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Paying Attention to Profitable Investments

Experimental Evidence from Renewable Energy Markets

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Abstract

This paper provides an explanation for why many information campaigns fail to affect decision-making. The authors experimentally show that a large information intervention about a profitable and climate-friendly household investment had limited effects if it only provided generic data. In contrast, it caused households to make new investments when it followed a campaign strategy designed to minimize information processing costs. This finding is consistent with a model of selective attention, where individuals prioritize information believed to be valuable after accounting for the costs of attending to the data that arise due to limited mental energy and time. The paper studies a range of possible mechanisms and finds corroborative evidence of selective attention as an inhibitor to learning.

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Paying Attention to Profitable Investments:

Experimental Evidence from Renewable Energy Markets

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I. *Introduction*

Every year, governments and development partners spend billions on campaigns aimed at tackling information failures that impede human capital formation and economic growth (e.g., Amico et al., 2012, Birkhaeuser et al., 1991; Kondylis et al., 2017). The evidence about their effects tends to be mixed and the understanding of the heterogeneity in results is limited (e.g., Dupas, 2011; La Ferrara, 2016).

This paper proposes a new explanation for why many public[1] information campaigns have failed to affect decision-making. In particular, individuals do not attend to potentially useful information when processing costs are high, such as selecting among different options that are difficult for them to evaluate (e.g., health insurance plans or lightbulbs; see review by Handel and Schwartzstein, 2018). The human brain has limited cognitive capacity that is occupied with multiple important decisions on a daily basis. Consequently, people have to prioritize what to spend their mental energy and time on. In turn, information (correctly or wrongly) *believed* to be worth attending to after accounting for processing costs has the highest chance of being selected. This behavior may be especially relevant among poor populations that frequently have to take care of urgent issues such as managing volatility in the incomes and expenditures at the expense of processing information *perceived* as little important (Mani et al., 2013).

Our randomized control trial provides the first experimental evidence of the extent to which selective attention shapes the impact of public information campaigns. The studied intervention followed a typical approach to information provision and a new campaign strategy designed to have minimal information processing costs. The experimental data allow us to estimate the relative effects of the two and analyze possible complementarities.

We examine two components of a large program commissioned by a major international institution that informed households in rural Senegal about solar lamps—a profitable and climate-friendly technology innovation in the study area (e.g., Grimm et al., 2017; Rom et al., 2017)[2]—with a focus on the benefits for home production and quality attributes. Importantly, while one program component provided generic information content, the other singled out for each of the main applications the most suitable type among solar lamps available at local markets and the associated

[1] This is to refer to any information campaign that is financed without expected private returns. Commissioning parties include, for example, governments, non-profit organizations, and development institutions. This is different from advertisement.

[2] Solar lamps have been shown to significantly reduce energy expenditures in rural areas unconnected to the electricity network and amortize within five to ten months (Grimm et al., 2017). They also generate savings in CO_2 emissions (e.g., Rom et al., 2017).

benefit with the objective of simplifying choices for households.[3] The significant advantage associated with simplified choices is a reduction of the cognitive effort and time required to select the technological configuration that yields maximum returns. In particular, a costly process of trial and error may no longer be needed to learn about solar lamps and select the best model for a specific use.

The direct contribution of our paper is advancing the literature concerned with assessing the impacts of public information campaigns in developing countries. While most of this literature has focused on policies that address imperfect access to information about profitable investments (La Ferrara, 2016; Dupas, 2011), we analyze a (component of a) larger intervention that is motivated based on a small but emerging body of research suggesting that (irrational) inattention to readily available information can be an important constraint. We also add to a broader literature on behavioral questions around information about profitable investments. Even though our study is not the first to empirically study selective inattention (e.g., Hanna et al., 2014), to the best of our knowledge, we are the first to empirically analyze whether and how information interventions at scale can improve results by incorporating these insights (Handel and Schwartzstein, 2018).[4] Moreover, we provide the first evidence of comparing the effects of addressing information access versus psychological distortions in information-gathering, attention, and processing. In doing so, our results are also relevant for a wide literature on the determinants of learning and information diffusion (e.g., Banerjee et al., forthcoming; Liverpool-Tasie et al., 2019; Paul and Dillon, 2019).

The focus on a profitable technology is of particular interest. On the one hand, heterogeneity in technology levels can explain significant variation in the performance between economies and among economic agents within the same country (e.g., Caselli and Coleman 2001, Syverson 2011). On the other hand, practitioners have faced many challenges when introducing technology innovations. This has led to a large literature on this subject (e.g., Nelson and Phelps, 1966; Foster and Rosenzweig, 1995; Jovanovic and Nyarko 1996) which by and large highlights information deficit as a key barrier (Foster and Rosenzweig, 2010).

Studying solar lamps as profitable investment is motivated on multiple grounds. First, they are associated with significant savings in energy expenditures, environmental benefits, and improved development outcomes (e.g., Grimm et al., 2017; Rom et al., 2017; Hassan and Lucchino, 2016). In fact, these lamps play an important role in the strategy of the global development community to provide basic energy services to over 400 million people worldwide without access to electricity.

[3] Examples of different lamp types are illustrated in Figure A1 in the Appendix.
[4] Hanna et al. (2014) develop and test a model of technological learning without showing how and the extent to which their insights can be used to transform the effectives of actual policies.

Even though the technology offers a cheaper and cleaner energy source than kerosene or battery-powered torches for households over time, take-up remains far below aspired levels. Underinvestment especially applies to quality products that have the lowest cost per lumen among locally available lighting solutions in developing countries (World Bank, 2018).

The second important motivation for choosing a campaign on solar lamps in rural Senegal when studying selective inattention is that the costs of processing generic information are arguably large. For instance, there exists a large variety of options at local markets which complicates household decision making. Moreover, lighting is seldom the primary factor affecting home production and, thus, may represent a dimension of lower priority in people's lives. For instance, a common product made by rural households in Senegal are baskets woven from natural grasses and strips from salvaged plastic (i.e., primary inputs). Lighting affects accuracy when production is expanded to continue after nightfall and therefore potentially affects the number of possible work hours or quality. However, the primary (and most salient) inputs in this case are the basket materials. Thus, informing about solar lamps in a generic way may be of limited value due to the considerable processing costs on the side of the target population and other more urgent issues that it faces.

Our study makes a methodological contribution to the literature by showing the use of a low-cost approach that produces unbiased effect estimates of an information campaign involving broadcasting – here one program component was broadcasted on national radio. To date, rigorous evidence is limited in the case of broadcasting campaigns where credibly exogenous variation in exposure is hard to find (La Ferrara, 2016; Banerjee, 2018).[5] We propose a randomized encouragement design that does not require randomization of (radio) airwaves. A randomly selected treatment group is encouraged through a call-in contest to tune into a radio channel broadcasting the campaign and another set of individuals—the control group—is encouraged in the same way to tune into a comparable radio channel that does not broadcast the campaign. Winners of the call-in contest receive significant monetary prizes.

While this approach is not entirely new (Berg and Zia, 2011), we arguably improve upon this influential work by delinking monetary rewards from remembering broadcasting content. In doing so, we aim to mitigate the concern that people are artificially induced to pay more attention to the information than they would normally do. Another novel aspect of our study is the demonstration

[5] While our study aims to generate close to real-world estimates of the effects of broadcasting, the literature has seen a growing number of proof of concept studies over the past years that provide rigorous evidence of mass media campaigns under artificial exposure (e.g., Banerjee et al., 2018; Mvukiyehe 2017).

that short low-cost automated voice telephone surveys as described in Ben et al. (2015) can make randomized encouragement designs financially viable against the backdrop of high sample size requirements and oftentimes expensive traditional household surveys.[6]

We randomly assigned 150 villages to the typical campaign component providing generic information broadcasted on national radio (GENERIC), the same generic component plus the distribution of print media that illustrate optimal lamp choices aimed to reduce information processing costs (GENERIC+CHOICE), and a control group. The medium of communication for both campaign components was determined based on the complexity of the information to be provided. While generic information tends to be easily communicable via audio messages, illustration was required for the presentation of the optimal lamp types.

For the purpose of directly comparing the impacts of both campaign components, we additionally aim to estimate the sole effect of CHOICE. In the absence of a separate treatment group, we exploit the fact that the symmetric radio encouragement creates two distinguishable populations within experimental groups: call-in contest participants and call-in contest non-participants.[7] The difference in the outcome variable between the experimental group that receives both campaign components and the control group among call-in contest non-participants approximates the effect of CHOICE on its own. For these non-participants, we have no reason to believe that they are subject to variation in exposure to GENERIC. In other words, these households were not exposed to the radio clips.

We find that information on optimal lamp types (i.e., CHOICE) was required to affect the extensive margin (increase the number of new adopters of solar lamps). However, providing information about the general benefits and quality (i.e., GENERIC) was sufficient to have an impact on the intensive margin (increase the number of owned solar lamps among existing customers). Our data are not suited to assess the effects of additionally exposing existing customers to CHOICE, which arguably represents a second order question given the success of the low cost generic information treatment on the extensive margin.[8] For the same reason, the corresponding regression for the

[6] While cost-efficient, this methodological approach comes with some limitations. Notably, the response rates for automated telephone surveys tend to fall disproportionally with an increasing number of questions which imposes a strong focus on a few hypotheses. This rules out, for instance, a comprehensive analysis of mediating variables. On the flip side, the limited number of variables reduces the risk of multiple hypothesis testing.

[7] Due to sample size restrictions, we were not able to include a third treatment arm consisting of CHOICE only in our experiment.

[8] In Section VI, we also argue that the effects on the intensive margin between GENERIC and CHOICE should be more similar than on the extensive margin.

intensive margin can only be estimated on a selected sample where Lee (2009) bounds show that results are not stable.

In order to conceptualize our results, we adapt the learning model of selective inattention of Hanna et al. (2014) to the relevant case of home production in poor rural villages. In our model, households face an abundance of potential features that might affect home production but cannot possibly attend to everything because attention is effortful.[9] Since households can only learn about the dimensions that they attend to, this choice becomes important for the learning process. Given their prior belief, they optimally choose what feature to attend to by weighing the returns of doing so with the costs of paying attention. These costs increase with the complexity of features (e.g., the number of variants of an input). If a household falsely believes an input is not useful, it will not invest and subsequently loses the opportunity to learn about the real returns, resulting in a learning failure and sub-optimal equilibrium.[10]

In our experiment, we argue that households did not pay attention to the generic information because it would have been effortful to translate it into an optimal investment. While GENERIC informs people about solar lamps in general, households realize that many variants of the technology exist and that certain features are more and less important for their production. However, it is not obvious which are the critical ones and, thus, they anticipate it would require several iterations to develop an understanding of the way lighting (and a solar lamp more specifically) enters their production function. This argument is supported by our finding of a negative effect of GENERIC on a measure of locus of control—to the extent that the latter indicates that households felt overwhelmed with translating the information into optimal investments (i.e., purchase the right solar lamp for a specific household need).[11]

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[9] While the model assumes that attention costs are ex-ante the same across individuals, we earlier made the point that in a poverty setting these costs can be quite high because of the many urgent issues they face and that require their full attention such as income and expenditure shocks (e.g., loss of crops due to extreme weather event or catastrophic healthcare expenses).

[10] In the study context, optimization failure due to attention costs implies that households underinvest in quality solar lamps that have the lowest cost per lumen among locally available lighting solutions. A simple indicator of its presence is that participating households had difficulties to explain their choice of lighting technology (Hanna et al., 2014). Another indication is that study participants strongly underestimated the durability of quality solar lamps. For instance, when a subsample of households was asked to estimate the durability of one of the most common quality certified solar lamp that was displayed to them, on average, they expected it to turn non-functional 16 months (52 months) before the undisclosed end of the warranty period (expected lifetime according to the social impact manufacturer). This suggests that they did not spend time and effort to learn about important aspects of the particular segment of the market that provides lighting at least cost per lumen.

[11] Households were asked how much they feel that what happens to them is because of their own doing. Given the design of the survey, we argue that respondents were primed when answering this question. Specifically, it

In contrast, households that employed the technology regardless of the campaign paid attention to GENERIC as evidenced by the findings of positive impacts on perceptions of the quality of solar lamps and the number of owned devices. We argue that, on average, they potentially believed the information was of higher value because they had better priors of solar lamps than the general study population to begin with. Alternatively, their shadow costs of paying attention were likely to be, on average, considerably lower than for the average household in our sample because they were already in the process of developing a better understanding of solar lamps. Consistent with this argument, information on optimal lamp types was found to be effective for the general target population since it reduced the need for iterative learning and the associated data processing effort.

The paper proceeds as follows. Section II explains the experimental setup and provides additional information about the interventions. Sections III and IV describe the experimental data and estimation method. Section V and VI present the results and the theoretical model that rationalizes the findings. Section VII analyzes mechanisms and discusses alternative explanations. Section VIII concludes.

II. The Experiment

A. Experimental Design

The study follows a cluster randomized design equally dividing 150 villages into a control group or one of two treatment groups. A third of the villages was exposed to GENERIC, in the form of radio clips with the general information that solar lighting products are of high quality and allow households to carry out a series of activities just as easily in the evenings as during the day. The clip lasted for 30 seconds following a traditional format (the script is presented in Section A2 of the Appendix). During the study period, approximately eight radio clips were broadcasted daily in a popular nationwide radio station at prime-time hours in the morning and evening.[12] Another 50 villages, in addition to GENERIC, were exposed to CHOICE through print materials. Specifically, flyers and posters displayed a variety of product variants along with an illustration of the specific types most suitable for all main technological applications (e.g., one illustrated a boy and a girl using a variant where the lamp is on top of a stand lighting up a specific area, i.e., textbook, for optimal illumination, see Figure A1 in the Appendix).[13]

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occurred directly after five questions focused on ownership and knowledge of solar lamps as well as another set of four questions on treatment exposure.

[12] Between 6:30am-9:30am and 5pm-8:30pm. The radio clips were in languages primarily spoken in the study area (French, Wolof, Serer, Djola, and Pulaar).

About 100 flyers were pinned to doors of houses in reasonable proximity (e.g., radius of 15 minutes walking distance) to the center of each treatment village, reaching approximately 25 percent of the village population. In addition, about five posters and 100 flyers per village were distributed at frequented locations and places, such as local shops.

Randomization was stratified by local markets to ensure balance of the supply of the technological innovation across experimental groups. At the majority of local markets, unaccredited lower cost solar technology options were available, ranging from below CFA 1,000 (cheapest; mostly solar torches) to CFA 5,000 (most expensive, mostly solar lighting products with ability to charge phones). To ensure availability of high-quality solar lamps in the study area, we partnered with the social responsibility arm of the company Total, which is promoting solar technology in Senegal. At each local market, Total sold solar lamps with accreditation under the Word Bank Lighting Africa initiative (all above CFA 5,000, i.e., approx. USD8.4).[14]

B. Methodology to Examine Radio Broadcasts

For the identification of the effects of the generic information provided by the radio broadcasts (used in both treatment arms), we followed a similar approach taken by Berg and Zia (2017). Households were encouraged to tune into one of two similar radio stations. Households residing in treated villages were asked to listen to national channel *Radiodiffusion Télévision Sénégalaise* (RTS), which was broadcasting the solar lamp clip during the intervention period. Households residing in control villages were asked to listen to national channel *SUD FM*, which did not broadcast the solar lamp clip. These stations were selected as they target similar segments of the population.[15] As encouragement, we told households that they would have a chance to win a cash reward of the value of CFA 20,000 (approx. USD 34) for listening to the assigned radio station. The cash reward amounts to roughly two monthly incomes of an average rural Senegalese household (Peters et al., 2013). We conducted call-in contests twice a day with each radio station throughout the intervention period. The radio stations would air a contest radio clip which announced that the tenth caller after the audio countdown would win the prize.[16] To account for the relative proportions assigned to treatment and control stations,

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[13] While households are informed about the existence of a variety of lamp types, only a subset of these are displayed in action. The treatment therefore gives a clear understanding of the recommended product type to be used for a technological application.

[14] A description of the products and information about their sales price can be found here. Regarding accreditation, the Lighting Arica initiative developed Pico-PV Quality Standards Lighting Global.

[15] We show in the result section that the profiles of listeners of the two channels are indeed very similar.

[16] Enumerators instructed respondents about the details of the contest at baseline. Specifically, they advised households to call the dial-in number (at no charge) as soon as they heard the countdown at their assigned radio channel to maximize the chance of being the tenth caller. They also handed out a flyer with summary

the treatment station (RTS) rewarded both the tenth and eleventh callers such that there were twice as many winners among households residing in villages assigned to RTS than among households residing villages assigned to the control SUD FM station. The script of the contest clips is presented in Section A2 of the Appendix.

As part of the encouragement design, callers received an automated feedback text message on their mobile phone within 24 hours, indicating whether they called before the countdown, were among the first nine callers, the winner, or called after the tenth caller.[17] The cash transfers were implemented via the service provider Wizall. We opted for the call-in contest rather than a lottery as in Berg and Zia (2017) because, according to the study's formative research, households in the study area are frequently invited to marketing campaigns that follow a lottery format but typically do not have experience of people winning prizes through these campaigns. Hence, trust in marketing campaigns is generally low. By naming it a call-in contest, we aimed to avoid an association with other marketing campaigns. We aimed to increase trust in the process by giving households both (the feeling of) slight control over the odds of winning a price and comprehensive feedback on how close they were to winning on each attempt.

The evaluation design of radio broadcasts addresses three common concerns when evaluating mass media broadcasts. First, participants may be more likely to listen to the radio than if they had not been incentivized. We note that radio listenership in Senegal is currently very high.[18] As such, we expect the incentive to be more likely to induce people to listen to a particular radio station rather than inducing to listen to radio at all. Second, when listening to the radio, participants might pay closer attention to the radio messages than they would do otherwise. We do not base rewards on the remembering of content from the show, and the radio component of the campaign occurs during advertising breaks where the contest was not aired. In doing so, we arguably mitigate the concern that people are artificially induced to listen more intently to the materials than they would normally do, an advantage over the Berg and Zia (2017) study. Third, self-selection issues may arise from inherent differences to the radio stations. However, the stations were selected based on listenership,

information about the call-in contest, including the timing of the contests (morning and evening throughout May 2016) and the dial-in number of the respective radio station.

[17] Only phone numbers provided in the baseline survey were able to establish a connection with the dial-in number to avoid unnecessary traffic on the line and restrict cash transfers to the study population. In the rare event of households participating in the call-in contest of the wrong radio channel, they received a text message with the information about their ineligibility for the cash price under this dial-in number together with the dial-in number of the right radio station.

[18] Our baseline data convey that more than 69 percent of the population listened to the radio during the previous day.

similarities of content, available slots, and their time of the day (among other logistical factors) in order to secure the comparability of listeners. This selection criteria alleviates the risk of confounding factors having an influence on the variables of interest.

III. Data

A. Sampling Framework

This study was conducted in two regions of Western Senegal: Thiès and Diourbel. The two regions were selected based on an exhaustive review of national radio signals, density of weekly local markets called *Loumas*, and proximity to the capital. Both were also among the eight Senegalese regions selected by Lighting Africa as priority targets for its information campaign.

Thiès and Diourbel are the most populated regions in Senegal after the Dakar region, with 1.7 million and 1.4 million inhabitants, respectively. Although they have the highest population density after Dakar[19] both regions remain greatly agricultural: about 52 percent and 43 percent of households live from agricultural production in Thiès and Diourbel, respectively, close to the country average (50 percent).

Within the two regions, we identified weekly local markets and searched for villages within a radius of 8 km to those markets. Those villages were considered proximate enough that a villager without access to transportation could visit the market and return to the village during daylight in one day. We only considered villages with approximately more than 100 inhabitants and national radio signal. Among the 169 eligible villages, we randomly selected 150 to be part of the experiment.

At baseline, enumerators visited two villages per day, completing as many surveys as possible with a minimum of 40 surveys per village. The village center was identified as the starting point for the listing in each village, which was typically the community center or village leader's office, and the exercise systematically worked through proximate residential properties. A household was defined as a group of individuals living together and putting together part or all of their resources to meet their basic needs (housing and food in particular). To be eligible for the study, households were required to either own a mobile phone or have direct access to one.

While power calculations suggested that 50 villages per treatment arm and 20 households per village would be a large enough sample to be able to detect an economically meaningful effect, we aimed for a substantially larger number of surveys per village because of potentially high survey non-response rate at end line due to the employed survey methodology described below.

[19] With 256 and 294 inhabitants per km^2; against 65 inhabitants/km^2 on average in Senegal.

B. Surveys

The data set includes a face-to-face survey, two mobile phone surveys (baseline and end line), and call-in data collected in 2016. While the face-to-face baseline survey was conducted during March and the first two weeks of April, the baseline mobile phone survey was implemented in the last two weeks of April. The call-in data was recorded throughout the intervention period (May), and the end line mobile phone survey was carried out in June.

Prior to the baseline face-to-face survey, households were randomly assigned to a long survey or short survey.[20,21] The long survey collects comprehensive household and individual-level information, while the short survey includes only very basic listing information. Short survey participants were invited by the enumerator at the end of the face-to-face interview to participate in the second part of the baseline survey, which would be conducted shortly after the face-to-face interview via mobile phone using interactive voice response (IVR) technology.

Mobile phone surveys are required to be concise in order to achieve satisfactory response rates, usually limiting the number of questions to roughly ten. The mobile phone survey is restricted to the outcome variables described below. Enumerators briefly trained households in the use of the technology by conducting mock surveys and providing a written summary of instructions. Senegal is classified as a good candidate for phone surveys based on linguistic fractionalization and mobile-phone penetration rate (Ben et al., 2015). The endline mobile phone survey consists of 15 variables and was administered to all households that had completed the (short) face-to-face baseline survey.

C. Variable Description

The primary outcome of this experimental study is the increased adoption of solar lamps (Coville et al., 2015). Specifically, outcome indicators are binary indicators for the extensive and intensive margins of solar lamp adoption. For the extensive margin we use dummy variables for whether the household has ever owned a solar lamp. For the intensive margin, households were asked whether they currently own between one and five or more than five solar lamps. We construct two binary variables whether the household currently owns more than one solar lamp and more than five solar

[20] Random assignment to survey type was stratified by village and enumerator. Only the first three households and the fifth, sixth, and seventh households surveyed by an enumerator in a given village were considered for assignment to the long survey. By implication, the fourth household (serving as buffer) and all households visited by the same enumerator after household seven automatically received the short survey.

[21] Enumerators were instructed to conduct the face-to-face interview with the household head. If the household head was not available for the interview, the spouse of the household head, the oldest child of the household head above the age of 18, or any other adult who is a household member was interviewed (in this order).

lamps, respectively.[22] We also use an indicator of whether households currently own a lamp of superior quality. Here, quality is approximated by the price of the most recently acquired solar lighting product which, according to formative qualitative field research, is a good indicator for the quality of the adopted technology.[23]

Solar energy knowledge is considered a secondary outcome of interest. Specifically, we use dummy variables for awareness and understanding of pico-PV technology. A household is deemed aware if it reports to use solar lighting products as their primary or secondary lighting source (in response to an unprompted question). A household is also considered aware if it correctly selects 'sun' among four possible choices (`dry cell battery', `wind', `sun', `mud') when asked which sources of energy are able to power a lantern or torch.[24] For solar technology understanding, a household is assigned the value one if it reports that a sunny weather is the best condition for the generation of solar power and zero otherwise.[25]

Additional secondary outcome variables consist of measures for quality expectations and locus of control. For the former, participants were asked whether they have heard of the Lighting Global Quality Standards (developed by Lighting Africa) and their expectations regarding the lifetime of a typical solar lamp, which was used to construct a binary indicator for whether expected durability exceeds six months (quality expectation). For comparison, a standard battery-driven torch has a maximum warranty period of two months in rural Senegal. Also, people may not be familiar with other quality features such as brightness measured in lumens. For the locus of control measure, participants were asked whether they feel that what happens to them is their own doing (binary indicator takes the value 1) or that they mostly lack control over the direction their life is taking (it

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[22] We constructed these outcome indicators based on data from the face-to-face and mobile phone surveys. At baseline, we observe this information only for households that demonstrate basic awareness that the sun is an energy source that can power a lantern or torch (see the definition of the variable related to pico-PV knowledge for more details). Following the survey protocol, we infer that those who do not demonstrate basic awareness have never owned a solar lighting product and adjust all other solar lamp adoption variables accordingly.

[23] A price below CAF 1,000 indicates that the solar lighting product is a torch. These are not considered solar lamps in our study. A price above CAF 5,000 commonly indicates that the solar lighting product can emit lumen in all directions and is better able to light up an entire room.

[24] A change was made to the face-to-face baseline questionnaire that affects this variable. A fifth choice option (`fire') was removed four days into survey implementation. To eliminate any potential bias from this change, all values recorded prior to this date were set to zero. Also, the response set differs slightly between the baseline and end line surveys. Instead of `battery', the option `all of the above' was used in the second mobile phone survey.

[25] At baseline, we have this information only for households that demonstrate basic awareness of solar energy. We have additional baseline information in relation to understanding of pico-PV technology. Households were asked whether they would place the solar lighting product inside the house, outside the house, or outside the house in the sun to maximize lighting hours.

takes the value 0). A concise summary of the definitions per outcome variable is presented in Table A1 in the Appendix.

To measure intervention exposure, at the end of the endline survey we asked households what radio stations they listened to most during the intervention period, whether they heard the radio clip (after playing a short audio sequence as part of the automated survey), and whether they received a flyer about solar lighting products during the intervention period. We also documented participation in the call-in contest using call-in data records.

D. Study Participants

The baseline survey yielded 5,887 short surveys and 1,198 long surveys, for a total of 7,085 (above the 1,000 households required by the power calculations). Baseline descriptive statistics for the study population are displayed in Table 1 (Column 2).

More than three-quarters of households did not have access to grid electricity, with dry cell batteries (73.2 percent) and candles (36.9 percent) being the most common energy sources in our study population. Awareness and understanding of solar energy is quite high. For instance, the majority of households that are aware of pico-PV technology (81 percent) identify 'sunny' as the condition most conducive for solar lighting products (second panel). Despite these high levels of awareness, less than 15 percent of households report to have ever owned a solar lamp and approximately 3 percent report to have ever owned a superior-quality solar lamp (top panel of the table). The average age of the study population is 45, and survey respondent's gender is approximately equally represented (53 percent men, 47 percent women; third panel of the table).

Of the households eligible for the baseline mobile phone survey, one-third completed the entire survey. Considering that in our study non-response is higher for mobile phone surveys than face-to-face surveys (statistically meaningful non-response rates are observed only for short-survey households), this yielded a disproportional drop in baseline information of about 45 percent (from 7,085 observations to 3,148).[26] However, despite the different response rates for both data collection methods, analysis of the follow-up data suggests that attrition rates are evenly distributed across experimental groups. Table A2 displays results from a regression of a dummy variable that indicates sample in- and outflows on the treatment indicators for the main outcome variable for the entire sample and separately for households participating in the face-to-face as well as mobile phone surveys at baseline. Attrition rates are orthogonal to treatment status, which we take as indication

[26] Mobile phone response rates for our main outcome variables were slightly higher as we placed them at the beginning of the questionnaire anticipating increasing non-response rates with survey duration.

that sample attrition is not very concerning for the internal validity of our estimation results. Figure 1 displays the timeline of the experiment along with the number of observations.

IV. Estimation Strategy

An experimental design is preferred over an alternative evaluation design based on observational data due to the difficulty of establishing causal relationships between information interventions and investments into the technology. For example, households with a large family network may have better access to information and, at the same time, are more likely to invest in technology innovations. The village-level randomization is motivated by potential information spillover effects.

Theoretically, there is no need to account for the baseline values of the outcome variable (or covariates) in the econometric model. However, randomization may yield a random imbalance in a finite sample. We use a difference-in-difference estimator in our analysis of the information intervention effects on the *extensive margin* of technology investment that controls for (in the present case statistically insignificant) random imbalance in our outcomes. This also likely reduces the variance of the estimate of the coefficient of interest and, hence, increases the efficiency of our estimation. The specification is:

$$Y_{ijt} = \alpha + \gamma_1 T_{Aj} + \gamma_2 T_{Aj} T_{Bj} + \gamma_3 \delta_t + \gamma_4 T_{Aj} \delta_t + \gamma_5 T_{Aj} T_{Bj} \delta_t + \sum_{k=1}^{n} \theta_k L_k + \epsilon_{ijt}, \quad (1)$$

where, Y_{ijt} is the outcome of interest (having ever owned a solar lamp), for household *i* in village *j* at time *t*, T_{Aj} is a dummy variable equal to one if village *j* is assigned to receive GENERIC and zero otherwise. Similarly, T_{Bj} is a dummy variable equal to one if village *j* is assigned to receive CHOICE and zero otherwise. The interaction term $T_{Aj} T_{Bj}$ indicates villages where households received both campaign components. The time fixed effect is indicated by δ_t. L_k is a dummy stratification variable equal to one if the household comes from a village linked to local market *k* and zero otherwise.[27] ϵ_{ijt} is the error term. We run the regressions on a repeated cross-section of households and cluster standard errors at the village level.[28]

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[27] We also include local-market time-fixed effects.

[28] We account for systematic differences in the study population even though they are orthogonal to treatment. The first is a survey dummy to indicate whether the participant received the long or short survey at baseline, which, at the same time, indicates whether a particular household information was collected through face-to-face interview or automated phone survey. The other control is a time dummy variable that indicates data collected prior to a change made to a question which speaks to the variable regarding awareness of solar energy (see also Footnote 12). Due to the way the survey was designed, this variable was partially used for the construction of our outcome variables (Table A1 in the Appendix). The inclusion of both variables serves to remove any statistical abnormalities that may arise due to the different nature of underlying data sources.

The coefficient γ_4 represents the difference-in-difference point estimate of the impact of GENERIC alone while γ_5 provides the estimate for GENERIC in combination with CHOICE. By means of an *F*-test of the difference between γ_4 and γ_5, we examine whether information characteristics drive effects on technology investment. To examine the effects of CHOICE alone, we re-estimate the same regression model on a restricted sample consisting of the call-in contest non-participants (and on a restricted sample consisting of the call-in contest participants).

To maximize sample size, we use the post-intervention cross-section for the analysis of the *intensive margin* of investment into the technology. The specification is:

$$M_{ij} = \alpha + \gamma_1 T_{Aj} + \gamma_2 T_{Aj} T_{Bj} + \sum_{k=1}^{n} \theta_k L_k + \epsilon_{ijt} \; if \; Y_{ij} > 0, (2)$$

where, *Mij* is a binary indicator for owning more than one and more than five solar lighting products, respectively.[29] Besides the quantity of solar lamps, we examine intensive margin effects in terms of the quality of the owned solar lamps (i.e., whether solar lamp is of superior quality). We then combine the two by analyzing the treatment effects on the quantity dummy variables for households reporting to own a superior solar lamp at endline.

V. Results

Baseline balance

Table 1 displays differences across the experimental groups at baseline. Villages are balanced across all observable village-level characteristics. Households show balance across all primary outcomes that we study including intensive and extensive measures of lighting product ownership. Of the 16 variables, which we run baseline balance checks on, we find two to be imbalanced at the five percent significance level: monthly income and the use of dry cell batteries. Differences for these variables appear small in magnitude.[30]

[29] To check the sensitivity of the results with respect to selection along the dimension of the extensive margin (i.e., treatments are correlated with the likelihood of having ever owned a solar lamp), we estimate bounds suggested by Lee (2009).

[30] There is imbalance on baseline radio station listenership as well. Yet, this appears to be driven by an operational artefact: people were mistakenly exposed to some information about the lottery shortly before they were asked to respond to the survey questions regarding radio listenership. Since treatment and control groups were assigned to participate in lotteries for different radio stations, the operational misstep is consistent with the direction of the imbalances whereby control households report being more likely to listen to SUD FM and treatment households report being more likely to listen to RTS. Since this difference is itself likely to be driven by the intervention and could be interpreted as a partial impact of the program, we disregard this information in our analysis and present differences across experimental groups at end line only. That said, we find that the two treatment groups are also significantly associated with listening to the treatment radio

While, proportionally, this number of imbalances is statistically expected through the randomization procedure, these are variables that may be correlated with our main outcomes of interest. In a robustness check, we include these variables as covariates to the first specification and find that our estimates do not change in sign or significance (see results section).

Program Exposure

We first provide reassurance that our careful selection of radio channels was successful in terms of the similarity of their broadcasting by examining characteristics of their regular listeners using our baseline data. If programs are similar, we expect that they also appeal to broadly the same audience. Table 2 presents descriptive statistics by radio channel listenership. With the exception of age, we find no statistically significant differences between participants who reported to listen to SUD FM (control channel) and RTS (treatment channel) most of the days, respectively. In fact, the two audiences appear to be very similar with respect to gender, household size, income, main outcome variables, and knowledge about the new technology.[31] Program exposure is assessed in two ways as presented in Table 3: measuring listenership through verified lottery call-in records, and through self-response of radio listening practices. Overall, we find evidence that our encouragement approach worked. First, approximately 33 percent of our entire sample called the radio station during a call-in contest event. By matching telephone numbers, we track all call-ins made for the lotteries held during the radio sessions when solar lighting promotional clips were being run. The encouragement was also successful in incentivizing control and treatment households to tune to their respective radio channels. Both groups record statistically similar call-in rates and participation. Only 1 in 500 people called to the incorrect radio station (this was also balanced across experimental groups).[32]

Second, we observe an increase of self-reported listening rates for both groups. We find that control households are 30 percentage points more likely to report listening to SUD FM as their main radio station compared to treatment households during the intervention period. Treatment households are 25 percentage points more likely to report listening to RTS. We also played a sequence of the radio clip as part of the end line survey and asked people to report whether they had heard it before.

station (RTS) in a difference-in-difference regression, which accounts for these baseline imbalances of radio listenership.

[31] The share of solar lamp owners is lower among participants with missing information about radio listenership which explains differences in the means between Table 1 and Table 2.

[32] In this case, a control household assigned to listen to SUD FM was registered as calling in to RTS or vice versa for the treatment group.

Even though treatment households were more likely to listen to RTS where the clips were played, they did not report being more likely to have heard them. In contrast, there is a large and statistically significant difference in recall of the print material where the combined treatment group is 10–13 percentage points more likely to have reported being exposed to flyers or posters.

Results for the Extensive Margin of Investment

Table 4 presents results for the effects of the information interventions on the *extensive margin* of investment into the profitable technology. GENERIC did not significantly change the likelihood that a household has ever owned a solar lamp. In contrast, we observe a significantly positive effect when this treatment is augmented by CHOICE. The point estimate amounts to 6.4 percentage points which corresponds to a 20 percent increase relative to the control group.[33] The difference between the coefficients of GENERIC and the combined treatment (GENERIC+CHOICE) is of similar size but not statistically significant (p-value 0.13).

In order to analyze whether information on optimal lamp types alone caused the effects on the adoption of solar lamps, we run the same regression separately for participants and non-participants of the call-in contest. Note that the control group was also encouraged by means of a call-in contest to listen to a "control" radio channel.[34] Selection of participants into the radio call in contest did not differ between the radio clip groups and the control group (Table A3 in the Appendix). Our results show that the effect of the combined treatment is observed among both households that did and households that did not participate in the call-in contest (Columns 2 and 3 of Table 4). Moreover, the estimated coefficients are roughly the same for both subgroups. This suggests that the statistically significant point estimate of the combined treatment is attributable to the effect of CHOICE. This finding is confirmed when splitting the sample by more direct (proxy) variables for exposure to the radio clip (Table A4 in the Appendix).[35]

We run several robustness checks for our results. First, to check whether imbalance of baseline variables may be influencing results, we run regressions with these indicators as covariates

[33] The rate of solar lamp ownership in the control group accounts for the positive coefficient of the time fixed effect.

[34] We observe a positive and statistically significant association (p-value 0.08) between call-in contest participation and the likelihood of having heard the radio clip for the radio clip treatment groups. We find a negative and statistically insignificant coefficient of call-in contest participation and the likelihood of having heard the radio clip for the control group. Results are available upon request.

[35] Results in Table A4 in the Appendix need to be interpreted with caution because the sample is split based on variables that are likely subject to self-selection of participants.

(results displayed in Table A5 of the Appendix).[36] The addition of covariates yields qualitatively the same results as our main specification (5.5 versus 6.4 percentage points), suggesting that these imbalances should not be driving any our results. Second, we analyze the sensitivity of our results with respect to handling of households with logically inconsistent responses across survey questions and time. Following Devlin et al. (2003), we examine the effects on the estimation results of dropping households with inconsistent responses from the analysis and using the original values, i.e., the raw data. These results including definitions of inconsistent responses are presented in Appendix Table A6. The methods of removal of inconsistent responses and relying on raw data tend to slightly increase our point estimates and strengthen the significance. Importantly, our main results also remain qualitatively unchanged through these alternative methods of dealing with logically inconsistent responses. Third, we check sensitivity with respect to the functional form assumption of the estimator. Marginal effect estimates following probit regressions are almost identical to those obtained from the linear probability (results available upon request). Thus, providing information on optimal lamp types increases the propensity of households having ever owned a solar lamp. It also suggests that there are no complementarities between the two information interventions.

Results for the Intensive Margin of Investment

For the analysis of the effects of the two information interventions on the intensive margin, we run regressions on subsamples of households which reported having ever owned solar lamps.

As displayed in the top panel of Table 5, we observe a statistically significant and positive effect of GENERIC (Column 1). Relative to the control group, households exposed to the general treatment were more than 9 percentage points more likely to own more than one lamp. There is no statistically significant effect on the probability of owning more than five products (Column 2). Adding information on optimal lamp types does not have an effect on any of the two outcome variables. Columns 3–6 display intensive margin results for participants and non-participants of the call-in contests which corroborate the general pattern of significantly positive effects of the generic information but no independent effects of CHOICE. Interestingly, here, we also observe a statistically

[36] The inclusion of the covariates modifies the difference-in-difference regression as follows: $Y_{ijt} = \alpha + \gamma_1 T_{Aj} + \gamma_2 T_{Aj} T_{Bj} + \gamma_3 \delta_t + \gamma_4 T_{Aj} \delta_t + \gamma_5 T_{Aj} T_{Bj} \delta_t + \sum_{k=1}^{n} \theta_k L_k + \sum_{q=1}^{m} \vartheta_q W_q + \epsilon_{ijt}$, where control variables are denoted by W_q.

significant effect of GENERIC in the regression using more than five solar lamps as the outcome variable.[37]

We further report effects on the probability of owning a solar lamp of superior quality (middle panel) and the number of lamps conditional on owning a superior lamp (bottom panel). In former regressions, neither the coefficient of the general information nor the coefficient of the additional campaign component are statistically significant. In latter regressions, we no longer observe any significant effect of GENERIC on the probability of owning more than one solar lamp. However, the considerably smaller sample size arguably drives the statistical insignificance despite the relatively large point estimates (e.g., Columns 1, 3, and 5 in the bottom panel). On the contrary, adding information on optimal lamp types has a negative effect on the probability of owning more than one solar lamp (p-value 0.09, Column 1). In other words, the impact of GENRIC on the intensive margin can become diluted if it is combined with information on optimal lamp types.

These results for the intensive margin survive the same robustness checks that we conducted to assess the sensitivity of our findings for the extensive margin (results are available upon request). Yet, as in all analyses of intensive margin effects, we are concerned that point estimates can be biased because the estimations are conducted on a potentially selected sample given observed associations between the treatment(s) and the probability of having ever owned a solar lamp. In order to check the sensitivity of our results with respect to selection into the intensive margin estimation sample, we estimate upper and lower Lee-bounds (Lee 2009).[38]

Our estimates for these Lee-bounds for the effects of the general information on more than one solar lamp are positive and tend to be below or around the threshold of statistical significance (p-value 0.1; Columns 1 and 2 of Table A8 in the Appendix). This indicates that even an extremely selective sample is unlikely to yield a different result, confirming that selection patterns are not driving our intensive margin findings of GENERIC. Yet, the estimated Lee-bounds for the interaction term between GENERIC and CHOICE are statistically insignificant. Moreover, a clear tendency for the direction of the interaction effect cannot be observed given opposing signs of the upper and lower

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[37] We find similar results for additional subsamples that vary by the extent of variation in exposure to the radio spots across experimental groups (Table A7 in the Appendix). These estimates need to be interpreted with caution because the sample is split based on variables that are likely subject to self-selection of participants.

[38] This procedure trims the distribution of the outcome variable for the experimental group (treatment or control) that suffers less from sample attrition during the intervention period (which has relatively more participants with information on the outcome variables, i.e., "excess observations") in the quantile that corresponds to the share of excess observations in this group. Then the difference in means for the trimmed sample of one group and the untrimmed sample of the other group yields the estimated treatment effect bound.

estimated bounds.[39] The same result pattern applies when using the probability of owning more than five solar lamps, except the Lee-bounds for the estimate of the effect of GENERIC turns statistically insignificant (Columns 3 and 4).

Overall, findings from our sensitivity analysis suggest that only the intensive margin effect of generic information is confirmed and that selection effects represent a concern for the point estimates of the combined intervention (and, by implication, information on optimal lamp types alone). The latter is consistent with our previous finding of significant effects of CHOICE on the extensive margin. Hence, in subsequent discussion of the experimental results, we will abstain from interpreting the effects of information on optimal product types on the intensive margin due to concerns about selection effects.

VI. Theoretical Model

To conceptualize our experimental results, we adapt a simple behavioral learning model of Hanna et al. (2014) to the study context that is consistent with our findings. It generates predictions about the impacts of the two campaign components on investment into the technology. We focus on the decision problem of rural villagers in the context of home production. For simplicity, we assume that for N periods a household produces one good, such as a commodity, math scores, social interactions, etc. The production technology has N dimensions j that might matter (e.g., lighting) and within each dimension N variants v (e.g., different types of candles, kerosene lamps, and solar lamps) can be employed. Accordingly, the households chooses a NxN-dimensional vector of inputs, $\mathbf{x}=(x_{11}, x_{12}, \dots, x_{NN})$.

Given the input bundle, the dollar value of each output net of the costs of inputs is: $y_t = \sum_{j=1}^{N} \sum_{v=1}^{N} f_{jv}(x_{jv}|\theta_{jvt}) + \varepsilon_t$, where $\theta = (\theta_{11}, \theta_{12}, \dots, \theta_{NN})$ are parameters of the production function and ε a mean zero shock that is independent across periods.

Importantly, we assume that not all input dimensions j are actually relevant for production and for irrelevant inputs it does not matter what variant v in what quantity is employed. Accordingly, in this case, the household randomly chooses a quantity per variant, i.e., $x^{r}_{jv} \sim N(0, v)$. This means that it does not attend to the input. On the contrary, if an input is relevant, there exists some particular variant and quantity of that variant that produces the greatest output.

[39] Levels of statistical power do not permit testing for the difference between the two point estimates.

We assume that a household does not know θ but has some prior beliefs $\tilde{\theta}_{jv}(x_{jv})$. These are independently distributed across inputs *j* and imperfectly correlate among *v* of the same *j*. This implies that households have general priors about an input but recognize that there is heterogeneity across variants. Household are further assumed to face shadow costs *e* of mental energy and time of choosing x_{jv}, i.e., max $x_{jv}\ f_{jv}(x_{jv}|\tilde{\theta}_{jv})$, and remembering this choice in the next period.

Households which do not attend to an input do not learn about the relationship between this input and the output. The reason is that the input is set at a random in period 0 and households do not know the specific input variant and level in period 1.[1] On the contrary, households that attend to an input learn about the relationship between the output and the level of employed input in terms of variant and quantity.

We further assume that knowledge of how to set input *jv* does (partially) inform the household set input *jv*' (but not in any way how to set another input *j'*). Thus, households learn about the relationship between the output and the attended input more generally. This results in an update of the priors in period 1 to $\tilde{\tilde{\theta}}_{jv1}$ and, due to the correlation between priors for variants of the same input, for all other variants of input *j* to $\tilde{\tilde{\theta}}_{jv'1}$, i.e., $f_{jv}\left(x_{jv}\middle|\tilde{\tilde{\theta}}_{jv1}, \tilde{\tilde{\theta}}_{jv'1}\right)$.

Household decision problem

Households are risk-neutral and maximize the expected undiscounted sum of net payoffs — value of the output minus attentional costs—across the total number of periods. In doing so, they face a tradeoff between the future benefits of experimentation and maximizing current expected payoffs.[2] Households consider an input relevant and attend to it if the benefits of learning about the input outweigh the mental and time costs of doing so:

$$\max x_{jvt}\ \sum_{t=1}^{N} f_{jv}(x_{jv}|\tilde{\theta}_{jvt}) - N * e\ \geq\ \sum_{t=1}^{N} f_{jv}(x_{jvt}^{r}|\tilde{\theta}_{jvt}).\ (3)$$

[40] Our model is flexible enough to allow households to employ several variants in varying quantity of the same input for the production of a good. Importantly, it covers also a common utilization behavior of households that employ only one input variant for producing a good. The scenario of households using one input variant for various goods can be covered through an extension of the model, which, however, would not alter the main prediction about the relative impacts of the two campaign components on the adoption of the technology.

[41] We assume there arise no tangential issues due to multiperiod experimentation, such as considering the degree to which agents are sophisticated in updating given missing information (Schwartzstein 2014).

[2] We assume there arise no tangential issues due to multiperiod experimentation, such as considering the degree to which agents are sophisticated in updating given missing information (Schwartzstein 2014).

Prepositions

In a first scenario, along the lines of GENERIC, we suppose households receive information at the end of period 0 that the net value of output for a subset q of variants of input j (e.g., the subset of lighting products that include solar technology, i.e., solar lamps) is positive: ($y_{j1} > 0$, $x_{j1} > 0$); … ; ($y_{jq} > 0$, $x_{jq} > 0$).

This information simultaneously affects both sides of Equation 3 and, thus, the direction of the predicted impact on the extensive margin is ambiguous. On the one hand, due to this information, households may be able to learn quicker. For instance, assume that the variant with greatest returns belongs to q and a household has very negative priors about it. In this case, it is likely that the information causes the household to evaluate the features of this variant differently and, consequently, is in period 1 more likely to test a variant with some or many of these features. This sets the household up for success in the following trial and error periods (first term). In each trial and error period, the household may achieve larger benefits. Moreover, the household may identify the optimal variant more quickly (i.e., reduction of trial and error periods N in the second term), which increases the number of periods in which it optimally employs the input. On the other hand, information about the lower bound of the returns for variants q increases the expected payoffs of randomly choosing input j for households with negative priors about at least one of them.

In the case a household decides not to attend to the input after receiving this information, it will not invest e to remember it in period 1. Households that consider the input relevant after receiving the treatment (i.e., Equation 3 holds) will remember and incorporate the information when updating their priors in the first period.[3] Priors for variants p that households originally believed to have a non-positive expected output improve to $\tilde{\tilde{\theta}}_{jp1}$ and priors for all other variants s of input j are revised to $\tilde{\tilde{\theta}}_{js1}$. Overall, among these households, priors are expected to change from $f_{jv}\left(x_{jv}\middle|\tilde{\theta}_{jv0}\right)$ to $f_{jv}\left(x_{jv}\middle|\tilde{\tilde{\theta}}_{jp1}, \tilde{\tilde{\theta}}_{js1}\right)$, where $p, s \subset v$; $p \not\subset s$. This update of priors has little impact on the choice of the variants q for which priors were negative to begin with. However, due to positive indirect effects, the relative importance of all variants s can change such that households choose a different variant. For the same reason, the quantity per employed variant is expected to increase.

In a second scenario, along the lines of CHOICE, we suppose households receive information about $\left(y_j\left(x_{jv}^*\right)\right)$, i.e., the monetary returns of employing the *optimal variant and quantity* of input j.

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[42] For illustration purpose, we abstract from additional learning stemming from experimentation in period 0 and any interactions between information obtained from learning by doing and the treatment.

Since household can realize the same benefits by investing e that it otherwise would have realized in period N at shadow costs $N * e$, we expect this information to cause more households to attend to the input. Specifically, households consider input j relevant if the monetary returns of employing the *optimal variant and quantity* exceeds the shadow costs, i.e., $y_j(x_{jv}^*) > e$. Households that consider it relevant will then choose $x_{jv1} = x_{jv}^*$. This affects their choice of the variant and quantity. The same applies to households that already consider the input relevant in the absence of this information.

Differences in the predicted results of the two scenarios

A key advantage in the second scenario is that households are no longer required to engage in a costly trial and error phase to identify the variant with the greatest returns. Consequently, the returns of attending to the input are larger and we predict larger utilization rates of high return input variants (e.g., solar lamps) in the second scenario. This illustrates that the information content matters for the specific choice of input dimension j and the costs of doing so. Once households consider an input as relevant, this utilization rate differential between the two scenarios turn much smaller.

Implications for the interpretation of experimental findings

This theoretical model explains the experimental finding that the optimal product types information was required to increase adoption of solar lamps on the extensive margin but generic information was sufficient for adoption on the intensive margin. Households without previous experience with the technology did not pay attention to the generic information because it would have been costly in terms of time and mental energy to learn about the technological features that maximize returns. Only households that already invested into the technology paid attention to the generic information because for them the shadow costs of doing so was considerably lower (i.e., they could build on knowledge from previous iterations). Information on the optimal lamp types, on the contrary, was less costly to attend to because it made this iterative learning for the most part obsolete.

VII. Potential Mechanisms and Alternative Explanations

In this section, we discuss through which potential channels the campaign components operated and whether these are in accordance with the proposed theoretical model.

In Lighting Africa's originally conceptualized theory of change, the multifaceted campaign was expected to increase people's awareness of the availability of solar lights and understanding of how this technology can be used to improve their lives. We hypothesized that it improves the understanding of the benefits of solar lighting and convey information about the quality of solar lamps. We also examine the effects on a measure of locus of control.

We employ the available measures as outcome variables in cross-section regressions at follow up. [43] Results for the entire study population are presented in Table 6. We find no discernible effect of either intervention on awareness or understanding (Columns 1–3). This is not surprising since levels of awareness and understanding at baseline were relatively high in our sample (as shown in Table 1, three-quarters of households demonstrated basic awareness of solar energy).[44] It suggests that the households in our sample do not lack information about the technology itself.

For product quality perceptions we find effects of the combined intervention but not for GENERIC alone (Columns 4 and 5). Participants who additionally received CHOICE are more likely to (i) have heard of the Lighting Global Quality Standards developed by Lighting Africa and (ii) expect the lifetime of solar lighting products to exceed six months. For the locus of control measure, we find opposing effects between GENERIC and the combined treatment (Column 6). While the provision of GENERIC information deteriorated the participant's locus of control, the addition of CHOICE removed this negative effect.

We conclude from our findings for the extensive margin sample that GENERIC—if at all—negatively affected mediating variables, whereas the addition of CHOICE activated relevant effect channels that are consistent with the theoretical model.

Among early adopters of solar lighting technology, i.e., the intensive margin sample, results show that GENERIC tends to be positively correlated with quality expectations but not any other measure (top panel of Table 7).[45] Its effect turns statistically significant for early adopters of lamps of superior quality (bottom panel of the table).

A limitation of this analysis is that we lack a suitable measure for information about the uses of solar lanterns that would make it possible to assess whether the campaign components improved the understanding of the benefits of solar lighting products (i.e., our data do not allow us to specifically test this mechanism).[46]

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[43] We have baseline data for the awareness proxy measure and the understanding variables (knowledge of natural resources and weather conditions suitable to power lighting devices). Results of differences-in-differences results are qualitatively the same (results available upon request).

[44] In line with this argument, we observe that TYPES affects the extensive margin of households which were aware of solar technology at baseline more relative to households which were unaware at baseline (see Table A9 in the Appendix).

[45] Qualitative interviews with study participants confirmed that the content of the clips improved their ability to distinguish between high-quality and low-quality products, which may have contributed to a change in the perception of the durability potential of solar lamps.

[46] We only have the participants choice from a drop down menu on the ways participants think a solar lantern is of most use in their household. We observe weakly significant shifts from the category "added security" to the categories "better socialize in the evening" and "children can better study" as a result of the combined intervention in the full sample (results available upon request).

Alternative Explanations

While the experimental results are consistent with our learning model of selective attention, other explanations are possible. The print materials may be more credible than the radio clips because they (more clearly) signal authority through the display of the logos of the messenger (e.g., the World Bank and Total).[47] This can explain the asymmetry in the results for the extensive and the intensive margin. Indeed, our finding of effects of GENERIC among early adopters of high-quality products but not among households without any previous experience with the technology seems consistent with this argumentation because credibility becomes less important if the message is congruent with one's own experience. However, if this is the case, it seems unlikely that the radio clips convey any new information as suggested by the effects of GENERIC on expectations about quality of solar lighting products reported in the analysis of mechanisms (see Table 6) to these early adopters since their content (i.e., solar lamps are of high quality) would already be known. It seems more likely that GENERIC gave new information about product quality for early adopters who have evidently demonstrated interest in optimizing lighting levels and therefore may pay close attention to quality signals such as advertisement.

An alternative explanation is that the intensity of exposure to radio spots may be much lower when compared to the print materials. The radio spots are 30-seconds and audiences were exposed at most to 10 radios spots (in other words, 5 minutes) per day. The print materials were distributed to households directly (flyers) and, in addition, placed in areas regularly visited by people such as markets (posters). They remained there for an extended period of time. While (relatively) low intensity of the radio intervention is consistent with the results for the extensive margin, it does not explain why we observe a different results pattern for the intensive margin. We have no indication that exposure to GENERIC was larger among early adopters compared to the general study population. Hence, while other explanations may explain learning failures, limited attention appears to be the most plausible explanation of this key fact.

VIII. Conclusion

This paper proposes and experimentally examines a new explanation for why many public information campaigns have failed to affect decision-making (e.g., Dupas, 2011; La Ferrara, 2016). Due to limited mental energy and time, people have to prioritize to what information they pay attention.

[47] The radio clips just refer to the Lighting Africa Initiative of the World Bank but do not explicitly mention the World Bank.

They will select data that are believed to be valuable after accounting for information processing costs (Handel and Schwartzstein, 2018).

We report effect estimates of two components of an information campaign in relation to a profitable and climate-friendly technology investment in rural Senegal. One component provided information about the benefits and quality of solar lamps in a general way, while another singled out for each of the main applications the most suitable type among a large variety of solar lamps available at local markets and the associated benefit. The significant advantage of the second component is a reduction in data processing costs through simplification of choices.

We find that the provision of information on optimal lamp types was required to increase adoption of solar lamps on the extensive margin. However, providing information about general benefits and quality was sufficient to increase the number of owned solar lamps among existing customers (intensive margin).

In our mechanism analysis, we further show that the generic information component does not seem to affect any intermediate variables in the extensive margin sample, whereas the addition of information about optimal product types significantly increases the perceived quality of solar lamps. The only exception are the results for the locus of control measure that arguably captures the extent to which households felt overwhelmed with translating information of the campaign into optimal investment. Here, we find significant effects of both campaign components. However, while the provision of generic information was negatively correlated with the measure of locus of control, information that simplified the choice set removed this negative effect. Among early adopters of solar lamps, results are different. We show that the generic information intervention is positively correlated with quality expectations. Hence, existing consumers update priors as a result of the generic information.

A simple learning model of selective attention along the lines of Hanna et al. (2014) is presented to rationalize the experimental data. Households face an abundance of potential features that might affect home production but cannot possibly attend to everything because paying attention is effortful. We argue that, in our experiment, households did not pay attention to the generic information because they anticipated that, while they would more quickly identify the lamp type that yields best results, the costs of doing so would still be large. Only households that invested in the technology anyway focused on this information because for them the shadow costs of doing so was considerably lower (i.e., they could build on prior market knowledge). Information on the optimal product-types, on the contrary, was effective because it made iterative learning for the most part obsolete.

We contribute to the literature by being the first to examine the extent to which selective attention shapes the impact of public information campaigns and to compare the effects of addressing information access (La Ferrara, 2016) versus psychological distortions in information- gathering, attention, and processing (Handel and Schwartzstein, 2018). In doing so, we combine two strands of the literature. We find that the latter is crucial for the success of information campaigns. Our results are also of interest for a larger literature on the determinants of learning and information diffusion (e.g., Banerjee et al., forthcoming; Liverpool-Tasie et al., 2019; Paul and Dillon, 2019). They are of particular relevance for studies concerned with improving our understanding of the process of technology adoption and diffusion (e.g., Nelson and Phelps, 1966; Foster and Rosenzweig, 1995; Jovanovic and Nyarko 1996). This is an important area of research because heterogeneity in technology levels can explain significant differences in the performance between economies and among economic agents within the same country (e.g., Caselli and Coleman 2001, Syverson 2011).

In terms of methodological contribution, our study presents a low-cost experimental approach to estimate the effects of two components of a nationwide mass-media information campaign. We conducted a symmetric randomized encouragement design together with automated voice telephone surveys. This approach has two important features. First, it generates exogeneous variation in campaign exposure without randomizing the broadcasting itself (e.g., radio airwaves). This makes the approach relatively straightforward to reproduce. We arguably improve upon Berg and Zia (2011) by delinking monetary rewards from remembering of broadcasting content with the objective of mitigating the concern that people are artificially induced to pay more attention to the information than they would normally do. Second, inexpensive mobile phone surveys compensate for the fact that encouragement designs usually require a relatively large sample size which are difficult to achieve within a standard personal interviewing-based data collection budget. We hope that this low-cost evaluation design can serve as a blueprint for future research and encourages more studies of media broadcasts to reduce the dearth of knowledge in this area (La Ferrara, 2016).

We see an important avenue for future research in generally increasing the evidence base of nationwide mass media campaigns. Also, a more complete understanding is desirable as to what information content is important for campaigns to consider for greater and more predictable impacts on the adoption and diffusion of technologies. This may include generating more knowledge on the implications of selective inattention for individual decision-making and the design of public programs.

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Table 1: Pre-Intervention Descriptive Statistics by Experimental Group (Means)

		Total		Control		GENERIC		CHOICE	
Variable	N	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Outcome Variables									
Has ever owned solar lighting product:[#]									
Yes	3801	0.142	0.531	0.153	0.470	0.143	0.540	0.128	0.578
Superior product	3801	0.029	0.228	0.032	0.233	0.030	0.218	0.025	0.237
Currently owns (any quality):									
More than 1 product	3972	0.129	0.495	0.138	0.426	0.124	0.509	0.125	0.551
More than 5 products	3972	0.050	0.290	0.047	0.215	0.054	0.340	0.050	0.306
Energy situation									
Awareness									
Demonstrates basic awareness of solar energy	7085	0.727	0.760	0.721	0.755	0.721	0.743	0.739	0.787
Source to power lantern/torch?									
Dry cell batteries	6585	0.895	0.903	0.895	0.918	0.889	0.934	0.900	0.876
Wind	6585	0.069	0.312	0.067	0.340	0.072	0.303	0.068	0.298
Sun	6585	0.766	0.689	0.765	0.601	0.753	0.705	0.779	0.754
Mud	6585	0.065	0.389	0.067	0.407	0.067	0.383	0.063	0.385
Understanding									
Correct weather condition	1784	0.811	0.422	0.795	0.499	0.817*	0.394	0.821*	0.366
Would place lantern outside	5151	0.829	0.808	0.809	0.797	0.825	0.853	0.851	0.769
Would place lantern outside and in the sun	5151	0.795	0.851	0.771	0.886	0.791	0.899	0.820*	0.755
Energy source									
None	7085	0.311	1.263	0.317	1.149	0.300	1.335	0.317	1.320
Grid	7085	0.242	2.411	0.188	2.064	0.262	2.549	0.276	2.589
Candles	7085	0.369	1.439	0.361	1.380	0.387	1.522	0.359	1.432
Solar lights	7085	0.258	1.290	0.261	1.198	0.269	1.389	0.245	1.296
Dry cell batteries	7085	0.732	1.287	0.773	0.995	0.699**	1.332	0.723*	1.462
Individual characteristics									
Age	7085	45.239	21.442	44.935	19.174	45.491	26.945	45.290	17.332
Gender	7085	0.472	0.982	0.471	1.075	0.480	1.017	0.465	0.860
Household size	2416	12.724	14.323	12.589	13.443	13.551	15.808	12.015	12.773
Monthly income									
< CFA 30,000	2447	0.375	0.608	0.419	0.609	0.361**	0.583	0.346**	0.605
CFA 30,001 – CFA 60,000	2447	0.220	0.433	0.197	0.403	0.233**	0.425	0.229	0.465
CFA 60,001 – CAF 90,000	2447	0.088	0.284	0.074	0.274	0.099**	0.257	0.091	0.315
> CFA 90,001	2447	0.070	0.284	0.077	0.253	0.055	0.276	0.079	0.315
Not sure	2447	0.246	0.504	0.233	0.451	0.252	0.550	0.254	0.511
Local market visit	2497	0.713	0.536	0.717	0.468	0.697	0.558	0.726	0.579
Participated in IVR at baseline	5887	0.333	1.073	0.330	1.026	0.340	1.241	0.328	0.952
Village characteristics									
Estimated population (2009)	150	942.773	529.968	969.340	567.232	989.520	586.442	869.460	424.221
Number of households surveyed in the village	150	47.233	3.933	47.200	4.504	47.200	3.954	47.300	3.334

N: number of observations.SD: Standard deviation. ** and * indicate the difference of the control group at the 5 and 10 percent critical levels of statistical significance. Specifically, we use p-values that stem from linear regressions that regress the baseline characteristic on binary indicators for the two treatment groups, including dummy variables for local markets, survey type, and time prior to the change of the questionnaire response options. Standard errors were clustered at the village level. [#] quality of solar light is approximated by purchasing price (quality: CFA 1,000-5,000; high quality: CFA > 5,000).

Table 2: Descriptive Statistics by Radio Listenership (Means)

		RTS (Treatment)		SUD FM (Control)		OTHER CHANNELS	
Variable	N	Mean	SD	Mean	SD	Mean	SD
Outcome Variables							
Has ever owned solar lighting product:#							
Yes	2079	0.213	0.420	0.220	0.513	0.189	0.477
Superior product	2079	0.043	0.222	0.047	0.225	0.041	0.227
Currently owns (any quality):							
More than 1 product	2240	0.173	0.418	0.184	0.494	0.162	0.463
More than 5 products	2240	0.079	0.312	0.077	0.315	0.055**	0.266
Energy situation							
Awareness							
Demonstrates basic awareness of solar energy	2279	0.626	0.536	0.713	0.591	0.681	0.546
Source to power lantern/torch?	1493	0.708	0.512	0.768	0.393	0.844	0.380
Dry cell batteries	1540	0.829	0.412	0.849	0.432	0.836	0.474
Wind	1540	0.806	0.441	0.820	0.452	0.799	0.518
Sun	2054	0.899	0.347	0.892	0.453	0.911	0.415
Mud	2054	0.103	0.300	0.059	0.215	0.067	0.246
Understanding	2054	0.692	0.513	0.779	0.521	0.723	0.499
Correct weather condition	2054	0.090	0.326	0.063	0.273	0.071*	0.288
Would place lantern outside	2279	0.318	0.591	0.290	0.571	0.294	0.654
Would place lantern outside	2279	0.200	0.715	0.248	0.848	0.232	1.017
and in the sun	2279	0.343	0.644	0.389	0.600	0.398	0.741
Energy source	2279	0.234	0.511	0.231	0.538	0.235	0.599
None	2279	0.787	0.505	0.726	0.610	0.747	0.680
Grid	2279	0.626	0.536	0.713	0.591	0.681**	0.546
Candles	1493	0.708	0.512	0.768	0.393	0.844	0.380
Solar lights	1540	0.829	0.412	0.849	0.432	0.836	0.474
Dry cell batteries	1540	0.806	0.441	0.820	0.452	0.799	0.518
Individual characteristics							
Age	2279	45.62	16.12	46.51**	19.07	43.20	15.82
Gender	2279	0.420	0.581	0.445	0.599	0.461	0.587
Household size	2044	12.529	13.092	13.401	13.629	12.910	14.077
Monthly income							
< CFA 30,000	2279	0.424	0.516	0.352	0.587	0.307*	0.526
CFA 30,001 – CFA 60,000	2279	0.204	0.407	0.244	0.467	0.166*	0.376
CFA 60,001 – CAF 90,000	2279	0.062	0.220	0.091	0.257	0.067	0.271
> CFA 90,001	2279	0.067	0.244	0.067	0.262	0.057	0.226
Not sure	2279	0.244	0.468	0.246	0.522	0.404**	0.551
Local market visit	2125	0.767	0.433	0.745	0.422	0.650**	0.585
Village characteristics							
Estimated population (2009)	2279	923.04	1238.65	932.83	1308.21	941.40	1698.45
Number of households surveyed in the village	2279	47.879	14.854	47.816	15.021	47.732	18.746

N: number of observations.SD: Standard deviation. ** and * indicate the difference of the control group at the 5 and 10 percent critical levels of statistical significance. Specifically, we use p-values that stem from linear regressions that regress the baseline characteristic on binary indicators for the two treatment groups, including dummy variables for local markets, survey type, time prior to the change of the questionnaire response options, treatment indicators and their interaction with radio listenership. Standard errors were clustered at the village level. # quality of solar light is approximated by purchasing price (quality: CFA 1,000-5,000; high quality: CFA > 5,000).

Table 3: Exposure to Intervention and Treatments

	Frequency of call-in contest participation			*Radio Channel*				
	At least once	More than five times	More than ten times	SUD (no radio clip)	RTS (radio clip)	Neither	Heard radio clip	Remembered flyer
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mean	0.296	0.107	0.071	0.261	0.295	0.444	0.585	0.344
SD	0.456	0.309	0.257	0.439	0.456	0.497	0.493	0.475
Coefficient estimates								
GENERIC	-0.005	0.017	0.011	-0.301**	0.258**	0.043*	-0.025	-0.036
	(0.021)	(0.016)	(0.013)	(0.023)	(0.024)	(0.025)	(0.023)	(0.026)
GENERIC+CHOICE	-0.026	-0.012	-0.004	0.046**	-0.047*	0.001	0.015	0.143**
	(0.023)	(0.017)	(0.015)	(0.020)	(0.026)	(0.027)	(0.026)	(0.027)
# of observations	7085	7085	7085	2415	2415	2415	2415	2415

The number of observations for the frequency of call-in participation is based on the estimation sample used for our main result. The value displayed for t-tests are the differences in means across the groups. Standard deviations (SD) are clustered at the village level. ** and * indicate the difference of the control group at the 5 and 10 percent critical levels of statistical significance. Specifically, we use p-values that stem from linear regressions that regress the respective independent variables on binary indicators for the two treatment groups, including dummy variables for local markets. Standard errors were clustered at the village level.

Table 4: Effects of Information Interventions on Extensive Margin

		Participation in call-in contest	
	All	No	Yes
	(1)	(2)	(3)
Time period°	0.160**	0.142**	0.183**
	(0.025)	(0.038)	(0.040)
GENERIC	-0.004	-0.001	-0.010
	(0.018)	(0.020)	(0.030)
GENERIC+CHOICE	-0.016	-0.014	-0.015
	(0.017)	(0.020)	(0.027)
Time period x GENERIC	0.003	0.006	0.001
	(0.030)	(0.030)	(0.034)
Time period x (GENERIC+CHOICE)	0.064**	0.064*	0.064*
	(0.024)	(0.033)	(0.038)
# of observations	6,216	4042	2174

° Cannot be interpreted as genuine time trend due to (i) a deflation of the share of households with a solar light at baseline due to sample composition and (ii) handling of inconsistent responses. As to (i), households who were not aware of solar lighting were assigned the value zero for pico-PV ownership (`ever owned solar lighting product'). Since at baseline we have complete information about solar technology awareness (face-to-face question) but incomplete information about solar light ownership (mobile phone question), households unaware of solar lighting products are overrepresented in the share of households with a solar lighting product. As to (ii), this deflation carries over to the other main outcome variables due to replacement of logical inconsistencies. In line with this argumentation, when limiting the sample to households which were aware at baseline, we see a large decline in the coefficient of the time period variable, i.e., a more reasonable positive coefficient. ** and * indicate the difference of the control group at the 5 and 10 percent critical levels of statistical significance. Also included were dummy variables for local markets, survey type, and time prior to the change of the questionnaire response options. Standard errors were clustered at the village level.

Table 5: Effects of Information Interventions on Intensive Margin

			Participation in call-in contest			
	All		No		Yes	
	(1)	(2)	(3)	(4)	(5)	(6)
Quantity	More than 1	More than 5	More than 1	More than 5	More than 1	More than 5
GENERIC	0.094**	0.042	0.040	0.006	0.192**	0.109**
	(0.043)	(0.029)	(0.060)	(0.043)	(0.043)	(0.029)
GENERIC+CHOICE	-0.058	-0.033	-0.025	-0.016	-0.103	-0.054
	(0.046)	(0.030)	(0.060)	(0.038)	(0.082)	(0.053)
# of observations	657	657	414	414	243	243
Quality#	Superior product		Superior product		Superior product	
GENERIC	0.039		-0.003		0.085	
	(0.046)		(0.055)		(0.074)	
GENERIC+CHOICE	-0.014		0.021		-0.032	
	(0.045)		(0.050)		(0.077)	
# of observations	660		416		244	
Quantity/superior product	More than 1	More than 5	More than 1	More than 5	More than 1	More than 5
GENERIC	0.148	0.017	0.161	-0.026	0.165	0.116
	(0.091)	(0.076)	(0.122)	(0.103)	(0.161)	(0.138)
GENERIC+CHOICE	-0.152*	-0.066	-0.233**	-0.090	-0.042	-0.052
	(0.080)	(0.067)	(0.099)	(0.082)	(0.160)	(0.126)
# of observations	193	193	117	117	76	76

** and * indicate the difference of the control group at the 5 and 10 percent critical levels of statistical significance. Included were dummy variables for local markets. Standard errors were clustered at the village level. # Only the coefficients that represent the estimates of the treatment effects from a difference-in-difference regression are reported, i.e., interaction terms between the treatments and time period.

Table 6: Mechanisms (Extensive Margin Sample)

	Awareness	Understanding		Quality Expectations		Other
	Sun as power source able to charge lantern or torch	Knows best° weather condition to power solar lantern	Recharging costs for torch with dry cell battery but not for solar lanterns	Heard of Quality Standards Lighting Global	Lifetime of solar lantern above six months	Locus of control
	(1)	(2)	(3)	(4)	(5)	(6)
GENERIC	-0.014	-0.002	-0.016	-0.018	0.008	-0.044*
	(0.027)	(0.023)	(0.021)	(0.022)	(0.024)	(0.042)
GENERIC+CHOICE	0.032	-0.009	0.002	0.057**	0.045*	0.048**
	(0.029)	(0.024)	(0.021)	(0.021)	(0.025)	(0.026)
# of observations	1,849	2735	2735	2735	2735	2735

** and * indicate the difference of the control group at the 5 and 10 percent critical levels of statistical significance. Standard errors were clustered at the village level for all regressions. All regressions are based on data from the follow-up survey if not otherwise indicated. ° This difference-in-difference regression includes dummy variables for local markets, survey type, and time prior to the change of the questionnaire response.

Table 7: Mechanisms (Intensive Margin Samples)

	Awareness	Understanding		Quality Expectations		Other
	Sun as power source able to charge lantern or torch	Knows best° weather condition to power solar lantern	Recharging costs for torch with dry cell battery but not for solar lanterns	Heard of Quality Standards Lighting Global	Lifetime of solar lantern above six months	Locus of control
Solar lighting product at end line						
GENERIC	-0.028	-0.010	0.027	0.064	0.076	-0.038
	(0.057)	(0.037)	(0.036)	(0.047)	(0.051)	(0.040)
GENERIC+CHOICE	0.022	-0.012	0.027	-0.005	0.050	0.001
	(0.050)	(0.037)	(0.035)	(0.045)	(0.047)	(0.046)
# of observations	517	660	660	660	660	660
Superior solar lighting product at end line						
GENERIC	-0.078	-0.098	0.069	0.154*	0.229**	-0.004
	(0.117)	(0.063)	(0.066)	(0.086)	(0.088)	(0.093)
GENERIC+CHOICE	0.016	0.055	-0.008	-0.066	-0.008	-0.048
	(0.095)	(0.072)	(0.064)	(0.083)	(0.080)	(0.086)
# of observations	155	195	195	195	195	195

** and * indicate the difference of the control group at the 5 and 10 percent critical levels of statistical significance. Standard errors were clustered at the village level for all regressions. All regressions are based on data from the follow-up survey if not otherwise indicated.

Figure 1: Timeline and Flow Diagram

FEBRUARY 2016
Random selection of 150 villages among 169 eligible villages
Random selection of villages into treatment condition

150 Villages

	CONTROL	GENERIC	GENERIC+CHOICE
MARCH/APRIL 2016			
Household Listing (>40 households per village)			
Listing Survey	2360	2360	2365
Personal Baseline Interview (same day of listing)			
Selected	398	400	400
Participated*	380	389	379
APRIL 2016			
Automated Telephone Baseline Survey			
Selected	1962	1960	1965
Participated*	884	908	861
MAY 2016			
Intervention Implementation			
Participated in Call-in-contest (All Participants Invited to Participate)	723	713	657
JUNE 2016			
Automated Telephone Endline Survey (June)			
Participated* (All Participants Invited to Participate)	789	829	797

*Notes:** Number of observations corresponds to the number of participants with a non-missing value of the main outcome variable (i.e. has ever owned solar lamp). These observations form the extensive margin sample. Respective observations included in the extensive margin sample, which is based on follow up data only, amount to 221 (control), 212 (GENERIC), and 224 (GENERIC+CHOICE).

Appendix – Section A1: Tables and Figures

Table A1: *Variable Definitions*

Outcome Variables

Has ever owned a solar lamp[3]

Takes on the value '1' if the household reports to have ever owned a solar lighting product and paid more than CFA 1,000, '0' otherwise

Has ever owned a superior solar lighting product[2,3]

Takes on the value '1' if the household reports to have ever owned a solar lighting product and paid more than CFA 5,000, '0' otherwise

Currently owns more than two solar lighting products[2,3]

Takes on the value '1' if the household reports to currently own more than two solar lighting products, '0' otherwise

Currently owns more than five solar lighting products [2,3]

Takes on the value '1' if the household reports to currently own more than five solar lighting products, '0' otherwise

Mechanism Variables

Awareness:[1] A binary variable that equals `1' if the household demonstrates basic awareness that the sun is an energy source that can power a lantern or torch

Qualifies as aware at baseline:

- Uses a solar lighting product as primary or secondary lighting source (unprompted)
- ***Or*** selects `sun' but not `mud' among four possible choices (`dry cell battery', `wind', `sun', `mud') as a source to power a lantern or torch

Qualifies as unaware at baseline:

- Does not use a solar lantern as primary or secondary lighting source (unprompted)
- ***And*** does not select `sun' as a source to power a lantern or torch

Qualifies as aware at end line: Selects 'sun' as source to power a lantern or torch

Correct weather condition: (i) A binary variable that equals '1' if the household correctly responds sunny as the best weather condition for solar lighting product among four possible choices (`windy weather conditions', `rainy weather conditions', `cloudy weather conditions', `sunny weather conditions'), '0' otherwise; (2) A binary variable that equals '1' if the household correctly responds that charging dry-cell batteries costs money and, at the same time, solar lantern charging costs are zero, '0' otherwise

Quality expectations: (i) A binary variable that equals '1' if the household responds that it has ever heard of Lighting Global Quality Standards for solar lantern quality assurance, '0' otherwise; (ii) A binary variable that equals '1' if the household expects to be able to use a typical solar lantern more than six months before it breaks, '0' otherwise

Locus of control: A binary variable that equals '1' if the household member responds that his or her feelings these days is best described by the statement "What happens to me is my own doing", '0' if feelings are best described by the statement "Sometimes I feel that I don't have enough control over the direction my life is taking"

[1] A response option 'fire' was removed four days into conducting the survey. To eliminate any potential bias from this change, all values recorded prior to this date were set to `0'. [2] Value set to '0' at baseline for all those who were unaware. [3] Value set to '0' for cases where households report they had never owned a solar lighting product. This is for logical consistency in case of the current ownership variables.

Table A2: Sample Composition (In- and Outflows) in Analysis of Extensive Margin as Function of Treatments

	In-and Outflows		
	All	Face-to face	Mobile
GENERIC	0.019	-0.016	0.027
	(0.017)	(0.032)	(0.018)
GENERIC+CHOICE	-0.012	0.030	-0.021
	(0.016)	(0.033)	(0.017)
# of observations	7,085	1198	5887

** and * indicate the difference of the control group at the 5 and 10 percent critical levels of statistical significance. Also included were dummy variables for local markets, survey type, and time prior to the change of the questionnaire response options. Standard errors were clustered at the village level.

Table A3: Comparability of Participants of Call in Contest Across Experimental Groups

	Has ever owned a solar lighting product
GENERIC	-0.0002
	(0.020)
GENERIC+CHOICE	-0.016
	(0.020)
GENERIC * participant of call in contest	-0.011
	(0.029)
(GENERIC+CHOICE) * participant of call in contest	0.001
	(0.030)
Participant of call in contest	0.050**
	(0.020)
# of observations	3801

** and * indicate the difference of the control group at the 5 and 10 percent critical levels of statistical significance. Also included were dummy variables for local markets, survey type, and time prior to the change of the questionnaire response options. Standard errors were clustered at the village level.

Table A4: Effects of Information Interventions on Extensive Margin by (Proxy) Variables for Exposure to Radio Clip (Subsample Analyses)

	No	Yes
	Listened to radio	
GENERIC	-0.045	0.024
	(0.034)	(0.030)
GENERIC+CHOICE	0.085**	0.024
	(0.037)	(0.033)
# of observations	895	1520
	Listened to RTS	
GENERIC	-0.007	-0.027
	(0.031)	(0.055)
GENERIC+CHOICE	0.061*	0.025
	(0.034)	(0.037)
# of observations	1702	713
	Heard radio clip	
GENERIC	-0.007	0.003
	(0.033)	(0.032)
GENERIC+CHOICE	0.065*	0.037
	(0.035)	(0.036)
# of observations	1003	1412

** and * indicate the difference of the control group at the 5 and 10 percent critical levels of statistical significance. Regressions are based on the cross section of follow-up respondents.

Table A5: Effect of Information Interventions (Controlling for Imbalanced Covariates)

	Extensive Margin	Intensive Margin				
		Quantity		Quality	Quantity/Quality	
		More than 1	More than 5	Superior Product	More than 1	More than 5
Time period°	0.123**					
	(0.028)					
GENERIC	-0.012					
	(0.015)					
GENERIC+CHOICE	-0.015					
	(0.014)					
Time period x GENERIC	0.009	0.090**	0.035	0.030	0.141	0.009
	(0.023)	(0.042)	(0.030)	(0.047)	(0.093)	(0.076)
Time period x (GENERIC+CHOICE)	0.055**	-0.048	-0.026	-0.007	-0.152*	-0.062
	(0.024)	(0.046)	(0.030)	(0.046)	(0.084)	(0.069)
< CFA 30,000	-0.031	0.013	-0.039	-0.136	0.052	0.024
	(0.037)	(0.107)	(0.075)	(0.103)	(0.214)	(0.153)
CFA 30,001 – CFA 60,000	0.021	0.008	0.001	-0.100	0.045	0.090
	(0.039)	(0.110)	(0.080)	(0.102)	(0.232)	(0.160)
CFA 60,001 – CFA 90,000	-0.005	0.066	0.040	-0.041	0.230	0.308
	(0.041)	(0.133)	(0.097)	(0.127)	(0.245)	(0.187)
Conditions	0.099**	0.064	0.035	0.106*	-0.116	0.115*
	(0.024)	(0.080)	(0.042)	(0.064)	(0.209)	(0.068)
Would place pico-PV outside and in the sun	-0.010	-0.084	-0.035	-0.009	-0.037	0.047
	(0.023)	(0.061)	(0.047)	(0.059)	(0.124)	(0.082)
Batteries as energy source to light dwelling	-0.005	0.052	-0.010	-0.028	-0.013	-0.079
	(0.013)	(0.039)	(0.030)	(0.045)	(0.082)	(0.067)
# of observations	6216	657	657	660	193	193

** and * indicate the difference of the control group at the 5 and 10 percent critical levels of statistical significance. Dummy variables for local markets were included and standard errors were clustered at the village level.

Table A6: Robustness Checks for Extensive Margin Results (Variable Definition)

	Permitting Within-time Logical Inconsistencies	Exclusion of Observations With Within-time Logical inconsistencies	Replacing Inter-temporal Inconsistencies##	Exclusion of Observations With Inter-temporal Inconsistencies
	(1)	(2)	(3)	(4)
Time period°	0.072**	0.154**	0.176**	0.319**
	(0.024)	(0.025)	(0.025)	(0.034)
GENERIC	0.012	-0.0024	-0.004	-0.001
	(0.021)	(0.019)	(0.018)	(0.022)
GENERIC+CHOICE	-0.021	-0.017	-0.016	-0.017
	(0.021)	(0.019)	(0.017)	(0.024)
Time period x GENERIC	-0.013	0.001	0.002	-0.015
	(0.024)	(0.023)	(0.023)	(0.028)
Time period x (GENERIC+CHOICE)	0.069**	0.065**	0.070**	0.048*
	(0.025)	(0.024)	(0.025)	(0.026)
# of observations	6,216	6,031	6,040	6,674

Households are automatically assigned the value 1 at end line if they reported to having ever owned a solar light at baseline regardless of the entry at end line (even if they report not having ever owned a solar light at end line° Cannot be interpreted as genuine time trend due to (i) a deflation of the share of households with a solar light at baseline due to sample composition and (ii) handling of inconsistent responses. As to (i), households who were not aware of solar lighting were assigned the value zero for pico-PV ownership (`ever owned solar lighting product'). Since at baseline we have complete information about solar technology awareness (face-to-face question) but incomplete information about solar light ownership (mobile phone question), households unaware of solar lighting products are overrepresented in the share of households with a solar lighting product. As to (ii), this deflation carries over to the other main outcome variables due to replacement of logical inconsistencies. In line with this argumentation, when limiting the sample to households which were aware at baseline, we see a large decline in the coefficient of the time period variable, i.e., a more reasonable positive coefficient.). ## Households are automatically assigned the value 1 at end line if they reported to having ever owned a solar light at baseline regardless of the entry at end line (even if they report not having ever owned a solar light at end line). If households report at end line that the most recently acquired price is below the respective threshold (CFA 1,000 and CFA 5,000, respectively), this variable takes the value 0. If the price information is missing, the value is set to 0. This variable definition causes that the number of households with a solar lighting product at end line is larger compared to our preferred definition (see Table A1) and so is the chance of missing price information. ** and * indicate the difference of the control group at the 5 and 10 percent critical levels of statistical significance. Also included were dummy variables for local markets, survey type, and time prior to the change of the questionnaire response options. Standard errors were clustered at the village level.

Table A7: Effects of Information Interventions on Intensive Margin by (Proxy) Variables for Exposure to Radio Clip

	Quantity				Superior product		Quantity/Superior product			
	More than 1		More than 5				More than 1		More than 5	
	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)0	(7)	(8)	(9)	(10)
Listened to radio										
GENERIC	0.012	0.127**	0.074	0.013	-0.013	0.079	0.065	0.158	-0.007	0.024
	(0.087)	(0.057)	(0.066)	(0.035)	(0.080)	(0.053)	(0.192)	(0.117)	(0.164)	(0.085)
GENERIC+CHOICE	-0.002	-0.093*	-0.020	-0.041	-0.016	-0.023	-0.064	-0.150	-0.024	-0.068
	(0.083)	(0.056)	(0.065)	(0.036)	(0.075)	(0.061)	(0.184)	(0.097)	(0.149)	(0.076)
# of observations	231	426	231	426	232	428	66	127	66	127
Listened to RTS										
GENERIC	0.081	0.088	0.029	0.121**	0.016	0.038	0.102	0.235	-0.037	0.167*
	(0.055)	(0.102)	(0.037)	(0.050)	(0.056)	(0.098)	(0.123)	(0.176)	(0.095)	(0.089)
GENERIC+CHOICE	-0.051	-0.084	-0.030	-0.046	0.008	-0.042	-0.123	-0.161	-0.030	-0.076
	(0.060)	(0.068)	(0.039)	(0.0440	(0.005)	(0.081)	(0.123)	(0.140)	(0.087)	(0.119)
# of observations	452	205	452	205	454	206	130	63	130	63
Heard radio clip										
GENERIC	0.066	0.101*	0.025	0.053	0.080	0.032	0.085	0.200	-0.002	0.008
	(0.076)	(0.057)	(0.061)	(0.037)	(0.077)	(0.059)	(0.166)	(0.121)	(0.131)	(0.100)
GENERIC+CHOICE	-0.063	-0.045	-0.120**	0.023	-0.096	0.031	-0.297**	-0.067	-0.239**	-0.017
	(0.069)	(0.062)	(0.049)	(0.043)	(0.068)	(0.057)	(0.146)	(0.120)	(0.114)	((0.086)
# of observations	256	401	256	401	257	403	72	121	72	121

** and * indicate the difference of the control group at the 5 and 10 percent critical levels of statistical significance. Standard errors were clustered at the village level.

Table A8: Lee-Bounds for Estimates of Effect on Intensive Margin (Quantity)

	More than 1		More than 5	
	lower	upper	lower	upper
	(1)	(2)	(3)	(4)
GENERIC	0.089 (0.050)*	0.094 (0.061)	0.037 (0.036)	0.042 (0.046)
# of selected observations/# of observations	415/1617			
GENERIC+CHOICE	-0.083 (0.080)	0.100 (0.056)*	-0.113 (0.036)**	0.028 (0.035)
# of selected observations/# of observations	445/1584			

Table A9: Heterogeneous Effects of Information Interventions by Baseline Awareness

	Solar lighting product
Subsample: households which lack basic awareness that the sun is an energy source that can power a lantern or torch at baseline	
GENERIC	0.017
	(0.035)
GENERIC+CHOICE	0.009
	(0.037)
# of observations	707
Subsample: households with basic awareness that the sun is an energy source that can power a lantern or torch at baseline	
Time period°	0.107**
	(0.029)
GENERIC	-0.028
	(0.029)
GENERIC+CHOICE	-0.032
	(0.029)
Time period x GENERIC	0.021
	(0.028)
Time period x (GENERIC+CHOICE)	0.081**
	(0.031)
# of observations	3,575

° Cannot be interpreted as genuine time trend due to (i) a deflation of the share of households with a solar light at baseline due to sample composition and (ii) handling of inconsistent responses. As to (i), households who were not aware of solar lighting were assigned the value zero for pico-PV ownership (`ever owned solar lighting product'). Since at baseline we have complete information about solar technology awareness (face-to-face question) but incomplete information about solar light ownership (mobile phone question), households unaware of solar lighting products are overrepresented in the share of households with a solar lighting product. As to (ii), this deflation carries over to the other main outcome variables due to replacement of logical inconsistencies. In line with this argumentation, when limiting the sample to households which were aware at baseline, we see a large decline in the coefficient of the time period variable, i.e., a more reasonable positive coefficient. ** and * indicate the difference of the control group at the 5 and 10 percent critical levels of statistical significance. Also included were dummy variables for local markets, survey type, and time prior to the change of the questionnaire response options. Standard errors were clustered at the village level.

Section A2: Details about the interventions

A. Script of the radio clip (translation from French)

Something new in Senegal!
Did you know that thanks to solar lamps, children are now able to study in the evening at home without difficulty?
You can now serenely carry out all your activities just as easily in the evening as you can during the day.
Portable solar lamps are high quality, accessible, and available at affordable prices. They are easy to recharge and some of them can also charge cellphones.
Head quickly to the closest reseller to pick one up.
For more information, please visit the website www.ligthingafrica.org/products
Portable solar lamps, the lighting solution for all!

B. Example of print material (in one of the four original languages)

Figure A1: Flyer

C. Scripts of the contest clip (translation from French)

Clip aired on RTS

Hello gentle participants of the household energy use survey in Thiès and Diourbel! Your survey enumerator previously provided you with all call-in contest instructions. You win the prize if you are the 10th or 11th caller and qualify for the call-in contest on RTS Chaine Nationale. The contest starts in three – two – one seconds: Ring – ring- ring! Please call immediately 328244500 to win the prize. Use your chance again: the call-in contest happens twice a day on RTS Chaine Nationale for the entire month. Good luck!

Clip aired on SUD FM

Hello gentle participants of the household energy use survey in Thiès and Diourbel! Your survey enumerator previously provided you with all call-in contest instructions. You win the prize if you are the 10th caller and qualify for the call-in contest on SUD FM. The contest starts in three – two – one seconds: Ring – ring- ring! Please call immediately 328244505 to win the prize. Use your chance again: the call-in contest happens twice a day on SUD FM for the entire month. Good luck!