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Short-Run Subsidies, Take-Up, and Long-Run Demand for Off-Grid Solar for the Poor

Evidence from Large-Scale Randomized Trials in Rwanda

Rowan P. Clarke, Manuel Barron, and Martine Visser

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The Short-Run Subsidies, Take-Up, and Long-Run Demand for Off-Grid Solar for the Poor: Evidence from Large-Scale Randomized Trials in Rwanda

Rowan P. Clarke, Manuel Barron and Martine Visser*

Abstract

Over a billion people lack access to electricity, instead relying on kerosene and other dirty lighting sources, while grid expansion is not expected to keep pace with population growth. Moreover, pneumonia is the leading cause of death for under-fives in the world and kerosene smoke is a significant risk factor. For-profit distribution of low-cost solar LEDs has been suggested as a solution, but adoption remains low, especially by the poorest. This study estimates demand curves for both the initial price of low-cost solar LEDs and the subsequent user fee for repeated purchases, while also estimating the impact of short-run subsidies, or a free trial period, on long-run demand. We find uptake is highly sensitive to price, with most households purchasing at zero price and none at full cost. Using unique big data on long-term usage, we show that households that received lights for free use them as much as those that paid, disproving the notion, in this context, that consumers will not use goods received for free. Finally, we find short-term subsidies for user fees actually increases long-term demand in the context of repeated purchases.

Keywords: subsidies; health; pricing; learning; energy; behavioral economics

JEL Codes: D11, D12, D83, I11, I18, O12

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1. Introduction

Subsidies are a common, but controversial, tool used by governments and development practitioners to overcome the poor's credit and liquidity constraints. In this paper, we study the role of subsidies to the fixed costs faced by households in adopting LED lights (i.e., the price of buying a light) and to the variable cost (the price of recharging the light). LED lights are a key tool to provide clean lighting to ultra-poor households given the low electrification rates in Africa, where 600 million people remain off-grid. Moreover, this figure is projected to rise to 700 million given the inability of grid expansion to keep pace with population growth (IEA, 2016; World Bank, 2018). Our study setting is Rwanda, a country with an impressive electrification program and higher rates of grid connections than most central and east African countries, where electrification rates are the lowest in the world (IEA, 2016; World Bank, 2018). However, as of 2018, only 12 percent of rural households were electrified, with the rest relying on dim, expensive, polluting, and harmful kerosene lamps, flashlights, fires or even simple sticks for their lighting needs (USAID, 2018).

Respiratory infections are the leading cause of death in the world for children under age five (Liu et al., 2016). A robust relationship between air pollution and infant mortality in Africa has recently been demonstrated (Burke et al., 2018), and experimental evidence from El Salvador indicates kerosene smoke has a significant impact on indoor air pollution (IAP) and child respiratory health (Barron and Torero, 2017). However, reaching rural households with the electric grid is expensive and can even reduce welfare, as documented by Lee et al. (2016a) in the case of Kenya. Moreover, even in places where the grid is available, most rural households are unable to afford the typically steep connection fees (Lee et al., 2016b). Therefore, off-grid solutions must be explored to provide clean energy to these households. In fact, the International Energy Agency projects that 70% of all rural households will have to rely on micro-grid or off-grid solutions (IEA, 2012).

International organizations like the World Bank propose distribution of solar-powered lights and home systems. For instance, the UK and US, via Power Africa, committed $1 billion to off-grid and small-scale solar solutions in Sub-Saharan Africa. Yet, adoption of these technologies remains low and there is some evidence to suggest that many poor households are unable to afford even the lowest-cost solar lanterns (Grimm et al., 2016b).

The question is, then, how to increase adoption of clean lighting among the poorest households, and whether this can be done in a financially sustainable way. In particular, what are the most effective pricing strategies to do so? Our implementing partner sells LED lights that are recharged by a solar-powered generator at a centralized recharge center in the village, with operations in over 1500 villages across Rwanda. Each village has a village microenterprise which sells single rechargeable LED lights to their community and provides a centralized, solar-powered recharge service for mobile phones and radios for a small fee. This is an innovative approach because centralizing power distribution lowers the cost per light and is a natural pay-as-you-go model with payments directly related to use. The pricing model depends primarily on two key factors: the price of the lights (fixed cost) and the recharging fee (variable cost). In our study setting, LED lights can be considered an experience good, because they are of significantly higher quality than alternatives and are not widely available in rural villages, leading to the existence of information frictions. In this case, the lack of knowledge of product quality and its benefits may hinder demand. There is therefore scope for learning and habit formation. We conduct two randomized controlled trials to experimentally vary both the fixed and the variable costs to identify the optimal pricing strategy from the social enterprise's perspective.

In the first field experiment, we study the effects of subsidies to the upfront fixed costs (the price of purchasing the light) on take-up. In this experiment, 1987 households were randomly assigned discount vouchers for the price of lights, with one of seven possible subsidy levels ranging from 0 to 100% of the retail price of 3000 RWF ($10 Purchasing Power Parity or PPP[1]). In line with related literature, we find demand is highly elastic: charging the full retail price reduces demand by 88 percentage points relative to a full subsidy. Next, we show that usage is unrelated to the price paid by the household. This is an important finding, because using the light requires paying the recharge fee. Hence, there could be a concern that households that are not able or willing to pay for the light will not use it at all. However, in our setting, the price paid by the household does not act as a mechanism to screen out those less likely to use the product.

In the second field experiment, we explore the role of introductory subsidies to the variable cost (the price of recharging the light). In this experiment, a sample of 2867 households received lights for free, together with randomly assigned recharge coupons which vary the recharge fee from 0 to 120 RWF. These coupons were valid for three months, and after that point the recharge fee was established at our partner's status-quo rate of 100 RWF. Light usage during the introductory period was highly price-sensitive. For instance, a 50 percent subsidy on the variable cost increased the number of recharges by 57 percent relative to the comparison group (100 RWF) while a full subsidy increased it by 156 percent. Upon expiration, these introductory prices could either boost or hinder demand for recharges. The former would happen if these subsidies allowed households to learn first-hand about this new technology and to get used to the light quality and

[1] Rwanda PPP as of 2017 is 305.71. Available from the World Bank at: https://data.worldbank.org/indicator/PA.NUS.PPP

indoor pollution reduction provided by the LED light, while the latter could happen if households use the introductory prices as anchors. We find that the former effect prevailed, as higher introductory subsidies increased the number of recharges during the three-month period following their removal. For instance, full subsidies increased subsequent usage by 133 percent compared to the control group. We conclude, therefore, that introductory subsidies increased demand for recharges, and thus habit formation and learning outweigh any price-anchoring effects. Data from a follow-up survey suggest that habit formation is a more important driving channel than learning.

In both experiments, we leverage unique data collected with remote sensing technology. Each recharge station is equipped with a device that records the ID number of the light to be recharged together with a time stamp. This information is sent by Global System for Mobile communications technology (GSM) to our servers. Thus, our data measure objective usage rates, which have advantages over self-reported data from customers or the sales teams, like Hawthorne effects or social desirability bias (Zwane et al., 2011). Moreover, this infrastructure transmits sales data in real time, a crucial factor for many for-profits. We complement our usage data with household surveys and GPS data.

Our main contribution is to the literature on pricing products for the poor. Most of this literature has evolved around health products. Some influential studies show that heavy subsidies are necessary for poor households to take up health products. For instance, Kremer and Miguel (2007) show that removing subsidies to deworming pills reduced take-up by 80 percent. In line with this finding, Cohen and Dupas (2010) show that high subsidies are necessary for poor households to access insecticide-treated mosquito nets, but also show that these subsidies do not reduce the likelihood that households use the bed nets. In a similar setup, Dupas (2014a) shows that subsidy recipients were more likely to purchase a bed net at full price one year later, even though their first bed net was still operational (they have a life span of 4 to 5 years). However, other studies find opposite effects, like Ashraf et al. (2010), who show that subsidizing home water purification systems in Zambia reduced their subsequent usage. In addition, Fischer et al. (2016) find that free distribution reduced subsequent demand of three curative health products (Panadol, Elyzole, and Zinkid) in Uganda. A key difference between the products analysed by Ashraf et al. (2010) and Fischer et al. (2016) compared to Cohen and Dupas (2010) and Dupas (2014a) is that the former require frequent purchases, while a bed net can last for several years. We contribute to this literature, providing evidence that learning and habit formation can play a role in the adoption and usage of a good with a two-part tariff, with an application to clean lighting technologies.

The studies closest to ours are Dupas (2014a) and Fischer et al. (2016). These studies have an initial wave with subsidized prices and a follow-up measurement with a uniform market price where additional units of the product are offered for sale. The key difference of our study is that we study a good with a two-part tariff and repeated purchases (over a six-month period). By showing that habit formation can play a key role in usage even among the poorest households, we also contribute to the growing literature which tests behavioral economics and marketing in the field, as well as the literature on incentive-based behavioral interventions (Mochon et al., 2017) especially related to business models targeted at the 'base of the pyramid' (e.g., Becker and Murphy, 1988; Mochon et al., 2017).

The rest of this paper is organized as follows. Section 2 briefly outlines the related literature and background. Section 3 examines the experimental designs and data. Section 4 details the empirical results from field experiments one and two. Section 5 discusses the findings and concludes.

2. Background and Related Literature

Understanding models for the effective distribution of renewable lighting to the ultra-poor is crucial given evidence on the effects of electrification on health, studying patterns, increased labor supply and productivity in housework, and more convenience during leisure activities (Dinkelman, 2011; Furukawa, 2012; Lipscomb, Mobarak and Barham, 2013; Sovacool and Drupady, 2016; Grimm et al., 2016a; Barron and Torero, 2017). However, newer evidence suggests smaller impacts, with grid electricity potentially decreasing welfare given the high costs faced by rural consumers (Lee et al., 2016a). A low-cost solution which displaces dirty lighting is then essential to fill this gap.

For these reasons, for almost a decade, the International Finance Corporation (IFC) has actively pushed the idea of for-profit distribution of low-cost solar lanterns and larger home solar systems via its Lighting Africa initiative (Lighting Africa, 2012). However, a major challenge facing social enterprises has been to distribute clean, reliable off-grid lighting to the rural poor in a financially sustainable way. Preliminary evidence suggests that current models, even when successful, fail to reach the ultra-poor (Wong, 2012; Grimm et al., 2016a).

A frequent concern in development policy is that one-off subsidies might reduce long-term demand via reference-dependent preferences—also known as price anchoring (Dupas, 2014a). People could anchor on the subsidized price and not be willing to pay the full price later (Köszegi and Rabin, 2006; Simonsohn and Loewenstein, 2006; Dupas, 2014a). The situation is more complex when there is scope for positive learning—i.e., if a free trial gives users a chance to learn about the positive benefits of a product. We call the good an experience good if information frictions exist and households have previously underestimated the good's value. In this case, it is possible that short-term subsidies or a reduced pricing strategy could increase long-term demand via the elimination of information frictions and resultant positive learning; as consumers come to value the product more, they are willing to pay a higher price (Fischer et al., 2016).

For learning to have a positive effect on subsequent demand, consumers must use the product. Many argue that if products are given away for free, households will not use them, thereby reducing the screening effect of prices—where only households which will use a good are willing to pay a positive price for it (Cohen and Dupas, 2010; Ashraf, Berry and Shapiro, 2010; Chassang, Padro i Miquel and Snowberg, 2012)—and, conversely, consumers use a good more because they have paid for it.

Other than removal of information constraints and positive learning, another factor which could counteract the effects of price anchors is habit formation: consumers could become used to an increased amount of a product and this behavior could decay slowly over time, even when the subsidy period is over. Indeed, a behavior can rationally persist when past consumption levels affect current consumption (Becker and Murphy, 1988).

There exists an extensive literature on how non-budget constraint factors affect demand. The marketing, psychology, and economics literature finds a large role for price anchors: price histories play a significant role in subsequent demand, such that lower initial prices are anchored on making consumers unwilling to pay higher prices later. The evidence from this literature draws primarily from lab experiments but also supermarket scanning data (Kalyanaram and Little, 1994; Fischer et al., 2016). In contrast, evidence from development field experiments largely finds no role for these non-budget constraint effects on prices. For example, Cohen and Dupas (2010) show in the context of insecticide-treated bed nets that demand is highly price-elastic: charging even a small positive price reduces demand substantially and does not lead to increased use. They therefore find no role for screening effects. Kremer and Miguel (2007) find similar results in the context of deworming pills in Kenya: charging even a small price dramatically reduces demand. Moreover, Dupas (2014a), again in the context of antimalarial bed nets, finds that short-run subsidies actually increase long-run demand through positive learning about the value of the product—such that a lower price today increases later demand at full price. In this case, the positive learning effect outweighs the negative effect of price anchoring. This evidence from development field experiments led to a "loose policy consensus" on the free distribution of health products given: *i*) very high subsidies are necessary to increase initial adoption, *ii)* households use goods they paid low or zero prices for and *iii)* short-term subsidies actually raise long-term demand (JPAL, 2011; Dupas, 2014b; Fischer et al., 2016). The conclusion is subsidies, or reduced prices, are required to effectively reach the poor.

This consensus, however, is no longer watertight. For instance, one early study which did not fit the body of evidence, in the context of water chlorination, found prices play a screening role such that higher initial prices stimulate subsequent use (Ashraf et al., 2010). Most significantly, Fischer et al. (2016) find a large role for price anchors: free distribution of medication lowers long-term demand consistent with the predictions of models of reference-dependent preferences (Simonsohn and Loewenstein, 2006; Köszegi and Rabin, 2006; Mazar et al., 2013; Heidhues and Koszegi, 2014**;** Fischer et al., 2016). Given how results are frequently contradictory, the authors call for more research and replication in different contexts and for exploring other important drivers (Fischer et al., 2016).

3. Experimental Design and Data

3.1 Experimental Designs

This paper leverages two large-scale randomized field experiments in a sample of over 5000 households in Huye and Ruhango, two districts in rural Rwanda. Our main aim is to understand the drivers of demand for lights, noting that using a light incurs two types of cost: a fixed cost to purchase the light, and ongoing variable costs (the recharge fee). The first field experiment exogenously varied the price of the lights, while the second intervention varied the marginal cost of usage.

In the first randomized control trial, lights are sold to households, which is the initial stage in the business model. We randomly allocated discount vouchers for the light, de-facto randomizing the price faced by consumers. We analyze data on light take-up and usage. A key innovation in our experiment is that each recharge was automatically time-stamped by the recharging station, and the information was sent to our implementing partner by GSM. Administrative data sidesteps recall problems or other sorts of measurement error.

In our second field experiment, households receive free lights but face randomized pay-as-you-go (PAYG) usage costs, in the form of light recharge fees. This experiment consisted of introducing randomly allocated recharge subsidies for 3 months. We study the impact of this variation on usage during the subsidy period and usage after the subsidies are removed. Recharge data was recorded as in the first experiment.

Both experiments were implemented in the field by Innovations for Poverty Action (IPA). IPA staff, in close collaboration with the research team, explained the study to village leaders and got the leaders' and households' consent, conducted the randomizations, assigned vouchers, and collected the data. Household surveys in the second field experiment were collected electronically, with a number of consistency checks and audits as implemented by IPA. IPA staff, in collaboration with the research team, created a data quality assurance plan and materials before launching the surveys. This plan lays out in detail the requirements for back checks, high frequency checks, accompaniments, and spot checks. The surveys were bench-tested (in the office) and piloted (in the field) prior to launching. Field supervisors and a research coordinator accompanied the survey teams.

3.1.1 Randomization of the Price of Lights

The first field experiment was conducted in rural Rwanda in Huye and Ruhango districts –districts broadly representative of rural Rwanda and East Africa in general. We present household socioeconomic summary statistics in Table A4 in the appendix. Following the methodology of Cohen and Dupas (2010), Dupas (2014a) and Meredith et al. (2013), a sample of 1987 households from 18 villages were randomly assigned discount vouchers for the price of lights. The experiment started in 6 villages from Huye in January 2016 and 12 villages from Ruhango in January 2017. Villages were selected according to our partner's business model as follows: they were located in rural areas, had no existing or planned grid connection, and had at least 90 households.

First, we obtained a list of all households in each study village. Thereafter, randomization was done at the household level, stratified at the village level. Price randomization was implemented using discount vouchers which were handed out door-to-door by trained enumerators. Vouchers were later redeemed for lights at the village recharge center in the presence of our partner and field staff. Following the business model of our partner meant that we had to make the vouchers valid for only a few days.

There were nine prices possible prices: 0, 200, 300, 500, 800, 1000, 1500, 2000, and 3000 RWF, so discounts ranged from a 100% subsidy of 3000 RWF ($10 PPP or roughly $4 in current dollars) to no subsidy. Vouchers were printed with the actual price faced by consumers as well as the size of the discount and the market price of lights. The market price advertised, however, was 3000RWF, as our implementing partner had already determined that the actual market price of 4500RWF was too high. The main outcome variables in this experiment are light take-up and usage. Usage is measured by the number of times a household recharged its light during the study period.

3.1.2 Randomization of the Usage Fee

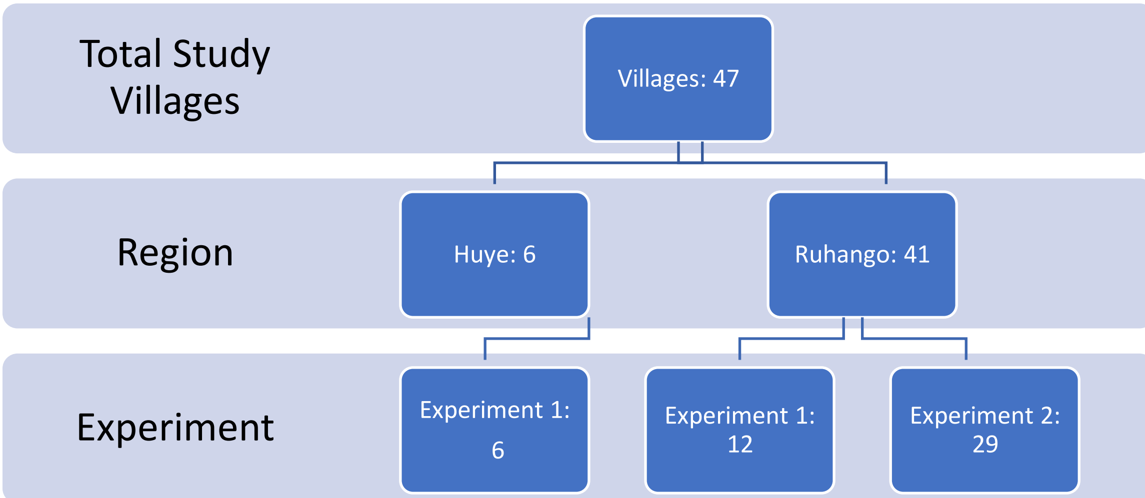
This field experiment focused on randomly varying the recharge fee faced by customers. Households received a light for free and faced differing usage fees ranging from 0 to 120 RWF per recharge. This experiment was carried out in Ruhango district. This component of the study contained a sample of 2,867 households and lights (one light per household) from a total of 29 villages or enumerator areas.

First, a list of households in each village was obtained. We recruited households who desired to acquire our partner's lights, without revealing that we would give them away for free. Then, coupon-light combinations were randomly distributed in fixed proportions within each village, i.e., stratification was done at the village level. There were around nine households per village per treatment arm, and seven price treatments: 0, 50, 60, 70, 80, 100 and 120 RWF. Our partner's standard fee is 100 RWF per recharge, which we use as the comparison category. In a similar fashion as the first experiment, random assignment of recharge prices was achieved using recharge coupons which were redeemable at the village-level microenterprise each time the household wanted to recharge its light. The coupon system was designed to operationalize the price randomization and to ensure the internal validity of the experiments by limiting potential arbitrage. Vouchers featured the name of the household head and the light ID number, and the recharge only took place if the voucher corresponded to the light and the household. It should also be noted that each household received only one light and explicitly put their name down as wanting a light in the initial listing phase, so it seems unlikely they would subsequently sell their only light. Our surveys confirm this, as 98% of households recharged their lights during the study period.

When a free light was distributed to a household head, it came with an associated coupon card. This card included the name and date of birth of up to two household heads, the serial number of the light assigned to that household, the recharge price for that light, and the expiration date of the coupon (i.e., after three months). Consumers were also aware that the recharge prices were the result of a lottery. Consumers were happy with the system and believed it to be fair. A card included 15 perforated coupons, each with a unique coupon ID. At every recharge, the household head would bring their government issue ID document, and a coupon would be torn off and handed to the entrepreneur, who was instructed to confirm that the name and date of birth and light serial on the card matched the ID document and the light serial. Given a match, the entrepreneur entered the specific coupon code into the charger and recharged the light. The light serial number, coupon code, and timestamp were then automatically transmitted to our cloud database; if there was a match, the Village Level Entreprenuer (VLE) was reimbursed the difference between the full price and the discounted price, thereby incentivizing honesty by all parties.

The coupons and thus experimental interventions ran for a total of three months (which we term Phase II, short-run demand). Thereafter pricing reverted to the standard recharge price of 100RWF per charge. We then tracked usage rates for three subsequent months (for a total study-length of six months), to see the extent to which behavior persisted or decayed after the interventions had concluded.

Figure 1: Details of Experimental Design



	Field Experiment 1	Field Experiment 2
Location	Huye: 6 villages Ruhango: 12 villages	Ruhango: 29 villages
Randomized Trial	Fixed cost of the light	Variable cost of the light
Stratification	village-level	village-level
Cost of the light	0, 200, 300, 500, 800, 1000, 1500, 2000, and 3000 RWF	Free
Recharge fee	100 RWF	0, 50, 60, 70, 80, 100 and 120 RWF for 3 months, 100 RWF for all thereafter
Track Usage for total	6 months	6 months
Follow up surveys	None	GPS survey 29 villages Customer survey in subset of 12 villages in Ruhango limited by budget 1000 HHs

3.2 Data

There are five sources of data utilized in this paper. We outline these here and break each down specifically for the two randomized trials. The sample in the first experiment includes 1987 households from 18 villages. There are two sources of data for these households. First, we collected information on the pricing voucher offered to each household in this sample, whether the household purchased a light, a household identifier, the light serial number and the recharge-center serial number, as well as village and district information.

Second, we developed data collection technology to gather recharge data remotely via GSM. Lights are recharged at the villages' recharge centers. Therefore, to objectively measure usage statistics, individual lights were programmed to communicate with the recharge center and the recharge center was engineered to communicate via GSM to our cloud-based database. At each recharge, the light transmitted its serial number to the charger. The charger then recorded a date-time stamp, accurate to the minute, as well as the amount of charge delivered to the light, and the length of time on the charge. Thus, we have data on both the extensive margin (whether a household used a light) and the intensive margin (how much a household used a light), giving us a richer picture of usage patterns. Hence we rely on administrative data, not prone to social desirability bias, recall or experimenter demand effects (Zwane et al., 2011).

For the second field experiment (variable cost), we collected data from 2867 households in 29 villages. First, we collected pricing coupon data (which varied the recharge price or user fee). Here we recorded, for each household, the recharge price, coupon codes, household identifier, names and personal ID numbers of household heads, light and recharge-center serial numbers, and village and district information. Second, we implemented GSM data collection technology in this sample to objectively measure light usage behavior in the same fashion as in the first field experiment. Third, we collected Global Positioning Systems (GPS) location data, as well as the location of each recharge station. Since recharging requires visiting the recharge center, a household's distance from this center is a measure of the inconvenience faced by households in using their lights and could be an important factor driving usage; therefore, its inclusion in our models would increase the precision and reliability of our estimates, and we include this variable in robustness and balance tests. Fourth, in 12 of the study villages, we collected detailed follow-up data on 1,000 households. This customer household survey captures characteristics of households including demographics, income, energy usage and expenditures, as well as questions to investigate the role of information constraints and other factors on consumer demand.

4. Experimental Results

In this section we report the results of our field experiments. In the first experiment, we vary the fixed cost of the light and study its impact on (i) light take-up and (ii) subsequent use. In the second experiment we vary introductory subsidies to the variable cost (pay-as-you-go fee), studying their effects (i) during the introductory 3-month period and (ii) 3 additional months after subsidies are removed. The effects of usage subsidies after the introductory period is this paper's main contribution to the literature.

4.1 The Impact of Light Price on Take-up

In the first experiment we offered subsidies to randomly selected households, varying the fixed cost of adopting the light as described in section 2. To study the effect of price on take-up, we estimate the following linear probability model:

$$Y_{iv} = \beta_0 + \sum_{j=200}^{j=3000} \beta_j P_{ivj} + \gamma_{iv} + \varepsilon_{iv} \tag{1}$$

where "Y_{iv}" is the outcome variable, in this case a dummy variable equal to 1 if household *i* in village *v* purchased a light. P_{ivj} is a treatment indicator variable which takes the value of 1 if household *i* in village *v* was offered the light at price *j*. The price options are 0 (full subsidy), 200, 300, 500, 800, 1000, 1500, 2000, and 3000RWF. Their associated coefficients, β_{200} through β_{3000}, are the coefficients of primary interest: they give the percentage point reduction in demand or take-up for each randomly assigned price treatment. Given randomization, these coefficients can be unbiasedly and consistently estimated by Ordinary Least Squares (OLS). Village fixed effects account for treatment stratification at the village level, and increase the precision of our estimates in the above equations (Bruhn and McKenzie, 2009). ε_{iv} is the idiosyncratic error term. Our preferred specification uses heteroskedasticity-robust White standard errors. Since treatment was allocated at the household level, there is no need to cluster standard errors (Abadie et al., 2017). However, we report results clustering at the village level for a robustness check.

The experimental results are shown graphically in Figure 2. The figure plots the share of households purchasing lights at each randomized price level. When lights are fully subsidized, the adoption rate is over 90 percent. One reason this take-up is not 100 percent is that households are aware that using the lights requires subsequent paying of the recharge fee. This would also signal that households were not intending to give away or sell their lights. The take-up rate falls sharply as the subsidy decreases, and at the full price of 3000RWF ($10 PPP), adoption is practically null.

Figure 2

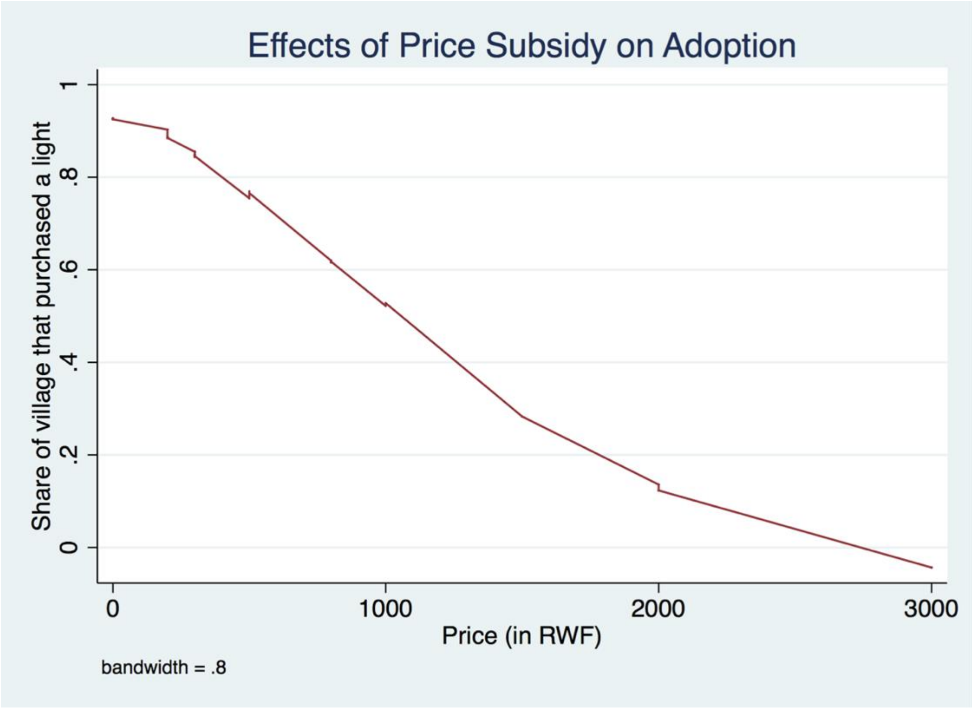

Effects of Price Subsidy on Adoption
Share of village that purchased a light
0
.2
.4
.6
.8
1
0
1000
2000
3000
Price (in RWF)
bandwidth = .8

Table 1: Light Price and Take-up

	1	2	3	4
Prices				
p200	-0.036*	-0.036	-0.020	-0.020
	(0.021)	(0.024)	(0.021)	(0.019)
p300	-0.019	-0.019	-0.051**	-0.051*
	(0.024)	(0.028)	(0.024)	(0.026)
p500	-0.155***	-0.155***	-0.171***	-0.171***
	(0.026)	(0.028)	(0.026)	(0.027)
p800	-0.251***	-0.251***	-0.284***	-0.284***
	(0.046)	(0.079)	(0.043)	(0.072)
p1000	-0.444***	-0.444***	-0.453***	-0.453***
	(0.035)	(0.072)	(0.034)	(0.069)
p1500	-0.700***	-0.700***	-0.732***	-0.732***
	(0.043)	(0.077)	(0.042)	(0.077)
p2000	-0.841***	-0.841***	-0.857***	-0.857***
	(0.025)	(0.037)	(0.025)	(0.037)
p3000	-0.892***	-0.892***	-0.879***	-0.879***
	(0.027)	(0.028)	(0.033)	(0.035)
Village Fixed Effects	No	No	Yes	Yes
Constant	0.891***	0.891***	1.092***	1.092***
	(0.018)	(0.053)	(0.031)	(0.023)
Observations	1,987	1,987	1,987	1,987
R-squared	0.403	0.403	0.451	0.451

Notes: The outcome variable is a dummy that indicates whether the household acquired the lights at the randomly assigned price. Heteroskedasticity-robust White standard errors are reported in columns 1 and 3. Standard errors in columns 2 and 4 are clustered at the village level. Statistically significant at 90(*), 95(**), and 99(***) percent confidence.

Results are reported in Table 1. The coefficients of all prices, except the lowest price of 200RWF, are statistically significant and economically large. When lights are offered at a price of 300RWF, demand falls by 5.1 percentage points relative to when the light is offered for free. At 500RWF, demand drops by 17.1 percentage points. When 1000RWF is charged, demand falls by 45 percentage points. Each price increase reduces demand further, with the highest price of 3000RWF ($10 PPP) reducing demand by 88 percentage points. The high price-sensitivity of demand indicates either that households face financial (credit, savings, or liquidity) constraints, or that they do not value lights as much as their market price. The finding that price is a significant factor in investment in solar lights is in keeping with the literature of preventive health products reviewed above. In particular, results are in line with those from insecticide-treated bed nets in Cohen and Dupas (2010) and Dupas (2014a, 2014b), medications in Fischer et al. (2016) and deworming pills in Kremer and Miguel (2007). Moreover, Meredith et al. (2013), in the context of multiple health products, finds 78% of demand is driven by prices alone; the authors argue their results highlight the importance of subsidization. Our findings provide further support for the subsidization of health-related products generally, and more specifically, low-cost solar LEDs.

There are two caveats worth considering before moving forward. First, it should be noted that these effects may be an upper bound on the true effect of prices on demand. This is because it is possible that households were especially price sensitive because they were aware they were receiving different prices via the voucher system. Although there was only a day or two between receiving the vouchers and when they were redeemed, it is certainly possible households discussed the prices they received with their neighbors. However, the evidence in sections 4.2 and 4.3 shows that households were sensitive to price over a 6-month period, even after subsidies were removed, so we believe this is not a substantial source of bias. Second, it is also possible that households were nudged into purchasing by the fact that the vouchers had an expiration date, or that liquidity constraints for those facing the high prices may have reduced demand and thus increased the elasticity measures. However, this reduction in the liquidity constraint would be part of the effect of the discount vouchers.

4.2 The Impact of Light Price on Light Usage

We have established that heavy subsidies are required if high take-up is to be achieved in our study setting. We next examine the implications of such a strategy on usage of lights. Note that using the light has a positive variable cost, so households that required heavy subsidies to take up the light may not be willing or able to use the light as much as households that adopted at a lower subsidy. To investigate this issue, we analyze light usage over the six months after light subsidies were offered. Does charging a high price for the light screen out those who will not use or do not need LEDs? Do initial prices act as a signal for how much consumers value the good, leading to higher usage by those that paid a nonzero price?

Many actors in development argue that positive prices should be charged since users will not sufficiently use goods if they are given away for free, as documented by Cohen and Dupas (2010). This possibility is even more important in our setting, where usage carries a fee, so that initial willingness to pay might predict ability to pay over time. We analyze this question using the data on the randomized upfront price as well as long-term usage patterns (6 months) remotely captured minute-by-minute via GSM technology.

To investigate this issue, we regress usage (defined as recharge frequency per light over the six months following the allocation of light subsidies), on upfront price paid, controlling for village fixed effects. Standard errors are robust to heteroskedasticity (once again, it is not necessary to cluster them because the level of randomization is the household). We estimate the following equation:

$$Y_{iv} = \beta_0 + \beta_1 Price_{iv} + \gamma_v + \varepsilon_{iv} \qquad (2)$$

where "Y_{iv}" is usage, or the recharge frequency (and the inverse hyperbolic sine transformation of recharge frequency), for light *i* in village *v*. Recharge frequency is the number of times a light was recharged over the first 6 months after distribution of subsidies. $Price_{iv}$ is the upfront price level a household faced and takes the following values: 0, 200, 300, 500, 800, 1000, 1500, 2000, and 3000RWF. Village fixed effects account for unobserved village-level heterogeneity. ε_{iv} is the error term. β_1 is the coefficient of primary interest: it gives the impact of upfront price paid on subsequent usage. An important caveat to note here is that the sample size is reduced because it consists only of households that purchased a light (whether at zero price or any positive price). It is only within this sample of households that usage can be defined and tracked.

Alternatively, we estimate regression (2) more flexibly, as follows:

$$Y_{iv} = \beta_0 + \sum_{j=200}^{j=3000} \beta_j P_{ivj} + \gamma_v + \varepsilon_{iv} \qquad (3)$$

where "Y_{iv}" is the recharge frequency (or IHS of recharge frequency) for light *i* in village *v*. P_{ivj} is a treatment indicator variable which takes the value of one if light *i* was randomly assigned price *j* in village *v*. Price categories *j* are 200, 300, 500, 800, 1000, 1500, 2000, and 3000RWF, while 0 is the comparison category. If *Y* is recharge frequency, then β_0 gives the average usage in the reference village when the price is 0. β_{200} through β_{3000} are the coefficients of interest: they give the change in usage for each randomly assigned price treatment. All but two of the eight coefficients are negative and only one price is robustly significant: having paid the highest price (3000RWF) is associated with a reduction in long-term usage relative to those that paid zero price.

Results are reported in Tables 2 and 3. Column 1 presents the results with the outcome variable in levels. For an alternative interpretation and as a robustness test, column 2 presents results with the outcome variable transformed using the inverse hyperbolic sine transformation (IHS). It thus gives an approximate percent effect of price on usage. We use the IHS transformation instead of the natural logarithm, because recharge frequency, or usage, has a non-trivial number of zero values (Burbidge et al., 1988). This is becoming standard practice in the empirical literature (for example, see the influential paper by Haushofer and Shapiro (2016)).

Table 2: Impact of Free Lights on Subsequent Usage

	Outcome Variable:	
	Recharge Frequency Per Light	
	Level	*In Hyp Sin*
Purchase Price	**0.109**	**-0.014**
	(0.322)	(0.058)
Village Fixed Effects	YES	YES
Observations	1,377	1,377
R-squared	0.171	0.140

Notes: The table reports the coefficients from OLS regressions where the dependent variable is number of recharges per light and the explanatory variables are purchase price and village fixed effects. Treatment was assigned at the household level and stratified at the village level. Heteroskedasticity-robust White standard errors in parentheses. Statistically significant at 90(*), 95(**) and 99(***) percent.

Upfront price has no overall statistically significant effect on subsequent usage rates over a 6-month period. This is a strong indication that price paid for the light does not signal how much a consumer would subsequently use the light. The coefficients are small in magnitude and differ in sign. The more flexible specification in Table 3 provides further support to this argument. All but two of the eight price coefficients are negative and only one price is robustly significant: having paid the highest price (3000RWF) is associated with a reduction in light usage relative to households that received the light for free.

This is strong evidence against the hypothesis that paying for a light means a household uses a light more. This result is in line with Cohen and Dupas (2010) and Dupas (2014a), who find households given antimalarial bed nets for free still valued and used them, but not with Ashraf et al. (2010), who find charging a higher price in the context of water chlorinators leads households to subsequently use the good more. This could also be evidence of the presence of liquidity constraints: households may simply be unable to afford to pay high lump sum prices for lights, yet when they can afford them they do use them, even where use carries a user fee.

Table 3: Impact of Free Lights on Subsequent Usage

	Outcome Variable: Recharge Frequency	
	Level	*In Hyp Sin*
	1	2
Prices		
p200	-0.376	-0.144**
	(0.302)	(0.065)
p300	-0.044	-0.093
	(0.314)	(0.074)
p500	-0.053	-0.077
	(0.333)	(0.065)
p800	-0.041	-0.042
	(0.411)	(0.098)
p1000	-0.325	-0.120
	(0.516)	(0.092)
p1500	0.721	0.127
	(0.734)	(0.179)
p2000	1.301	0.064
	(1.627)	(0.237)
p3000	-2.774***	-0.309***
	(0.495)	(0.088)
Village Fixed Effects	YES	YES
Constant Intercept	2.037***	0.889***
	(0.298)	(0.083)
Observations	1,377	1,377
R-squared	0.175	0.145

Notes: The table reports the coefficients from OLS regressions where the dependent variable is number of recharges per light. The level of randomization is the household. Treatment was stratified at the village level. Heteroskedasticity-robust standard errors in parentheses. Statistically significant at the 90(*), 95(**), and 99(***) percent.

4.3 The Impact of Introductory Usage Subsidies on Light Usage in the Context of Repeated Purchases

We have presented evidence that subsidies are required if high take-up is to be achieved and that light usage does not decrease if households are offered a higher subsidy. We now turn to examining consumer behavior after a light is already owned and consumers must pay a small pay-as-you-go (PAYG) user fee for repeated purchases (i.e., to recharge their light). In this section we ask, how do short-run usage subsidies affect adoption as well as demand in the context of repeated use carrying a user fee?

The current literature focuses on only two purchases: an initial purchase where the price is randomly varied and a subsequent purchase, which then allows estimation of the impact of the initial price paid on *additional* purchases. In this context, Dupas (2014a) finds short-run subsidies actually increase long-run demand for insecticide-treated bed nets and argues this is due to positive learning about the value of the product that a subsidy provides. Fischer et al. (2016) find the reverse: short-run subsidies decrease long-term demand for three medications due to anchoring on short-run prices. We extend this literature by focusing on multiple purchases. In this context, how do short-run subsidies, in the form of a three-month trial of reduced prices, affect light usage during the introductory period? What happens with usage upon removal of the subsidies?

4.3.1 The Impact of Introductory Usage Subsidies on Repeated Use

First, we ask, how do short-run subsidies on the variable cost affect light usage in the context of repeated purchases? To answer this question, we vary the user fee faced by consumers via the random assignment of discount coupons valid for a period of three months and collect data on the usage of lights, measured by the number of times a light is recharged. We check how effective randomization was at balancing covariates. We do not have consumer pre-intervention data so we are unable to carry out broader balance tests. However, we test for balance in distance from the household to the recharge center computed via GPS, a characteristic that is very unlikely to have been affected by our experiment. In forthcoming work, we show that distance is a significant predictor of light usage, so it is a particularly important variable to examine. In Table A2 in the appendix, we regress 'treatment', or each price dummy, on village dummies and linear distance from the recharge center. Distance and village do not predict any price dummy, indicating that randomization is balanced across this important driver of usage.

In this section our main estimating equation is:

$$Y_{iv} = \delta_{100} + \sum_{j=0}^{j=120} \delta_j \, P_{ivj} + \gamma_v + \varepsilon_{iv} \tag{4}$$

where "Y_{iv}" is the outcome variable—in this case, the recharge frequency for household *i* in village *v*. P_{ivj} is a treatment indicator variable which takes the value of one if household *i* in village *v* was randomly assigned recharge price j, which consists of 0, 50, 60, 70, 80, 100 and 120RWF. The comparison category is 100RWF, the usual fee charged by our implementing partner. Village fixed effects are included because treatment assignment was stratified by village. Given that treatment is allocated at the household level, in our preferred specification we do not cluster the standard errors and instead report the heteroskedasticity-robust White standard errors (columns 1 and 2). As a robustness check we report results clustering standard errors at the village level (columns 3 and 4). As in previous tables, the levels of statistical significance do not change much when we cluster at the village level. We also report specifications with recharge frequency transformed using the IHS transformation as robustness tests.

The coefficients δ_0 through δ_{120} are the coefficients of interest: they estimate the change in recharge frequency for each randomly assigned price treatment relative to the control of 100RWF. Table 4 presents the results of regressing recharge frequency on the usage fees paid by consumers, using OLS. During the three months that the discount coupons were active, subsidies have statistically and economically significant effects on usage. Usage is highly price-sensitive: offering free recharges increases the number of recharges by 2.8, which is a 156 percent increase in usage relative to the comparison category of 100RWF. In turn, charging a price of 50RWF increases usage by 67 percent compared to the full price. As expected, higher recharge costs reduce the frequency of recharge. The only exception is the recharge cost of 120RWF, which is not significantly different from 100RWF. Hence, in the short run, repeated use is highly price-elastic, as is the demand for lights. Therefore, a reduced pricing strategy or subsidies may be required to increase usage.

Table 4: Impact of Short-Run User Subsidies on Short-Run Usage

	Outcome Variable: Recharges per light			
	Level	In Hyp Sin	Level	In Hyp Sin
	1	2	3	4
Price 0	2.802***	0.833***	2.802***	0.833***
	(0.225)	(0.067)	(0.420)	(0.106)
Price 50	1.190***	0.408***	1.190***	0.408***
	(0.204)	(0.070)	(0.234)	(0.074)
Price 60	0.712***	0.250***	0.712***	0.250***
	(0.195)	(0.071)	(0.233)	(0.089)
Price 70	0.492**	0.158**	0.492**	0.158*
	(0.195)	(0.071)	(0.230)	(0.088)
Price 80	0.325*	0.120*	0.325	0.120
	(0.183)	(0.069)	(0.244)	(0.088)
Price 120	-0.060	-0.070	-0.060	-0.070
	(0.190)	(0.072)	(0.183)	(0.081)
Village Fixed Effects	Yes	Yes	Yes	Yes
Control group mean	1.794	0.752	1.794	0.752
	[2.295]	[0.719]	[2.295]	[0.719]
Observations	2,867	2,867	2,867	2,867
R-squared	0.302	0.331	0.302	0.331

Notes: The table reports the coefficients from regressions of the dependent variable, recharges per light, on randomized treatment indicators and village fixed effects. Prices were randomized at the household level, with village-level stratification. The reference category is 100 RWF per recharge (the status quo price). The control group mean and standard deviation, in brackets, are also presented for comparison purposes. Columns 1 and 2 report heteroskedasticity-robust White standard errors and columns 3 and 4 report standard errors clustered at the village level. Statistically significant at 90(*), 95(**), and 99(***) percent.

4.3.2 Usage Frequency after Removal of Introductory Subsides

In this subsection, we study light usage after the introductory recharge subsidies are removed. This is the core contribution of this paper. Recent research shows that incentive-based behavioral interventions can have long-lasting effects beyond the treatment period (Mochon et al., 2017). Short-run subsidies can be thought of as a price-incentivizing habit formation. A related hypothesis, popular in the literature, is that free trials increase learning and thus boost future demand: a free trial can remove information frictions because it allows uninformed consumers to use the product and learn its benefits over existing alternatives, which leads to higher use and willingness to pay in the long run (Dupas, 2014a).

The estimating equation is the same as in the preceding section, but the outcome of interest is the number of recharges after the introductory subsidies were removed and the recharge fee was set to 100 RWF. Results are reported in Table 5. Column 1 presents the main result in the paper: households that were randomly assigned free recharges for three months recharged 133 percent more over a three-month period *after* the subsidy was removed than households that were initially assigned the full price of 100 RWF. This result is statistically significant and practically meaningful. Furthermore, households that were assigned to pay 50 or 60 RWF per recharge also used their lights more: approximately 71 percent more than households assigned the full price over the post-intervention period. The effect fades and becomes not statistically significant among households that had to pay fees above 70 RWF per recharge.

It is interesting to notice that households do not reach the maximum number of available recharges even when user fees are zero. Therefore, factors other than price must also be driving take-up and long-term adoption. One possibility is the inconvenience costs associated with traveling to the village-level centralized solar recharge center. To increase the precision and validity of our estimates, we collect GPS data on the distance of each household from the centralized recharge center, and we control for this proxy in robustness tests which we present in the appendix. We study the impact of inconvenience in more detail in forthcoming work. Here we note that including distance as a covariate marginally reduces the size of the price coefficients. For instance, the coefficient on the impact of the free trial intervention is .348 before and .337 after inclusion of distance in our model (Table A1 in the Appendix; these numbers represent the number of additional recharges in the post-intervention period). Therefore, we can be confident our main results are not biased because the price treatments are orthogonal to inconvenience. Thus, in the context of repeated use with a usage fee, usage increases upon removal of introductory subsidies compared to status quo pricing.

4.3.3 Mechanisms

Given the importance of price anchoring and reference dependence preferences found in the psychology, economics, and marketing literatures, one might expect consumers would anchor on the subsidized user fee and thereafter be unwilling to pay full price. Instead, in the previous section we show that introductory subsidies increased light usage after subsidies were removed. The positive influences of habit formation and learning thus outweigh the negative ones of price anchoring and reference dependence. That begs the question: between habit formation and positive learning, which is the primary mechanism? In this section we shed light on this question while forthcoming work examines this in more depth.

Information frictions and learning could drive our finding if those that received higher subsidies learned more about the positive benefits of LEDs which then led to increased long-run use. To answer this question, we interviewed 1000 households in Ruhango district with questions designed to provide empirical evidence on information about the light. This includes questions on the product’s characteristics that only informed consumers would know. We study changes along the intensive and extensive margins of knowledge. To uncover extensive-margin changes, we study the percentage of households that used their free lights and so learned at least some of their benefits. To investigate changes along the intensive margin, we exploit the exogenous variation in usage arising from the randomly allocated subsidies that generated differences in knowledge of product characteristics. If not all consumers used their lights, and those that used them more also learned more about their benefits, then we could conclude learning and removal of information frictions are the likely drivers of increased long-run demand.

The survey data makes this seem unlikely. First, the majority of households still use their lights 18 months after they received them. Most significantly, 98 percent of households report having used their light at least a few times, so customers have some experience with this new product. Second, exogenous variation in price, and thus usage, is not related to product knowledge. We present this evidence in Table 6, where the reference category is the fully subsidized recharge prices.

No coefficient is statistically significant, and all are very small. For example, in the first row of column 1, households that paid the full price are 2 percent more likely to say it is brighter than alternatives. If this group had learned less, because of lower usage rates, the coefficient should instead be negative, larger, and statistically significant.

Finally, there is another reason that we believe habit formation, and not information frictions and learning, is the most likely driver of our result. As previously shown in Table 5 above, usage after removal of subsidies also varies with the introductory subsidy levels, i.e., there is a 'dose-response' relationship between the user fee consumers paid during the intervention period and usage in the months after. Indeed, there is persistence in behavior across all price levels. It is likely that households continued this behavior, once the free trial or discounts were removed, because they had become accustomed to using a certain amount of light, i.e., a new habit was formed, and not because they were learning more about the technology's benefits.

Summing up, first, our data show that 98 percent of households used their lights at least a few times. This likely led to the removal of any information frictions, allowing consumers to learn product benefits first-hand. Second, higher use is not causally associated with higher knowledge, making it unlikely that user fee subsidies led to greater knowledge of the benefits of this new technology which in turn drove higher long-run demand. Thus, information frictions and learning cannot explain our result. We argue instead that subsidies allow habit formation to be cemented and this leads to higher long-run demand, and does so in a dose-response manner across multiple price subsidies, even after the subsidy period is over.

Table 5: Long-Run Impact of Short-Run Subsidies

	Outcome Variable: Recharges per light			
	Level	In Hyp Sin	Level	In Hyp Sin
	1	2	3	4
Price 0	0.343***	0.147***	0.343*	0.147**
	(0.107)	(0.044)	(0.192)	(0.065)
Price 50	0.179**	0.088**	0.179	0.088
	(0.088)	(0.040)	(0.107)	(0.052)
Price 60	0.207**	0.078*	0.207	0.078
	(0.103)	(0.042)	(0.160)	(0.060)
Price 70	-0.021	0.001	-0.021	0.001
	(0.074)	(0.038)	(0.087)	(0.040)
Price 80	0.145	0.039	0.145	0.039
	(0.097)	(0.040)	(0.137)	(0.053)
Price 120	0.105	0.026	0.105	0.026
	(0.090)	(0.040)	(0.088)	(0.036)
Village Fixed Effects	Yes	Yes	Yes	Yes
Control group mean	0,254	0,128	0,254	0,128
	[0.909]	[0.359]	[0.909]	[0.359]
Observations	2,867	2,867	2,867	2,867
R-squared	0.181	0.192	0.181	0.192

Notes: The table reports the coefficients from regressions of the dependent variable, recharges per light, on randomized treatment indicators and village fixed effects. Prices were randomized at the household level, with village-level stratification. The reference category is 100 RWF per recharge (the status quo price). The control group mean and standard deviation, in brackets, are also presented for comparison purposes. Columns 1 and 2 report heteroskedasticity-robust White standard errors and columns 3 and 4 report standard errors clustered at the village level. Statistically significant at 90(*), 95(**), and 99(***) percent.

5. Conclusion

Available evidence shows that off-grid rechargeable solar LEDs can lead to household savings on lighting expenditures over time, have longer lifespans, and are of higher quality than alternatives. They can also be expected to have health impacts, as well as environmental impacts if adopted at scale. We implement pricing interventions designed to address liquidity constraints and remove information frictions among potential customers of a new technology. We show, however, that rural households fail to invest in them unless the price of the light is heavily subsidized. Moreover, we show that in addition to subsidizing the fixed cost of the light, an introductory discount period on the recharge fee can increase subsequent usage and may be profitable from the firm's perspective.

In our first experiment, we find that initial demand for lights is extremely price-sensitive, with barely any households purchasing at full price and over 90 percent doing so when the upfront price is 0. This has strong implications for take-up and consequently successful business or non-profit distribution models. Second, using variation in upfront price paid and objective data on usage over a three-month period, we show that subsequent long-term usage rates of LEDs does not depend on the initial price paid, even though usage is not free in this setting. Thus, initial price paid does not act as a signal for how much a customer will subsequently use the good, in contrast to Ashraf et al. (2010). This finding is in accordance with experimental evidence on insecticide-treated bed nets (Cohen and Dupas, 2010, and Dupas, 2014a), deworming pills (Kremer and Miguel, 2007), shoes to prevent worm infections (Meredith et al., 2013), a range of general health products (Dupas, 2014b), water chlorination (Ashraf et al., 2010), and a number of medications (Fischer et al., 2016).

Our findings also show that short-run subsidies over an introductory 3-month period can have a significant positive impact on long-run usage after the free trial is discontinued. Giving households an opportunity to use the lights at a subsidized recharge rate for an extended period increased the rate of use upon subsidy expiration. Our data suggests that the main driving mechanism is likely habit formation rather than learning. However, it is worth noting that in this experiment all study participants received an LED light for free, so all of them could try it, hence at least some learning could have come from access to the lights.

Clearly, there is no “one-size-fits-all” model in terms of pricing products for the poor. Results differ depending on the product and context, with our findings being more in line with Dupas (2014a) but differing from those of Fischer et al. (2016).

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Appendix

Table A1 - Results after controlling for distance

	Outcome Variable: Recharges per light			
	Without distance		**With distance**	
	Level	In Hyp Sin	Level	In Hyp Sin
	1	2	3	4
Price 0	0.348***	0.137***	0.337***	0.131***
	(0.122)	(0.048)	(0.121)	(0.048)
Price 50	0.179*	0.085**	0.179*	0.085**
	(0.096)	(0.042)	(0.096)	(0.042)
Price 60	0.257**	0.100**	0.252**	0.097**
	(0.115)	(0.045)	(0.114)	(0.045)
Price 70	-0.020	0.002	-0.020	0.002
	(0.080)	(0.040)	(0.079)	(0.040)
Price 80	0.141	0.046	0.129	0.040
	(0.102)	(0.043)	(0.101)	(0.042)
Price 120	0.053	-0.002	0.049	-0.004
	(0.095)	(0.042)	(0.095)	(0.042)
Distance			-0.000***	-0.000***
			(0.000)	(0.000)
Village Fixed Effects	Yes	Yes	Yes	Yes
Control group mean	0.254	0.128	0.254	0.128
	[0.909]	[0.359]	[0.909]	[0.359]
Observations	2,500	2,500	2,500	2,500
R-squared	0.188	0.197	0.194	0.205

Notes: The table reports the coefficients from regressions of the dependent variable, recharges per light, on randomized treatment indicators and village fixed effects. Treatment was assigned at the household level and stratified by village. The reference category is the status quo, 100 RWF per recharge. The control group mean and standard deviation, in brackets, are also presented for comparison purposes. Columns 1 and 2 report heteroskedasticity-robust White standard errors, and columns 3 and 4 report standard errors clustered at the village level. Statistically significant at 90(*), 95(**), and 99(***) percent confidence.

Table A2 - Balance Tests

	Outcome Variables: Randomized Price Treatments						
	Price 0	*Price 50*	*Price 60*	*Price 70*	*Price 80*	*Price 100*	*Price 120*
Distance	-0.000	0.000	-0.000	0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Village Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,500	2,500	2,500	2,500	2,500	2,500	2,500
R-squared	0.00300	0.00109	0.00165	0.00194	0.00215	0.00145	0.00147

Notes: The table reports the coefficients, and heteroskedasticity-robust White standard errors in parentheses, from regressions of the dependent variables, randomized treatment indicators, on village fixed effects and linear distance to the recharge center computed via GPS. Statistically significant at 90(*), 95(**), and 99(***) percent confidence.

Table A3 - Long-Run Impact of Short-Run Subsidies - Restricted Sample

Outcome Variable: Recharges per light

	Level	In Hyp Sin
	1	3
Price 0	0.562***	0.198**
	(0.210)	(0.080)
Price 50	0.335*	0.165**
	(0.172)	(0.071)
Price 60	0.443**	0.158**
	(0.205)	(0.075)
Price 70	-0.026	0.006
	(0.137)	(0.067)
Price 80	0.259	0.094
	(0.186)	(0.075)
Price 120	0.092	-0.006
	(0.171)	(0.073)
Village Fixed Effects	Yes	Yes
Control group mean	0.42	0.27
	[1.8]	[0.5]
Observations	1,384	1,384
R-squared	0.172	0.197

Notes: The table reports the coefficients from regressions of the dependent variable, recharges per light, on randomized treatment indicators and village fixed effects. Randomization was carried out at the household level with village-level stratification. The reference category is the control group where price was randomly assigned at 100 RWF per recharge. The control group mean and standard deviation, in brackets, are also presented for comparison purposes. Heteroskedasticity-robust White standard errors in parentheses. Statistically significant at 90(*), 95(**), and 99(***) percent confidence, so we report robust standard errors.

Table A4 - Household Socioeconomic Characteristics Huye

Characteristics	
Household Demographics	
Total of household members	5.10
Number of children (<18years) in the household	2.64
Age Head of HH	48.40
Years of education of head of HH	4.40
Household welfare	
Household income per week	14793.68
Household income (log)	8.98
Number of phones in the household	0.78
Household savings in last month	706.79
HH Mother Working Dummy	0.24
HH Working Dummy (non-farming)	0.62
Total working HH Members	1.47
Average hours of sleep per night	9.03
Lighting expenditures and usage	
Total sources of light in household	1.65
Light time per week	16.46
Expenditure on light per week	248.41
Uses a dirty light source	0.52
Studying and Light	
Children's study hours per week	4.10
Study use of dirty light source	0.17
Individual labor market variables	
Working other than farming dummy	0.28
Work hours per week (other than farming)	6.67
Observations	824