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Subsidies for technology adoption: Experimental evidence from rural Cameroon

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ABSTRACT

We use a two-stage experiment to study how a short-term subsidy for a new product affects uptake, usage, and future demand for the same product (a new solar lamp). We use an auction design to gauge willingness-to-pay, and randomly vary the strike price across villages to create random variation in purchase prices and uptake across villages. Our main results are that subsidies do not adversely affect subsequent product use, but stimulate uptake. If subsidies depress future willingness-to-pay, then this effect is outweighed by additional learning about the benefits of the new product. The net effect is that short-term subsidies increase future willingness-to-pay. However, prices play an important allocative role, and lowering prices via subsidies encourages uptake by households with low use intensity. We do not find any evidence supporting social learning and anchoring beyond the initial sample of beneficiaries.

1. Introduction

Economists have documented low adoption of a broad range of apparently cost-effective technologies—products and behaviors that seem to improve people's welfare, including bed-nets (Cohen and Dupas 2010; Tarozzi et al., 2014), stoves (Mobarak et al., 2012), energy-efficient technologies (Allcott and Kessler 2018), new agricultural techniques (World Bank 2007; Duflo et al., 2009), weather insurance (Gine and Yang 2009; Cole et al., 2013), toilets (Guiteras et al., 2015), seasonal migration (Bryan et al., 2014), and health-improving products (Meredith et al., 2013). Many of these studies find that price is a primary barrier for adoption, as summarized in a recent review article (J-PAL 2018).¹ Demand is typically low at positive prices, demand is highly price elastic, and the high price elasticity is not sensitive to the presence of non-monetary incentives and marketing strategies (Dupas 2014a).

Given the importance of adoption of productive new technologies for growth, it is important to develop guidance on the most effective ways to overcome the price barrier. Subsidies are the most direct way to address price concerns. Positive externalities from adoption, strategic complementarities and coordination failures in decision-making (Guiteras et al., 2019), or simply the presence of (capital) market failures, may make subsidized distribution of welfare-improving products an efficient use of societal resources.

However, subsidies can have a complex set of effects on technology take-up decisions, beyond increasing purchases by lowering price. Subsidies allow risk-averse beneficiaries to experiment with new technologies with unknown distributions of benefits and costs, thereby shifting demand in future periods (Dupas 2014b; Bryan et al., 2014). This creates more opportunities for neighbors and social contacts to learn about these products, affecting overall demand through learning externalities (Conley and Udry, 2010; Beaman et al., 2021; Fischer et al.,

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E-mail address: niccolo.meriggi@theigc.org (N.F. Meriggi).¹ Of course last-mile transaction costs may also be considerable, driving a wedge between market prices and full purchase cost for households. In several contexts this may be an important reason for non-adoption (e.g., Grimm et al., 2020), and it is also relevant for the solar lamp case studied in this paper. Non-adoption may depend on both supply- and demand side factors.<https://doi.org/10.1016/j.jdevec.2021.102710>

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2019).

Subsidies could also have non-benign effects. Technologies only improve welfare to the extent that they are actually *used* by adopters. There is a concern that subsidies compromise the allocative function of prices by screening in buyers with a low propensity to use the product for its intended purposes.² Moreover, a so-called psychological “*sunk cost effect*” suggests that people value products less, and use them less frequently, after they paid lower prices for them. Additional considerations enter when we adopt a dynamic perspective. If consumers have reference-dependent preferences and “anchor” their expectations with respect to future prices on current (subsidized) prices, then temporary subsidies will reduce future utility from purchasing these same products (e.g., Koszegi and Rabin 2006; Dupas 2014a). The reduction in future demand might cripple the development of product markets. This is the so-called “*anchoring effect*.”

The overall effect of subsidies on total demand is therefore a combination of the direct effect of price on adoption, the effect on future adoption (through experimentation, anchoring and learning), the spillover effect on future adoption of others (through social learning), and any mediating effect on actual usage. Comprehensive evaluation of the effects of subsidies should include all these mechanisms, and potential sunk cost effects as well. In [online Appendix C](#) we formalize these thoughts and provide a conceptual framework that includes these various factors.

Empirical evidence on the magnitude of these mechanisms has started to accumulate, but unfortunately is not conclusive. The screening (or allocative) effect of prices matters for a water purification technology (Ashraf et al., 2010) but not for subsidized bed nets (Cohen and Dupas 2010). The anchoring effect is unimportant for bed nets (Dupas 2014a) and wood stoves (Bensch and Peters 2019), but plays a role in purchasing decisions of certain curative health products (Fischer et al., 2019).³ The evidence supporting learning effects is also inconclusive (Dupas 2014a; Fischer et al., 2019), and there is no support for sunk cost effects in the context of development interventions (e.g., Ashraf et al., 2010). In light of the conflicting evidence, Fischer et al. (2019) call for more evidence and research on a broader range of product types: “*Building an evidence base for policy making requires understanding the extent to which these results generalize.*”

We study the effects of subsidies on the short-run and long-run adoption of solar lamps in rural Cameroon, where most people do not have access to on-grid electricity and rely on kerosene lamps for lighting. Solar lamps are a low-cost alternative, and provide relatively high-quality lighting. We present an integrated two-stage approach to study how short-term subsidies affect demand and use intensity. We use a Becker-DeGroot-Marshak (BDM) design to auction off solar lamps, and trace out the full demand schedule at the village level. We randomly vary the strike (transaction) price at the village level in Stage 1 of the experiment, and explore how Stage 1 transaction prices affect willingness to pay for identical lamps in Stage 2, two years later (capturing learning and anchoring). We also explain variation in use intensity with bidding behavior and transaction prices to gauge screening and sunk cost effects. Finally, we probe social learning and social anchoring, exploring how the experiences of early adopters, and the prices they paid, affect Stage 2 willingness-to-pay of nearby co-villagers.

We extend previous two-stage demand studies by using an auction design to gauge willingness-to-pay, rather than a binary adoption decision at a given price. We also randomly vary product prices at the *village*

level, rather than the respondent level. We believe this facilitates the creation of credible reference prices, which is conducive to generating some ‘anchoring’. Next, we revisit the important issue of social learning. While Dupas (2014a,b) found evidence of static spillovers, she did not detect positive dynamic effects—exposure to treated neighbors did not encourage long-term adoption of the product. By considering the effect on Stage 2 willingness-to-pay of co-villagers we can analyze this issue in more detail.

While many of these mechanisms have been examined individually for specific products in separate contexts, one of our contributions is to study all these mechanisms jointly for the same product in the same context, to generate a more comprehensive accounting of short and long-term demand. We hope our study contributes to building a broad, multi-product evidence base, which would enable meta-analyses probing which product and consumer characteristics are associated with specific demand shifters. The solar lamp that we study is distinct from the health products analyzed in most of this literature because its “convenience” benefits are direct, obvious and immediate, as opposed to occurring in the future, and subject to several mediating factors (such as with health inputs). Moreover, these benefits are to some extent material, in the form of reduced kerosene expenditures. Information about benefits is easily shared with others in the case of solar lamps, and benefits may even be directly visible from a distance. Compared to much of the technology adoption literature focusing on goods that are either difficult to learn from (e.g., preventative health technologies) or that require consumers to make trade-offs (e.g., cookstoves that pollute less but are harder to use, fertilizers that take a lot of time to apply correctly), learning and social learning about the benefits of using solar lamps should be easy.

Our main results are as follows. Consistent with earlier studies, we find that demand for solar lamps is steeply downward sloping. Subsidies greatly increase uptake. Only a minority of respondents in our sample would purchase a lamp when traded at market prices. We do not find evidence of a sunk cost effect—prices paid for lamps do not affect subsequent use intensity. On average, our respondents also show no evidence of anchoring. Even if there is any anchoring, it gets overwhelmed by the learning opportunities (about product benefits) that subsidized access creates. Current uptake, spurred by low prices, positively affects future willingness-to-pay. Next, we do not document any social learning, or anchoring due to knowledge spillovers. Willingness-to-pay in Stage 2 of the experiment by co-villagers is not correlated with past prices charged locally or with the number of nearby adopters.

The paper is organized as follows. Section 2 introduces the literature on the economics of demand for energy, in particular for electricity, in low-income countries. In section 3 we provide the details of the intervention, and outline the experimental design. We introduce the BDM auction and explain our sampling approach – enabling assessment of dynamic and spillover effects. In section 4 we summarize our experimental and observational data, and outline our identification approach. Section 5 contains the empirical results, distinguishing between static effects, dynamic effects, and spillover effects. Section 6 concludes.

2. Economics and electricity

Some 789 million people worldwide still lack access to electricity, and the electricity access rate in sub-Saharan Africa is only 47 % (IEA, 2020). The majority of these households, especially in rural areas, are unlikely to be connected to the electricity grid in the foreseeable future

² The classic, possibly fictitious, example is the use of subsidized bed nets as fishing nets (New York Times 2015).

³ Fischer et al. (2019) also document that the seller’s identity does not matter for reference point formation: empirically, it does not matter whether curative health products are sold by an NGO or a commercial firm. This is surprising because buyers should realize that firms are unlikely to supply products at subsidized prices for extended periods of time.

due to high costs of grid expansion (World Bank 2009, IEA, 2020).⁴ Bensch et al. (2017) describe a “lighting transition”, with early stages characterized by households moving from kerosene (and candles) towards battery-powered LED and solar lamps, or electric lighting via solar home systems. These early stages in the transition involve investments that are easily scalable, as households can decide about the size and number of lighting devices according to their income or wealth. Later stages in the transition process may include connection to the grid. Eventual completion of the transition towards the grid is fostered by aspirations of rural households with respect to ownership of electric appliances that cannot be supported by solar home systems (Lee et al., 2016b).

The transition is well underway in most parts of the world, including in rural Africa. Almost unnoticed by government and development agencies, battery-powered LED lights have replaced kerosene as the dominant source of lighting (Bensch et al., 2017). LED lamps, powered by batteries or the sun, are more efficient and cleaner than kerosene, and involve lower marginal costs.⁵ Households using kerosene may spend up to 5 % of their expenditures on lighting (Grimm et al., 2017), so cash-strapped households in remote areas may actually use very little artificial lighting, or rely on lumen emitted by the cooking fire. For such households, the day ends shortly after sunset, restricting the pursuit of activities such as working or studying.

A rapidly growing literature explores the impacts of the lighting transition. A common finding is that recurrent energy expenditures go down for households moving away from kerosene.⁶ However, the evidence for “downstream effects” is rather mixed. While various studies suggest that the lighting transition increases study time, which should facilitate studying and improve academic performance (Bernard 2012; Khandker et al., 2013; Litzow et al., 2019), other studies do not find such effects (Furukawa 2014; Rom et al., 2016; Grimm et al., 2017; Kudo et al., 2019), perhaps because children simply shift homework time from the day to the evening so that the net gain in time spent studying is small (Peters and Sievert 2016). Broader impacts on income, via increases in productivity labor supply, or sectoral change, are also contested and vary with the “type” of electrification (for recent reviews, see Bos et al., 2018 and Jeuland et al., 2021). Some studies document positive impacts (e.g. Kirubi et al., 2009; Dinkelman 2011; Rud 2012; Khandker et al., 2013; Aevarsdottir et al., 2017) especially in the longer term (Van de Walle et al., 2017), but others find smaller or zero impacts (e.g., Peters et al., 2011; Bernard 2012; Grimm et al., 2013; Lenz et al., 2017; Lee et al., 2020).

Some of the ambiguity about impacts is likely due to confusion about the counterfactual, or the baseline lighting situation for the control group—this may be battery-powered LED lamps as opposed to kerosene for many households. In other cases, additional constraints (unrelated to lighting) may prevent favorable outcomes from materializing. These include the absence of teachers precluding strong education benefits, or lack of access to markets precluding productivity effects in small enterprises. The context within which electrification occurs matters for impact evaluation, and varies from country to country and within countries (e.g. Hamburger et al., 2019; Peters and Sievert 2016).

⁴ Discussions about the costs of on-grid electrification are often based on the (lumpy) investment costs associated with grid expansion. However, Lee et al. (2016a) and Lenz et al. (2017) draw attention to the cost of connecting to the grid for individual households close to the grid. These household-level investment cost may also be prohibitive, so electrification rates remain very low, even in areas near grid infrastructure. Lee et al. (2016a) refer to this type of being unconnected as “under-grid” (as opposed to “off-grid”).

⁵ This transition process is causing new (environmental and health) challenges associated with the disposal of non-rechargeable batteries.

⁶ Savings on recurrent expenditures enable the household to recoup the investment cost associated with switching energy sources (e.g., Rom et al., 2016; Grimm et al., 2020). We return to this issue below.

Another stream of literature considers demand, or willingness to pay (WTP), for electrification. In light of the above ambiguity, it is not surprising that this literature arrives at some rather nuanced insights. While households value access to electricity, and are willing to sacrifice a significant part of their income to obtain it, payments are typically not sufficient to cover costs of electricity provision. Rom et al. (2016) sell solar lamps in Western Kenya for different prices. When charged the full market price of USD 9, only 29 % of their sample purchases the lamp. Demand is elastic, as discussed above, and this percentage increases to 37 % (67 %) when dropping the price to USD 7 (USD 4). Grimm et al. (2020) auction off three solar technologies in Rwanda, with costs ranging between USD13 and USD 180, and document WTP equal to 30–40 % of market prices (also see Sievert and Steinbuks 2020).⁷ Increasing the payment period increased WTP somewhat, but not much—an increase of 13 % for a 5 months repayment period. Lee et al. (2020) consider the advanced part of the energy transition and explore WTP for on-grid electrification in Kenya by randomizing connection fees. Demand is elastic and aggregate WTP covers only part of the costs of grid expansion.⁸

What does this imply for the scope of policy initiatives to promote universal access to modern energy? Lee et al. (2020) document large welfare losses if such a strategy is pursued through grid expansion (order of magnitude; USD 600–900 per household for Kenya). While this is an overestimate of the true welfare cost because revealed WTP only reflects internalized benefits of individual households and possibly is constrained by limits to the ability to pay (defined by payment period, access to credit), a sizable gap between costs and benefits remains. Grimm et al. (2020) therefore conclude that a subsidization scheme for solar energy is likely to involve smaller welfare costs. For similar reasons, Sievert and Steinbuks (2020) also argue in favor of low-cost off-grid electricity technologies, rather than expansion of the grid.

3. The intervention, background and experimental design

In this study we focus on the adoption of a portable solar lantern marketed in Cameroon by a joint venture of the for-profit multinational TOTAL and a social enterprise called D. Light. This lamp is called the D. light S20®: a water resistant and durable lantern, characterized by a high efficiency solar panel. It was advertised as a high-quality alternative to kerosene lanterns.⁹ A fully charged solar lamp of this type can burn for 4 h on high mode or 8 h on low mode (newer types, such as the D. Light S30 can even burn for 12 h when fully charged). At the time of the study, this lamp was new to the study region and TOTAL just started to offer it at selected gas stations across the country.

Our study was conducted in the Adamawa region of northern Cameroon; a sparsely populated region with approximately one million inhabitants (and five TOTAL gas stations at the time of the study). Slightly more than half the population lives below the poverty line (UNDP 2010), and Adamawa is the second least-developed and third least-educated region of Cameroon (IEA, 2020). While Muslim Fulbe (Fulani) are the major ethnic group in this region (60 %), other ethnic groups are also present, including people of Paleo Sudanese and Bantu origins (IEA, 2020). The main economic activity in the region is cattle herding. The lighting transition has hardly taken off in this region, and

⁷ See also Yoon et al. (2016), who auction off solar lamps in India, and finds willingness to pay levels of, on average, 10 % of market prices. While a trial period did not increase WTP by much, an extended payment period increased WTP by 17 %.

⁸ Deutschmann et al. (2020) study WTP for quality improvements in electricity supply in Senegal. While people are willing to pay to improve electricity and reliability, this WTP is unlikely to cover the marginal cost of quality improvement.

⁹ For more info on D.Light S20®, refer to <https://www.engineeringforchange.org/solutions/product/d-light-s20/>.

very few households had even transitioned to battery-powered LED lighting when we collected our data. More than 95 % of our respondents still used kerosene lamps at baseline as the main source of lighting, providing light of low quality at relatively high cost, and with adverse environmental and health effects (not to mention the risk of causing a fire).

We initially selected a representative sample of 199 villages from the region for data collection. These villages were randomly selected from all villages in the 2005 census. In the spring of 2013 we conducted a listing and mapping exercise in these villages, and used a household questionnaire to collect baseline information. This study was designed in 2012, and we did not pre-specify the details of the analysis below in a pre-analysis plan. This study is not based on a random sample of villagers—instead our sample frame is based on a set of eligibility criteria for another intervention, implemented by an international NGO.¹⁰ In 27 villages, no villagers proved eligible for the other intervention and so these villages were dropped from the sampling frame. In the remaining 172 villages, several households included in the first wave of data collection satisfied the eligibility criteria. Most of the analysis is based on subsamples of these households. We also conducted semi-structured interviews with village authorities to collect information about village characteristics.

Eligible households were re-visited in the Fall of 2013 for Stage 1 of the auction experiment, and were offered the opportunity to purchase a solar lantern D. Light S20® through a Becker-DeGroot-Marshak (BDM) auction. This experimental method provides an incentive compatible measure of the maximum willingness-to-pay (WTP), proxying for consumers' expected utility associated with purchasing the product. Respondents did not receive advance information about the opportunity to purchase a lamp. They were visited individually by our enumerators, who demonstrated the product and carefully explained the auction procedure (see the script in the [Appendix](#)). Next, households stated their maximum WTP for a lamp. At this point, neither the household nor the enumerator knew the relevant strike price, which varied at the village level and was revealed to all participants simultaneously during a meeting at the end of the village visit. This strike price was printed on an A4 sheet, folded in an envelope, sealed and signed by a district attorney. The leader of the enumerator team showed the sealed envelope to all attendants (to prove integrity) and opened the envelope in public. We believe this procedure added salience to the price level, which, if anything, probably biases the results towards the emergence of anchoring and sunk cost effects.

¹⁰ The other intervention, not studied in this paper, concerned the offering of biogas digesters to selected households. Since the biogas digesters would be fed with (animal) manure, only households with livestock were selected. In addition, households should be willing and able to attend an information session on biogas (sensitization campaign), and provide sand and gravel for the construction of the bio-digester. Taken together, a non-random subsample of the villagers participated in the biogas digester intervention and hence feature in the sample frame of the current study. Specifically, the wealthier strata participated, which should be kept in mind when evaluating the external validity of this study. Importantly, the projected timing of the construction of biogas digesters did not overlap with the time frame of two years for the current study, so we do not think there is meaningful interaction between the biogas and solar lamp interventions. (In reality the biogas intervention “failed”, and digesters were never built at all). However, we cannot rule out that *expectations* about future access to biogas may affect current bidding for the lamp as biogas lamps and solar lamps are substitutes in the generation of lumen. If we regress bids for the lamp on a dummy variable reflecting a stated interest in building a digester (and baseline controls, and district fixed effects), we obtain a significant and positive coefficient. This may be because we have an imperfect wealth proxy (so that the “stated interest in biogas” dummy picks up a residual wealth effect), or with a situation where some households have greater demand for energy (biogas or solar) than others. It is not consistent with biogas and solar lamps as substitutes, in which case the regression should have yielded a negative coefficient.

As mentioned, we randomly varied the strike price across the 172 villages, minimizing information spillovers across households assigned to different treatments. Specifically, villages were randomly assigned to either a 25 %, 50 % or 75 % discount on the (full) lamp price, inclusive of procurement costs. Our initial design allocated 59 villages (or 496 participants) to the treatment arm with high prices (low subsidies), 54 villages (438 participants) to the medium price treatment, and 59 villages (429 participants) to the low price (high subsidy) treatment. After the auction, participants whose bids exceeded the strike price were invited in a separate room where they would pay for and collect their lamp. They had until the end of the day to finalize the transaction, which meant that on average they had some 4–6 h to collect the necessary money to complete the purchase.

The D. light S20® sells for 5900 fCFA, or approximately USD 9, at the five TOTAL gas stations in Adamawa. This amount is a serious underestimate of the full costs involved for households who try to purchase it. We estimate such full costs, inclusive of transaction costs, to be around 10,000 fCFA. For example, based on discussions in the villages and local transport costs, the average transaction costs associated with purchasing a lamp exceed 3500 fCFA.¹¹ The three strike prices that we used to auction off the lamps were 3,500, 5500 and 7500 fCFA. Households were not explicitly informed about the subsidy they received, and only received information about the price they should pay for the lamp.

Two years later, in the Fall of 2015, we organized Stage 2 of the auction. Due to budget constraints we were forced to reduce the size of our sample, and randomly selected a sub-sample of 30 villages per treatment arm (hence 90 villages in total). All households taking part in Stage 1, regardless of their earlier willingness-to-pay, were offered the opportunity to bid for a D. light S20® in a follow-up BDM auction. In addition, for early adopters we collected detailed data on lamp usage and levels of satisfaction. The usage data are based on survey-responses, and not on measurements by sensor technology (as in [Rom et al., 2016](#)). During Stage 2 we also asked a subsample of respondents whether they knew the going market price. Only three respondents stated to know this price. We therefore believe that respondent behavior in the experiment is not based on respondents “anchoring” on (real) market prices.¹²

To study social learning we also invited a new sample of co-villagers to participate in the auction—a sample of individuals *not* eligible to participate in the Stage 1 auction.¹³ For every village we selected up to eight random co-villagers, and analyze their bidding behavior in the 2nd auction as well.

4. Data and identification

Our two-stage design allows us to measure both WTP for the lamp in an incentive-compatible manner as well as the causal effect of (exogenous) transaction prices on follow-up values and product usage. This section outlines our data and identification strategy.

During the baseline we collected data on background variables. Regressing these variables on treatment status suggests the randomization was successful in achieving balance across the three groups (regression results not shown). Summary statistics for the respondents,

¹¹ The average transport cost associated with traveling from the villages to the nearest Total gas station (using public transport) is 7200 fCFA. Assuming that households will buy two lamps (possibly one for a neighbor), we arrive at transaction costs of 3600 fCFA per lamp.

¹² Moreover, we include a measure of infrastructure (access via a paved road) as a village-level control variable in the regression analyses below. Road access may be correlated with knowledge about prices for lamps, elsewhere. The paved road access variable does not enter significantly in models explaining bidding behavior, further suggesting that market information does not threaten the internal validity of our experimental design.

¹³ Eligibility was based on owning livestock and being interested in owning a biogas digester, as explained above.

including relevant *p*-values for tests whether the means across treatment arms are identical are provided in Appendix Table A1. Across virtually all dimensions respondents in the treatment arms are comparable, with a few small differences presumably due to chance.

We use data from Stage 1 of the experiment to assess whether subsidies encourage the uptake of solar lamps, see Fig. 1. The share of participants bidding in excess of the strike price drops sharply as prices increase. Specifically, while 70 % of the households indicate that they are willing to pay at least 3500 fCFA for the solar lamp, the adoption rate drops to less than 40 % for the high price of 7500 fCFA. Such price-elastic demand is consistent with earlier studies on the adoption of new technologies by the poor. In particular, the numbers we present are close to those reported by Rom et al. (2016) for demand for solar lamps in Kenya. The finding that WTP of many households is below market prices is also consistent with the rest of the literature, discussed in section 2.

Descriptive results of Stage 1 are also provided in Fig. 2, where we trace out full demand curves based on bidding behavior. Using the stated bids, some 30 % of the population indicated a WTP matching our best estimate of the lamp’s full cost. The demand curve reveals useful information for policy makers interested in promoting adoption of the lamp. While lowering the full price from 10,000 to 6000 fCFA increases the share of adopters by 20 %, a smaller discount offered between the costs 6000 to 3000 fCFA increases this share by approximately 40 %.

However, the follow-up behavior of nearly a quarter of our participants was inconsistent with their bidding behavior. Specifically, 13.5 % of the sample (184 participants) are so-called *decliners*, bidding more than the strike price but not actually buying the lamp when this is offered to them. This number is of the same magnitude as the 15 % share of decliners reported in Grimm et al. (2020), who auctioned off different sized solar kits. We speculate decliners were unable to accumulate the cash by the time they had to pay and collect the lamp (4–6 h after the auction). Next, 10.9 % of the bidders (149 participants) are so-called *bargainers*—bidding less than the strike price, but seeking to purchase the lamp afterwards. Our partner organization was unwilling to enforce auction outcomes, so decliners did not purchase the lamp and bargainers could obtain one for which they paid the relevant strike price.

On average, decliners stated a bid nearly 3000 fCFA higher than the strike price, and bargainers bid 2000 fCFA less than the strike price (see Appendix Table B1 for summary statistics). In this paper we analyze both the full (pooled) sample that combines “consistent” and “inconsistent” bidders (i.e., including decliners and bargainers), and the subsample of “consistent bidders”. The latter results are reported in Appendix Tables (A2-A4), and are qualitatively similar to the ones reported and discussed in the main text.

As a further robustness check, to deal with inconsistent bidders, we

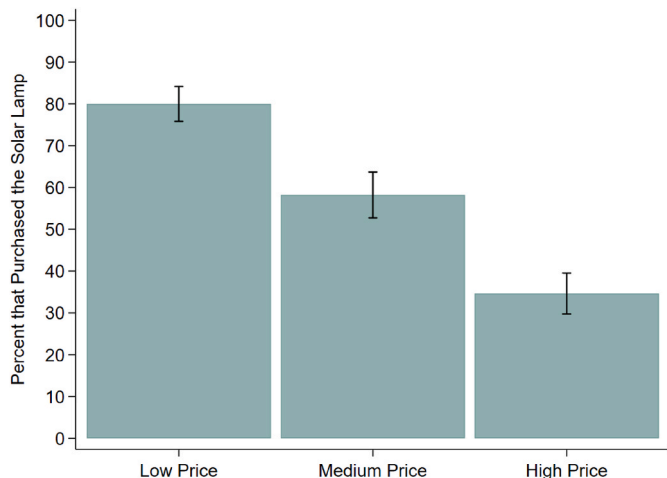


Fig. 1. a: The purchase decision for different subsidy levels.

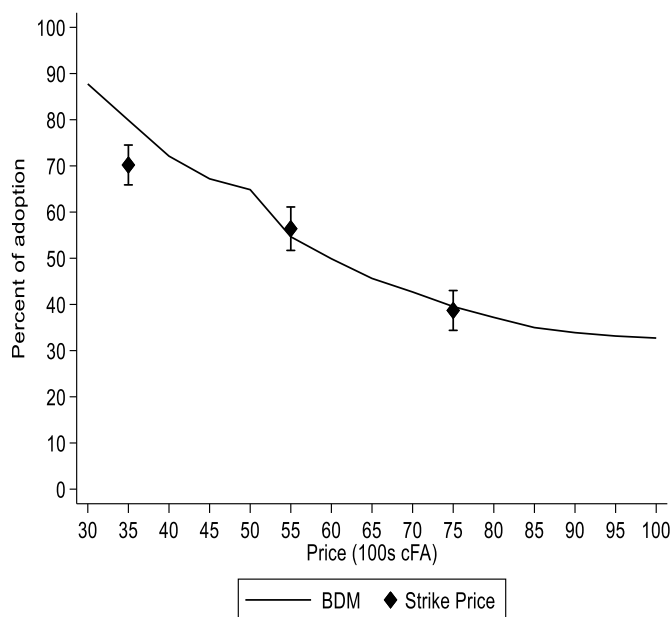


Fig. 2. Demand for Solar Lamps (pooled bids).

use an approach proposed by Grimm et al. (2020). For decliners, this amounts to “scaling down” their bids to various degrees in Appendix Figure B1 (10 %, 50 % and 70 % of bids). For households initially bidding below the strike price, we predict propensities to decline based on the declining decision in the subsample of winners, and assign those with a high likelihood of declining to a WTP as a share of their actual bid. For bargainers, we increase their bids to 110 %, 150 % and 170 % of stated bids in Appendix Figure B2. As is evident, patterns in the data are largely unaffected, and our main results are unaffected (Appendix Tables B2 and B3).

Importantly, we find no evidence that decliners and bargainers in Stage 1 bid differently in the follow-up auction during Stage 2 of the project. If we include dummy variables indicating decliner or bargainer status in regression models explaining follow-up WTP, these do not enter significantly. So we believe that inconsistent bidding did not undermine the integrity of the BDM mechanism in Stage 2 (details available on request).¹⁴ In Stage 2, 14 participants were decliners, of which one participant also declined in Stage 1. There is no significant correlation between decliner status across the two stages. We did not allow *ex post* bargaining about auction outcomes in Stage 2, so the share of bargainers in the sample dropped to zero then.

Next, we turn to the regression framework to analyze our data. In all models we cluster standard errors at the village level (172 or 90 clusters, depending on the model). The dependent variable in our first model is a dummy indicating whether respondent *i* in village *j* purchased the lamp (*Adopt_{ij}*). Our main explanatory variables are two dummies for medium and high strike prices (so that low prices, or high subsidies, are the omitted category), but in most models we also include vectors of district fixed effects and baseline controls *X_{ij}*. The latter vector includes the respondent’s age in years, size of the household, formal education (dummy), a wealth index, a measure of risk preferences, village size (number of people residing in the village), presence of electricity in the

¹⁴ Additional regression analysis probing the determinants of inconsistent bidding reveals that decliners and bargainers are very similar to consistent bidders—formal status proxies, measures of trust, and most socioeconomic demographics do not enter significantly in models explaining inconsistent bidding status (details available on request). This is also true for our education variables, suggesting that “lack of understanding” is unlikely to explain inconsistent bidding behavior.

village (dummy), and a measure of village infrastructure or accessibility (dummy for paved road to the village).

$$Adopters_{ij}^1 = \alpha + \beta_1 MedPrice_j^1 + \beta_2 HighPrice_j^1 + \beta_3 X_{ij} + \varepsilon_{ij}, \tag{1}$$

where superscript 1 indicates data from Stage 1.

The rest of the analysis combines data from both stages of the experiment, and is based on the sub-sample of 90 villages. To probe the sunk cost effect, we ask whether Stage 1 prices affect follow-up lamp usage (measured in minutes) in the 24 h and 72 h preceding our visit (Use_{ij}). Similarly, we probe the screening effect by analyzing the relation between lamp usage and initial willingness-to-pay. The OLS model we estimate reads as follows:

$$Use_{ij}^2 = \alpha + \beta_1 MedPrice_j^1 + \beta_2 HighPrice_j^1 + \beta_3 WTP_{ij}^1 + \beta_4 X_{ij} + \varepsilon_{ij}. \tag{2}$$

WTP^1 is driven by expected use intensity, hence coefficient β_3 is not intended to capture the causal effect of WTP on Use . Instead, the coefficient indicates whether current WTP predicts, in a narrow statistical sense, future use intensity, which is what the screening effect is about. Because the dependent variable is censored, we also estimate Tobit models.

We now turn to how Stage 1 prices and adoption affect follow-up willingness to pay, WTP^2 . Our design does not allow us to cleanly distinguish between anchoring and learning effects. Since adoption status depends on the strike price, we face a multicollinearity problem when including both variables in one model. Instead, we estimate separate models. Note that the effects of learning and anchoring work in opposite directions if the lamps perform better than expected. While low prices in Stage 1 encourage learning via enhanced adoption, increasing WTP^2 , they may decrease WTP^2 due to anchoring. The net effect is obtained by regressing WTP^2 on adoption status (instrumented by Stage 1 strike prices—equations (3a) and (3b)),¹⁵ and on Stage 1 strike prices directly (3c):

$$WTP_{ij}^2 = \alpha + \beta_1 Adopters_{ij}^{1*} + \beta_2 X_{ij} + \varepsilon_{ij}, \tag{3a}$$

$$Adopters_{ij}^1 = \alpha + \beta_1 MedPrice_j^1 + \beta_2 HighPrice_j^1 + \beta_3 X_{ij} + \mu_{ij}, \text{ and} \tag{3b}$$

$$WTP_{ij}^2 = \alpha + \beta_1 MedPrice_j + \beta_2 HighPrice_j + \beta_3 X_{ij} + \mu_{ij}. \tag{3c}$$

Finally, we consider social learning and anchoring, and try to measure learning spillovers based on the bidding behavior of “co-villagers” at Stage 2 (see also Alem and Dugoua, 2019 on this issue).¹⁶ Recall, this is the sample of respondents who did not participate in the auction during Stage 1. We estimate two models. First, we estimate model (3c) for this sample of co-villagers to probe whether bidding behavior is affected by lagged lamp prices as paid by co-villagers. This model picks up potential anchoring effects (if respondents learned about the level of these lagged prices) as well as learning effects, as the density of local adopters obviously varies with the lagged strike price. Second, we regress WTP^2 of co-villagers on the local density of early adopters in the village, instrumented by lagged strike prices:

$$WTP_{kj}^2 = \alpha + \beta_1 Adopters_{j^1}^{1*} + \beta_2 X_{kj} + \varepsilon_{kj}, \tag{4a}$$

¹⁵ Observe that it is not evident that we can use the random strike prices as instruments for adoption status (in a model explaining Stage 2 WTP). A potential anchoring effect implies the exclusion restriction might be violated. This is an empirical matter (see below, for regression results).

¹⁶ Alem and Dugoua (2019) study incentivized and unincentivized communication about the use of solar lamps, and the implications for willingness to pay for close friends of lamp owners. They find that information about benefits spills over from owners to friends, especially when owners are incentivized to discuss the lamp’s benefits. Willingness to pay for a similar lamp increases by 90 % and 145 %, respectively, compared to a control group of other friends who did not benefit from social learning.

$$Adopters_j^1 = \alpha + \beta_1 MedPrice_j^1 + \beta_2 HighPrice_j^1 + \beta_3 X_{kj} + \mu_j, \tag{4b}$$

where subscript k identifies the subsample of co-villagers and superscript * indicates predicted values.

5. Empirical results

We first discuss the findings based on Stage 1 auction data, and then combine the data from the two stages to explore the causal effect of (lagged) prices on usage, learning, and anchoring. We already established that demand for solar lamps is (steeply) downward sloping, or that the probability of purchase decreases in the price. This is also evident from Table 1 for the pooled data of consistent and inconsistent bidders, and Appendix Table A2 for the subsample of consistent bidders only.

Table 1 presents results of three OLS models, with and without controls. Compared to subjects from the low-price arm, the probability of adoption falls by 14–22 percentage points (depending on specification) when the price increases from 3500 fCFA to 5500 fCFA and by 31–39 percentage points when the price increases to 7500 fCFA. The latter treatment effect is large, cutting adoption by 44 %. Without subsidies, many poor households use expensive kerosene, or sit in the dark during the evening and night. Qualitatively similar results emerge if we adjust bids based on the robustness check proposed by Grimm et al. (2020)—see Appendix Tables B2 and B3.

Of course, low bids in the auction may simply reflect low expected utility from the lamp—people bidding low amounts because they anticipate they will not use the lamp frequently. To probe this screening effect, we estimate model (2) and regress variation in the intensity of lamp use on WTP . Results are reported in Table 2, which is based on the subsample of consistent bids (including decliners as adopters in this analysis would be misleading as they did not purchase the lamp, so they would have to be included with zero lamp usage). Columns (1–3) explain variation in lamp use during the past 24 h, and columns (4–6) explain variation in lamp use during the past 72 h. Columns (1–2) and columns

Table 1
Demand for solar lamps (pooled bids).

	Purchased the Lamp in T1		
	(1)	(2)	(3)
	OLS	OLS	OLS
Medium price	-0.138*** (0.052)	-0.164*** (0.047)	-0.227*** (0.046)
High price	-0.315*** (0.050)	-0.337*** (0.046)	-0.386*** (0.050)
Constant	0.702*** (0.035)	0.579*** (0.079)	0.573*** (0.071)
Mean of low price	0.702	0.702	0.702
Number of observations	1363	1350	1350
R squared	0.068	0.129	0.169
District fixed effects	No	No	Yes
Controls	No	Yes	Yes

Note: This table shows coefficients of an OLS regression where the depending variable is a dummy equal to one if the individual purchased the lamp in phase 1. The treatment variable is the outcome of the randomized price group drawn from the BDM experiment after the individuals stated their WTP for the solar lamp. The medium price treatment refers to the group that received the 50 % subsidy, and the high price treatment refers to the 25 % subsidy. The excluded group is the low price group that got a 75 % subsidy. Baseline controls: the respondent’s age in years, size of the household, formal education (dummy), a wealth index, a measure of risk preferences, village size (number of people residing in the village), presence of electricity in the village (dummy, and a measure of village infrastructure or accessibility (dummy for paved road to the village) are introduced in Column (2), and District Fixed Effects are introduced in Column (3). Standard errors are clustered at the village level. ***p < 0.01, **p < 0.05, *p < 0.1.

Table 2
Lamp usage conditional on purchase (consistent bids only).

	Past 24 Hours				Pat 72 Hours			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	OLS	OLS	Tobit	OLS	OLS	OLS	Tobit
Medium price	8.304 (21.367)	7.070 (20.938)	-2.657 (21.615)	-5.935 (34.542)	14.539 (58.023)	21.832 (59.368)	4.588 (61.962)	19.917 (93.874)
High price	-24.173 (21.045)	-22.070 (19.284)	-31.104 (22.753)	-75.316* (40.371)	-111.977* (60.356)	-104.934* (53.656)	-119.711* (66.315)	-249.479** (110.922)
WTP in T1 (hundreds)	0.987** (0.407)	0.967** (0.407)	0.903** (0.443)	1.531** (0.771)	3.144** (1.239)	3.116** (1.239)	2.948** (1.336)	4.824** (2.192)
Constant	82.900** (31.451)	137.398*** (45.667)	53.815 (64.957)	-149.274 (114.718)	248.111** (98.292)	422.630*** (129.048)	123.398 (173.666)	-469.456 (311.934)
Mean of low price	154.013	154.013	154.013		475.633	475.633	475.633	
Number of observations	326	326	326	326	318	318	318	318
R squared	0.022	0.063	0.109		0.028	0.072	0.119	
District fixed effects	No	No	Yes	Yes	No	No	Yes	Yes
Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes

OLS and Tobit estimates where the dependent variable is the number of minutes people use the solar lamp in time period 2, conditional on purchasing the lamp in period 1. Columns (1)–(4) show usage in the past day, and columns (5)–(8) show usage in past 3 days. The medium price treatment refers to the group that received the 50 % subsidy, and the high price treatment refers to the 25 % subsidy. The excluded group got a 75 % subsidy. Baseline controls included are: the respondent’s age in years, size of the household, formal education (dummy), a wealth index, a measure of risk preferences, village size (number of people residing in the village), presence of electricity in the village (dummy), and a measure of village infrastructure or accessibility (dummy for paved road to the village). Willingness to pay is measured in 100’s of fCFA. Standard errors are clustered at the village level. ***p < 0.01, **p < 0.05, *p < 0.1.

(4–5) are based on OLS models, and columns (3) and (6) are based on the Tobit estimator. Conditional on purchasing the lamp, use intensity is consistently increasing in the bid amount—current WTP predicts future use intensity. This is consistent with a screening effect of prices. The correlation is economically significant: a one standard deviation increase in WTP is associated with an 0.213 standard deviation increase in minutes of lamp usage (for the 24 h recall data).

Table 2 also contains our findings for the sunk cost effect, for which we find no empirical support. We do not find that subjects paying more for the lamp use it more intensively. Indeed, we document weak evidence to the contrary: respondents paying the highest price on average use the lamp fewer minutes (at least when considering the 72 h recall data). Perhaps this reflects that people who paid a high price treat the lamp more cautiously. Regardless, there is no evidence of a sunk cost effect, which is consistent with findings reported by Ashraf et al. (2010).¹⁷

The results on the screening and sunk cost effect are to some extent driven by zero usage. Some 40 % of the respondents did not use the lamp at all during the past 72 h, two years after the sale. Zero usage is obviously consistent with the screening effect, but could also be due to a worn-down battery. If batteries wear down because of extensive use earlier, then an alternative interpretation would exist for the empirical results above (e.g. low income households, with lower WTP, may have used the lamp more intensively shortly after the first auction, causing more rapid depreciation). However, this is not what we observe. Survey evidence reveals that 94 % of the lamps sold during Stage 1 were still operational two years later.¹⁸ We do not believe that zero-usage is explained by the lamp breaking down between Stage 1 and Stage 2.

Another caveat is relevant here. Marginal cost of using a solar lamp are very low, which is an inevitable shortcoming of using solar lamps as

¹⁷ The interpretation of the sunk cost effect is complicated because of collinearity between treatment groups and WTP (since individuals from the high-price treatment group can only be observed if they bid at least 7500 fCFA in Stage 1, whereas the other treatment groups involve individuals with lower winning bids). We deal with this by including lagged WTP levels. Another approach would be to restrict the analysis to individuals who bid at least 7500 fCFA across all three treatment groups. This yields similar results as the ones reported in Table 2 (details available on request).

¹⁸ According to official specifications, the lifespan of a D.Light is more than 5 years.

a vehicle to study screening and sunk cost effects. According to our data, only 10 % of the respondents used the lamp more than 10 min and less than 3 h. Low-usage therefore explains relatively little of the variation in our intensity of use data, and much of the variation is either from zero-usage (see above) or from respondents using the lamp for an extended time period. A fully charged lamp is functional for up to 8 h, so there is meaningful variation in lumen consumption after 3 h of lamp use (people start lighting their lamp around 5–6 pm).

Next turn to the impact of Stage 1 prices on Stage 2 bidding behavior. Dynamic effects may materialize due to learning and shifting reference points. As discussed, we cannot separate out the effects on learning and anchoring, so the relevant coefficients in models (3a-c) as reported in Table 3 capture both effects. In columns (1–2) we regress WTP^2 on lagged adoption decisions, instrumented by lagged strike prices, and in columns (3–4) we regress WTP^2 on lagged strike prices directly. The first column reveals that households who have experienced the lamp in their household are willing to pay more than double the amount offered by non-adopters. This is consistent with a strong learning effect about the benefits of the lamp. However, it is not evident that the exclusion restriction is satisfied—this may fail, for example, because of anchoring and sunk cost effects. The evidence for sunk cost (above) is weak, but if people anchor on past prices, then current WTP may be affected by another channel than learning due to adoption.

To this we now turn. Consider the outcomes reported in columns (3–4), where the main regressors are exogenous (because randomly assigned). Contrary to the anchoring hypothesis, higher strike prices are correlated with lower WTP in Stage 2. The size of the negative coefficient is economically significant as well. Observe that WTP falls more for respondents from the high price arm than for respondents from the medium price arm (coefficients roughly double in size). Either the average respondent in our sample does not anchor on prices or, if an anchoring effect exists, it is dominated by the learning effect. Lower prices increase adoption (Table 1) which facilitates own learning with the lamp. This helps households to update their preferences for a lamp.

Observe that diminishing marginal returns to lamp ownership presumably imply that the results in Table 3 are an underestimate of the true effect of learning. The expected lifetime of the lamp exceeds the time between the two auctions and, as mentioned, the great majority of lamps was still functioning during Stage 2. Early adopters therefore already own a lamp, and the Stage 2 auction reveals their WTP for an additional lamp.

Table 3
Self-learning and anchoring (pooled bids).

	WTP at T2					
	(1)	(2)	(3)	(4)	(5)	(6)
	IV	IV	IV	OLS	OLS	OLS
Purchased the lamp	33.283** (13.101)	32.827*** (12.490)	28.891** (11.687)			
Medium price				-6.360 (4.615)	-7.484* (4.437)	-7.551* (4.157)
High price				-11.309*** (4.197)	-11.818*** (4.157)	-11.284*** (4.130)
Constant	24.439*** (8.143)	24.065** (10.562)	21.096** (10.669)	50.355*** (3.030)	49.419*** (7.522)	36.328*** (10.552)
Mean of low price	50.355	50.355	50.355	50.355	50.355	50.355
Number of observations	618	618	618	618	618	618
R squared	-0.050	-0.008	0.080	0.023	0.074	0.127
First stage F-stat	14.663	17.281	17.586			
District fixed effects	No	No	Yes	No	No	Yes
Controls	No	Yes	Yes	No	Yes	Yes

Note: Regression estimates where dependent variable is WTP in Time Period 2 (in hundreds of fCFA). Columns (1)–(3) are IV regressions where a dummy variable equal to one if individual purchased the solar lamp is instrumented with price treatment groups. Columns (4)–(6) are OLS regressions where the WTP in Time Period 2 is regressed on price variables. Columns (3) and (6) introduce district fixed effects and control variables: the respondent’s age in years, size of the household, formal education (dummy), a wealth index, a measure of risk preferences, village size (number of people residing in the village), presence of electricity in the village (dummy), and a measure of village infrastructure or accessibility (dummy for paved road to the village). Standard errors are clustered at the village level. ***p < 0.01, **p < 0.05, *p < 0.1.

Finally, we turn to the issue of social learning. Villagers who did not participate in the first auction (so-called “co-villagers”) may respond to lagged prices, or may have learned about the benefits of the lamp from their early-adopting peers. As before, these effects may pull in opposite directions—if co-villagers anchor on past prices, their WTP should be an increasing function of lagged prices. In contrast, if they learn about the benefits of lamp ownership, then we expect their WTP to be a decreasing function of lagged prices, because lower prices increase the local density of adopters, which should facilitate learning. Indeed, the average percentage of all villagers owning a lamp is twice as high in villages with a low strike price (6.7 %) than in villages with a high strike price (3.4 %). This implies meaningful variation, even if it is an empirical question whether this variation is sufficient to result in meaningful variation in social learning. To explore this issue, we regress bidding behavior of random individuals from the same village on lagged local strike prices. Results are reported in Table 4.

We find that bidding behavior is not significantly correlated with lagged prices. Either social learning does not occur for the village-level adoption shares in our experiment, or it is offset by anchoring. In light of the earlier finding that anchoring is relatively unimportant for actual participants in the Stage 1 auction (Table 3), we speculate that any anchoring effects will be even smaller for co-villagers who did not participate in the first auction. If this is correct, and if our assumption holds that learning is facilitated by a greater density of local adopters, then these findings suggest that there is very little social learning about the benefits of solar lamps in our sample. This supports a recent insight that the diffusion of information even within small communities may be slow and very imperfect, unless sources of information are appropriately incentivized to invest in information dissemination (Sayinzoga et al., 2016; BenYishay and Mobarak 2018).¹⁹

In column (4) of Table 4 we report the results of an instrumental variables model. In the first stage we explain the share of local adopters with Stage 1 strike prices, and in the second stage explain variation in

¹⁹ An alternative interpretation should be mentioned here. In theory it is possible that social learning about the benefits of solar lamps is nearly perfect and independent of the local density of early adopters. If information about these benefits diffuses rapidly and uniformly across all villagers, then bidding behavior of co-villagers should be independent of the number of households who bought a lamp as long as at least one villager purchased a lamp.

Table 4
Social learning and willingness to pay (pooled bids).

	WTP of Random Co-Villager			
	(1)	(2)	(3)	(4)
	OLS	OLS	OLS	IV
Medium price	-2.68 (3.53)	-3.28 (3.56)	-1.28 (3.30)	
High price	2.57 (4.44)	2.34 (4.43)	2.75 (3.32)	
Number of people in the village		0.01 (0.01)	0.01 (0.01)	0.01 (0.01)
Share of households purchased the lamp				-8.19 (9.98)
Constant	54.87*** (2.688)	58.10*** (4.068)	43.06*** (6.50)	46.59*** (6.89)
Mean of low price	54.87	54.87	54.87	54.87
Number of observations	445	445	445	445
R squared	0.010	0.017	0.205	0.184
District fixed effects	No	No	Yes	Yes
Controls	No	Yes	Yes	Yes

Notes: This table shows OLS and IV regression estimates where the dependent variable is the Willingness to Pay (WTP). Columns (1)–(3) show the OLS regressions where WTP of the random villagers sampled in time period 2 is regressed on price variables. Column 4 shows the IV estimates where the share of villagers who adopted is instrumented with price treatment groups. Willingness to pay is measured in 100’s of fCFA. Columns (2), introduces village level controls: village size (number of people residing in the village), presence of electricity in the village (dummy, and a measure of village infrastructure or accessibility (dummy for paved road to the village), and Column (3) introduces district level fixed effects. Standard errors are clustered at the village level. ***p < 0.01, **p < 0.05, *p < 0.1.

co-villagers WTP by the predicted number of early adopters. The first stage is strong (as evident from the partial F) and in the absence of anchoring effects for co-villagers the exclusion restriction is also satisfied (an assumption). We find no significant correlation between the local density of early adopters in the village and bidding behavior.

These results appear inconsistent with those of Alem and Dugoua (2019), who document large spill-over effects between friends in India. Our analysis has sufficient statistical power to pick up the effect size they document. Two candidate explanations for the difference in results are

that (i) village-level adoption rates are relatively low in our study, and (ii) social learning is plausibly more likely to occur among friends than between random villagers. (Of course there are other differences between these studies that may be relevant.)

6. Conclusions and discussion

We hope the contribution of this paper is twofold. It contributes to our understanding of the welfare costs associated with subsidizing access to goods and services for the poor, especially in relation to several behavioral factors that might raise the costs of subsidy provision. It also speaks directly to ongoing policy debates about subsidizing access to electricity for poor households in rural areas.

Fischer et al. (2019: 124) wrote: “*Ultimately, the answer to the question “how will one-time subsidies affect future demand” is simple: it depends Building a robust model that both informs policy and extends our knowledge of markets requires further research that replicates prior tests in other contexts and explores additional potentially important factors.*” This is exactly what we did in this paper. We explore whether demand for a new product is driven by insights stemming from behavioral economics, and more specifically ask whether respondents exhibit behavior consistent with sunk cost effect and anchoring effect (reference-dependent utility). From an economic perspective this is crucial information if one wants to draw inference about welfare from market data. From a policy perspective this is crucially important to inform debates about (short-term) subsidies of important inputs or consumption goods for the poor. If subsidies invite wasteful behavior or cripple the development of future markets, then we cannot rely on conventional cost-benefit analyses to assess the efficiency of subsidy programs. Cost-benefit models would have to be enriched with behavioral elements.

Our empirical results suggest such amendments may not be necessary – at least not for solar lamps and for our sample population. We find no evidence of meaningful anchoring or sunk cost effects. We do document, however, that respondents “learn” about the benefits of the lamp, so that short-term subsidies increase future demand. Subsidies, however, also crowd-in low-intensity users, which compromises the efficiency of subsidy programs. Ideally, a subsidy program should, at the margin, balance the efficiency losses due to crowding-in with efficiency gains from learning.

While subsidies facilitate own learning, we find no evidence of significant social learning among random villagers. Information about the benefits of solar lamps does not seem to spread far beyond the source households who purchased the lamp. This interpretation is consistent with recent insights from other research: information does not diffuse automatically across target populations in low-income countries (e.g. Dupas 2014b; Beaman et al., 2021; Sayinzoga et al., 2016), perhaps unless individuals are incentivized to share the information they possess (BenYishay and Mobarak 2018, Alem and Dugoua, 2019). While learning externalities could potentially be an important reason for the provision of short-term subsidies in other contexts, our data do not support arguing along this line.

So what does this imply for the debate about subsidizing access to electricity in poor, rural areas? First and foremost, looking at WTP levels, our results appear to echo those of recent work by Rom et al. (2016), Grimm et al. (2020), Sievert and Steinbuks (2020), and Lee et al. (2020) in different contexts. In the absence of subsidies, the majority of the target population does not buy solar lamps, hence ambitions with respect to providing universal access to electricity will not be reached by market-based approaches only. While we find that learning about the benefits of the lamp increases WTP, the majority of the experienced respondents still bids below the full cost of the lamp during the second auction (to obtain a second lamp). If a gap remains between provision cost and WTP even for small-scale and inexpensive solar technologies, then it seems highly unlikely that expanding electricity grids into rural areas can be a welfare-enhancing proposition in the near future (Lee et al., 2020).

If the internalized benefits of solar technologies do not cover the full cost, then subsidies for electricity are likely welfare-reducing (unless positive external effects make up for the gap between price and WTP). However, we collected data on recurrent kerosene expenditures and estimate that, for the average (median) household in our study, the amortization period is only 6 months (10 months).²⁰ This suggests that there may be other explanations for low bids. If liquidity constraints prevent the majority of respondents from access to electricity, then electricity interventions may be welfare-enhancing and should be accompanied with microcredit programs or subsidies. The evidence for liquidity constraints as a major factor depressing demand is, however, mixed. Some studies find that relaxing liquidity constraints has a large accentuating effect on WTP (e.g. Yoon et al., 2016), but others find much less evidence of such effects (e.g. Grimm et al., 2020). We do not have access to an (exogenous) measure of access to credit, so are reluctant to explain variation in WTP by credit access. However, we do know that few households in our sample can access credit, so it is very well possible that liquidity constraints affect bidding behavior.²¹

Our analysis is by no means the final word on the welfare effects of short-term subsidies. First, in our context respondents are unaware of the product’s market price. In many other contexts knowledge about market prices is more widespread, and it is an open question whether this matters for the impact of subsidies. This is a concern about external validity. If a government were to temporarily subsidize the price of solar lamps for all households, including households with market access as an outside option, then it is not clear that our results would hold. Having an outside option that makes the subsidized price “attractive” may generate different treatment effects than a uniform subsidy with no outside option. This would be the case if market prices are a more salient anchoring point than prices in an experiment.

Second, the divergence of results in the literature suggests the usefulness of looking beyond “average respondents,” and developing a typology of consumers. Laboratory experiments suggest that sample populations consist of multiple types, more or less prone to behavioral biases. Similarly, it may be expected that reference-dependent utility and sunk cost matter more for certain types of consumers than for others, in which case lumping everybody together obscures important information. Similarly, it may be useful to develop a typology of goods, for example based on whether the good in question is durable or not, essential or not, and yields positive (or negative) externalities – also see Kremer and Willis (2016). For behavioral biases to enter, it might be relevant whether the good is a pure consumption good, or an input in a production process. For health inputs, the distinction between curative and preventive inputs might be relevant. These issues, and others, could be usefully explored in follow-up research.

Author statement

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²⁰ This estimate is in the same ballpark as Rom et al. (2016), who estimate an amortization period of 6–22 months, and lower than the amortization period found by Grimm et al. (2020), which was 30 months.

²¹ Specifically, less than 2 percent of the households reported that they accessed any credit during the past 12 months. Moreover, 67 % of the respondents reported that they receive part of their payment, when employed, as an “in kind” payment. This further suggests that many households likely face liquidity constraints.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.jdeveco.2021.102710>.

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