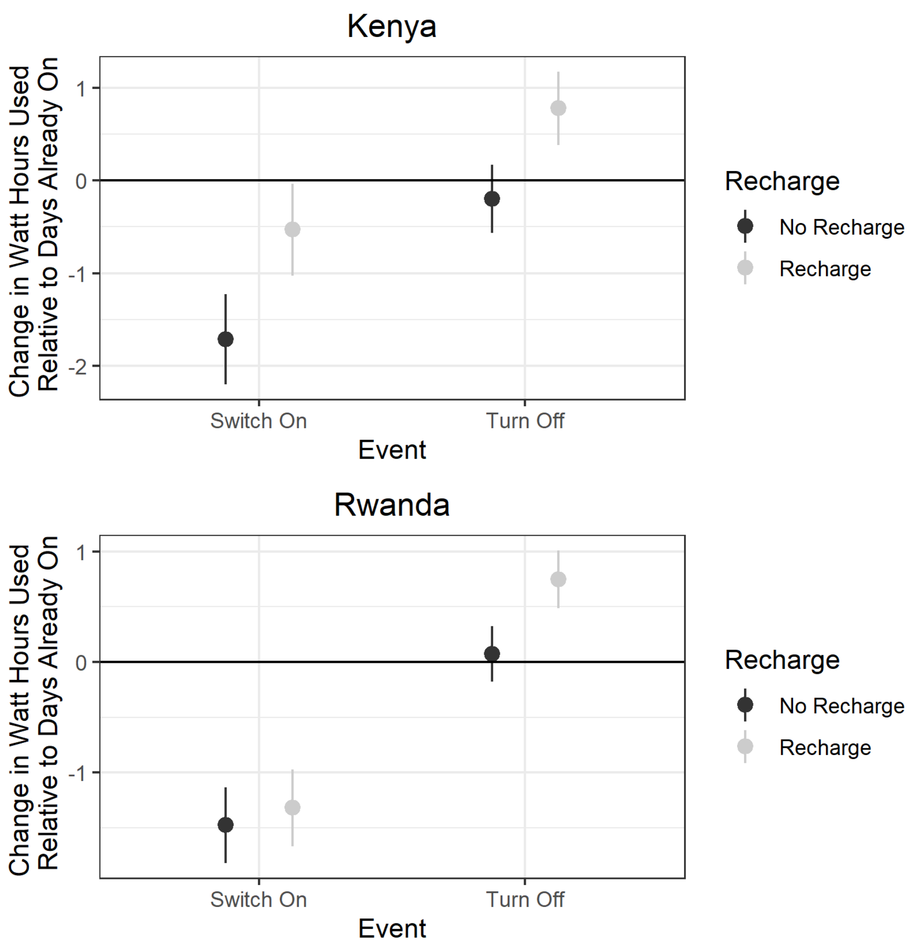
In Equation 6, $\delta_2 > 0$ indicates that consumers use more electricity on days when they make a purchase to get switched back on than on days when they have access to electricity but do not make a purchase. This pattern would be consistent with intertemporal substitution; however, it is difficult to see how consumers without rechargeable appliances can effectively engage in intertemporal substitution. The most convincing pattern suggesting intertemporal substitution would be to observe $\delta_2 > 0$ and to see that δ_2 is significantly higher for consumers with rechargeable appliances. The same logic applies to Equation 7: $\eta_1 > 0$ would be consistent with intertemporal substitution, and seeing a larger η_1 among consumers with rechargeable appliances would provide more convincing evidence of intertemporal substitution.

Figure A7 shows the estimated coefficients from the descriptive regressions. In both Kenya and Rwanda, consumers tend to use less electricity on days when their systems are switched back on relative to all other days when their systems are on and they are not making a continuation purchase. I do observe some patterns that are consistent with intertemporal substitution on the day before consumers get remotely locked out. Consumers with rechargeable appliances use significantly more electricity on these days than on other days when their systems are switched on in both countries. Even though the increase is statistically significant at the 5% level and they are significantly higher for consumers with rechargeable appliances compared to those without, the magnitudes are not economically meaningful. The median consumer in Kenya uses 41.5 watt hours on days when their system is switched on. The difference in use that I estimate for consumers with rechargeable appliances is 0.78 watt hours, only 1.9% of median use. In Rwanda, median use is 42.7 watt hours and my estimated difference is 0.75 watt hours, or 1.8% of median use. Taken together, the data do not display large enough intertemporal substitution to explain consumer non-response to the incentives.

Even without evidence of major intertemporal substitution, variable use of solar home systems suggests that consumers may gain variable utility from electricity access. I compute the standard deviation of watt hours used on days when consumers' systems are switched on in the four months prior to the start of the experiment. I examine heterogeneity along this dimension by dividing the distribution of standard deviations at the median and estimating heterogeneous treatment effects. Intuitively, consumers with highly variable demand for electricity may not be interested in the incentive because it may require that they pay for access on days when they may use little electricity.

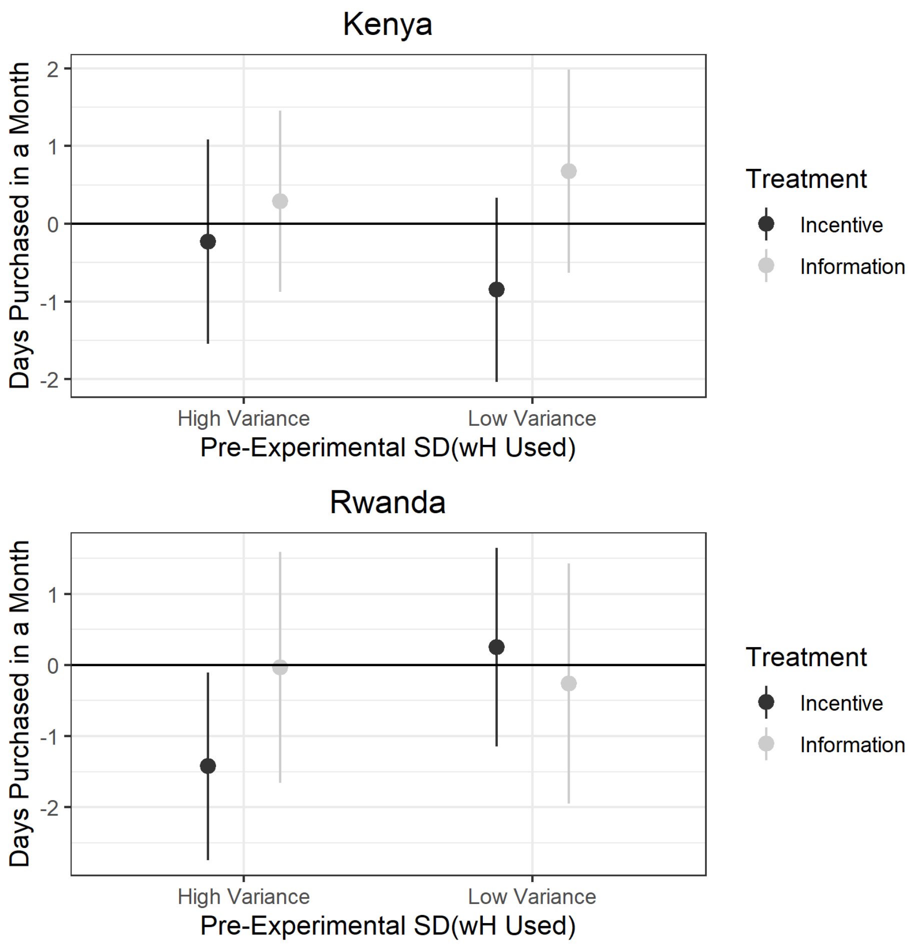
Figure A8 shows the results for Kenya and Rwanda. I find no evidence that consumers with higher variance in use respond to the incentive differentially in Kenya. In Rwanda, consumers with higher variance in use respond to the incentives less than those with lower variance in use, but the difference is only significant at the 10% level. Taken together, it appears that although consumers vary widely in the amount of electricity they use from day to day, it is likely a minor factor in consumers' response to the incentives.

Figure A7: Test for Intertemporal Substitution



Note: Estimated differences in watt hours used on days immediately following consumers being switched back on after a period of remote lockout (left) and on days immediately preceding remote lockout (right), relative to all days when consumers have access to electricity but are not making a purchase. Blue dots show differences for consumers who have at least one rechargeable appliance. Red dots show differences for consumers who do not have a rechargeable appliance from the solar company, though they may have a cell phone. Bars show 95% confidence intervals calculated using standard errors clustered at the level of the individual consumer.

Figure A8: Heterogeneous ATEs by Pre-Experimental SD(wH used)



Note: Estimated heterogeneous average treatment effects on days purchased in a month by varying levels of pre-experimental variance in watt hours used on days when systems are on. I divide the distribution of standard deviations in watt hours used into quartiles and estimate effects for each quartile. Bars are 95% confidence intervals, calculated using standard errors clustered at the level of the individual consumer.

D Appendix D - phone survey

ENDLINE QUESTIONNAIRE – ENGLISH VERSION

PROJECT "Incentives for Electricity Payment"

UC BERKELEY & INNOVATIONS FOR POVERTY ACTION

GENERAL CODING:
77 – NOT APPLICAPLE
88 – OTHER ANSWERS
99 – DON'T KNOW/REFUSES TO RESPOND (MISSING VALUES)

I - INTRODUCTION

Interviewer Name:			
INT_ID - Interviewer Number:		**HH_ID - Interview Number:**	

	Day		Month		Year			
Date of Interview *[Interviewer: Insert day, month, and year.]*								
Day of the week			Mon	Tue	Wed	Thu	Fri	Sat

	Hour		Minute	
Start time of interview *[Interviewer: Insert hour and minute, use 24 hours.]*				

Greetings. My name is _______________. I'm working with researchers to understand demand for solar electricity among rural consumers in Rwanda.

I1. Are you the one in your household who typically pays for your solar home system?	
Yes (Skip to informed consent)	1
No	0
Don't Know	99

I2. Can I speak to the person in your household who typically pays for your solar home system?	
Yes	1
No (If possible, note a good day/time to try back and end the survey:_________________________)	0
Don't Know	99

Interviewer: Read informed consent document to the participant. If participant consents to be surveyed, continue. If not, thank the participant and end the survey.

I3. Participant's gender	
Male	0
Female	1

I4. Can you please tell me the name, age, and gender of all people who currently regularly eat and sleep in your home?

ID	**Name**	**Age**	**Gender**
1			
2			

3			
4			
5			
6			
7			
8			

I5. What is your Ubudehe category?	
Ubudehe 1	1
Ubudehe 2	2
Ubudehe 3	3
Ubudehe 4	4
Don't Know	99

I6. What is the main construction material of your exterior wall?[i]	
Mud bricks	1
Mud bricks with cement (stucco)	2
Oven fired bricks	3
Cement blocks	4
Wooden planks	5
Stones	6
Tree trunks with mud	7
Tree trunks with mud and cement	8
Plastic sheeting	9
Other [Specify:_______________]	88
Don't know	99

I7. What is the main material used for roofing your main dwelling?[ii]	
Thatch/leaves/grass	1
Metal sheets/corrugated iron	2
Tiles clay	3
Concrete	4
Plastic/plywood/impermanent materials	5
Other [Specify:_______________]	88
Don't know	99

I8. What is the main material used for the floor of the dwelling?[iii]	
Beaten earth	1
Dung hardened	2
Wooden floor	3
Clay tiles	4
Cement	5
Bricks	6
Other [Specify:_______________]	88
Don't know	99

I9. What is the floor area of the dwelling?[iv]	____________ In m^2

A – ENERGY PURCHASING BEHAVIOR

A1. How long does it typically take you to reach the nearest mobile money agent?	_____________ minutes

A2. Think about the last 5 times you paid for solar. Of those 5 times, how many times did you have to visit a mobile money agent so you could deposit mobile money and use it to pay for solar?	
None	0
1 time	1
2 times	2
3 times	3
4 times	4
5 times	5

A3. If you had mobile money already in your account and you wanted to use it to pay for solar, do you know how you would do that?	
Yes	1
No	0

~~**A4. In the last 3 months, has there ever been a time when your system was switched off because you could not pay?**~~ ~~*Interviewer: make sure that participant does not tell you about times when they experienced technical difficulties with their system, we are only interested in systems being switched off due to non-payment.*~~	
~~Yes~~	~~1~~
~~No (Skip to A6)~~	~~0~~

A5. When you cannot pay for solar, what is the most common reason?	
Cannot find work	1
Poor harvest	2
Family member falls ill	3
Do not have time to reach the nearest mobile money agent	4
Do not have need of solar home system for some days (i.e., away from home)	5
Prioritize other expenses	6
Other [Specify:_____________]	88

A6. Have you ever borrowed money to pay for solar?	
Yes (Skip to A8)	1
No	0
Don't know	99

A7. If you have never borrowed money to pay for solar, why not?	
No one will lend for that purpose.	1
Didn't believe you would be able to pay back the loan.	2
Don't believe in borrowing for that purpose.	3
Have never needed to borrow to pay for solar.	4

Other [Specify:___________]	88
Don't know	99

A8. If your solar home system gets switched off or does not work for some other reason, what alternatives do you use for lighting?	
Battery operated torch/lamp	1
Candles	2
Kerosene lamp	3
Small solar lantern (not BBOXX)	4
Other [Specify:___________]	88
Don't know	99

A9. If your solar home system gets switched off or does not work for some other reason, what alternatives do you use for charging your phone?	
Go to a neighbor's house	1
Pay to charge phone at a shop	2
Do not use phone until system has been turned back on	3
Other [Specify:___________]	88
Don't know	99

A10. If your solar home system gets switched off or does not work for some other reason, what alternatives do you use for obtaining information that you would usually get from a radio or television?	
Go to a neighbor's house	1
Go to a local bar/restaurant with a radio/television	2
Speak to neighbors to hear any important news	3
None (that is, don't learn the news for the time the solar home system is switched off)	4
Other [Specify:___________]	88
Don't know	99

A11. Think about the past 7 days. On a typical day, how many hours did you spend lighting your home using the following?	
On-grid electricity	________ hours
Solar home system	________ hours
Oil lamp	________ hours
Firewood	________ hours
Candles	________ hours
Lantern	________ hours
Torch (powered with batteries, not including the rechargeable torch that the respondent may own as part of their solar home system)	________ hours
Other [Specify:________________]	________ hours

A12. In total over the past 7 days, how much did you spend on each of the following?	
On-grid electricity	_________ RWF
Solar home system	_________ RWF
Oil lamp	_________ RWF
Firewood	_________ RWF
Candles	_________ RWF
Lantern	_________ RWF
Torch (powered with batteries, not including the rechargeable torch that the respondent may own as part of their solar home system)	_________ RWF
Phone charging (meaning phone charging outside the home)	_________ RWF
Batteries to power small appliances	_________ RWF

B – CUSTOMER UNDERSTANDING OF INCENTIVES

B1. BBOXX recently ran a few different promotions for select customers. Do you remember being offered a promotion by BBOXX anytime between last May and now?	
Yes	1
No (Skip to section C)	0
Don't know	99

B2. Some BBOXX customers were given some bonus days if they purchased at least 4 weeks at once or at least 5 weeks at once. Were you offered that promotion anytime since last May?	
Yes	1
No (Skip to B5)	0
Don't know (Skip to B5)	99

B3. Do you remember if you needed to purchase 4 weeks or 5 weeks to qualify?	
4 weeks	1
5 weeks	2
Don't know	99

B4. Do you remember how many bonus days you would earn for purchasing that number of weeks?	
1 bonus day	1
2 bonus days	2
3 bonus days	3
4 bonus days	4
5 or more bonus days	5
Don't know	99

B5. Some BBOXX customers were given some bonus days if they purchased at least 4 weeks over the course of a month, or at least 5 weeks over the course of a month. Were you offered that promotion anytime since last May?	
Yes	1
No (Skip to B8)	0
Don't know (Skip to B8)	99

B6. Do you remember if you needed to purchase 4 weeks or 5 weeks to qualify?	
4 weeks	1

5 weeks	2
Don't know	99

B7. Do you remember how many bonus days you would earn for purchasing that number of weeks over the course of a month?	
1 bonus day	1
2 bonus days	2
3 bonus days	3
4 bonus days	4
5 or more bonus days	5
Don't know	99

B8. Did you ever purchase enough time on your solar account to earn bonus days?	
Yes	1
No (Skip to B11)	0
Don't know	99

B9. When is the last time you remember earning bonus days?	
January	1
December	12
November	11
October	10
September	9
August	8
July	7
June	6

B10. How many bonus days did you earn?	
1 bonus day	1
2 bonus days	2
3 bonus days	3
4 bonus days	4
5 bonus days	5
6 bonus days	6
7 bonus days	7
10 bonus days	8

12 bonus days	10
20 bonus days	20
Other [Specify:_______________________]	88
Don't know	99

B11. If you never earned bonus days, why not?	
Did not know/did not believe you were eligible for a promotion.	1
Do not need solar home system to be turned on for 4 weeks out of a month.	2
Never had enough money to pay for enough solar to earn the promotion.	3
Forgot about the promotion.	4
Did not believe you would receive bonus days	5
Did not understand how to earn bonus days.	6
Had difficulty keeping track of days purchased.	7
Other [Specify:_______________]	88
Don't know	99

B12. When you have a question or problem regarding your BBOXX account, how do you typically solve it? *[Select all that apply]*	
Check USSD menu	1
Call the call center	2
Visit the shop	3
Wait until you see a sales agent or technician nearby	4
Other [Specify:_______________]	88
Don't know	99

C – TIME USE

Think about a typical day in the 7 days between 6:00PM and 11:00 PM:							
Name (From HH roster)	ID (From HH roster)	C1. How many hours did _____ spend working?	C2. How many hours did _____ spend doing household labor (cooking, cleaning, childcare)?	C3. How many hours did ______ spend studying?	C4. How many hours did ______ spend listening to the radio/watching television?	C5. How many hours did ______ spend socializing or engaging in other leisure activities (not including sleep)?	C6. Total

D – CUSTOMER SATISFACTION

D1. On a scale of 1 to 5 where 1 is very dissatisfied and 5 is completely satisfied, how satisfied are you with your BBOXX solar home system?	__________

D2. On a scale of 1 to 5 where 1 is very dissatisfied and 5 is completely satisfied, how satisfied are you with BBOXX customer service?	__________

D3. On a scale of 1 to 10 where 1 is very unlikely and 10 is very likely, how likely are you to recommend BBOXX to a friend or a neighbor?	__________

D4. If you could change one thing about your BBOXX system or the service you get from BBOXX, what would it be?	
No energy service fee	1
Lower price	2
Option to pay more upfront to reduce daily rate	3
Better/faster technical service	4
Larger systems [Specify additional appliances that customer would like to have:______________________]	5
Free calls to the call center	6
Option to run a negative balance/borrow to keep system turned on	7
Option to transfer credit to another customer	8
Other [Specify:_______________]	88
Don't know	99

[i] Question from EICV4.
[ii] Question from EICV4.
[iii] Question from EICV4.
[iv] Question from EICV4.

E Appendix E - all pre-registered results

The solar company simultaneously experimented with three other types of incentives. The first were monthly rewards like the one I focus on in this paper, but with a 5-week qualifying threshold, meaning that consumers would need to purchase access time beyond the current month to earn a bonus. The second were bulk discounts set at the same level as the monthly reward (i.e., if consumers purchased four weeks in a single purchase, they would earn a bonus). The final incentives were bulk discounts with bonuses starting at 5 weeks. Table A7 and Table A8 show results pooling the treatments and fully disaggregating the treatment, respectively. Unsurprisingly, given low consumer response to the monthly reward with a 4-week qualifying threshold, none of these treatments led to any significant change in consumer behavior.

Table A7: Pooled Average Treatment Effects from Incentives

	Kenya			Rwanda		
	Days Bought	Times Off	Off Proportion	Days Bought	Times Off	Off Proportion
	(1)	(2)	(3)	(4)	(5)	(6)
Any Incentive	-0.341	-0.013	0.004	0.081	-0.015	-0.008
	(0.227)	(0.019)	(0.005)	(0.242)	(0.02)	(0.006)
Monthly Reward	0.082	0.007	0.003	-0.176	-0.017	0.005
	(0.35)	(0.021)	(0.008)	(0.348)	(0.022)	(0.009)
Control Mean	19.445	1.033	0.251	16.392	1.028	0.288
Observations	57588	56232	55749	86376	80881	77509
Consumer FEs	X	X	X	X	X	X
Month FEs	X	X	X	X	X	X

Notes: Average treatment effects on the number of days purchased in a month, times switched off in a month, and proportion of time in a month when the system was not switched on for any incentive and the incentive structured as a monthly reward rather than as a bulk discount. Estimates pool across randomized incentive levels, qualifying thresholds, and the sample stratification. Note that days purchased does not include bonus days earned from the incentive. I cluster all standard errors at the level of the consumer.

Table A8: Disaggregated Average Treatment Effects from Incentives

	Kenya			Rwanda		
	Days Bought	Times Off	Off Proportion	Days Bought	Times Off	Off Proportion
	(1)	(2)	(3)	(4)	(5)	(6)
Any Incentive	-0.349	-0.005	0.001	0.125	-0.021	-0.009
	(0.23)	(0.02)	(0.005)	(0.246)	(0.02)	(0.006)
Monthly Reward (MR)	0.463	-0.011	0.016	-0.04	-0.033	0.013
	(0.615)	(0.038)	(0.015)	(0.6)	(0.042)	(0.018)
Bulk x High Discount	0.345	-0.016	0.003	-0.11	0.021	0.005
	(0.588)	(0.04)	(0.014)	(0.617)	(0.046)	(0.017)
MR x High Discount	-1.563	-0.04	0.002	-0.287	0.007	-0.017
	(0.913)	(0.051)	(0.02)	(0.87)	(0.058)	(0.026)
Bulk x High Threshold	-0.249	-0.04	0.011	-0.448	0.052	0.012
	(0.634)	(0.037)	(0.015)	(0.861)	(0.043)	(0.018)
MR x High Threshold	0.411	0.074	-0.035	0.441	-0.002	-0.018
	(0.865)	(0.053)	(0.022)	(0.863)	(0.059)	(0.024)
Bulk x High Discount x High Threshold	-0.113	0.025	0.003	0.454	-0.052	-0.026
	(1.021)	(0.065)	(0.026)	(1.273)	(0.076)	(0.029)
MR x High Discount x High Threshold	0.806	-0.025	0.024	-0.993	0.073	0.041
	(1.274)	(0.075)	(0.029)	(1.247)	(0.083)	(0.034)
Control Mean	19.445	1.033	0.251	16.392	1.028	0.288
Observations	57588	56232	55749	86376	80881	77509
Consumer FEs	X	X	X	X	X	X
Month FEs	X	X	X	X	X	X

Notes: Average treatment effects on the number of days purchased in a month, times switched off in a month, and proportion of time in a month when the system was not switched on. Estimates pool across the sample stratification. Note that days purchased does not include bonus days earned from the incentive. I cluster all standard errors at the level of the consumer.