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Social Interaction and Technology Adoption: Experimental Evidence from Improved Cookstoves in Mali*

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Abstract

Easy-to-use and risk-free technologies, which require little investment and potentially provide health and environmental benefits, often have low adoption rates. Using a randomized experiment in urban Mali, we assess the impact of a training session in which information on an improved cookstove (ICS) is provided along with the opportunity to purchase the product at the market price. We find direct and spillover effects from our invitation to the session on ICS ownership and usage. We then randomly assign half of the training participants to receive information on a peer's actual purchase. Our results indicate that conditional on receiving information, an individual is more likely to adopt the product if informed about a peer they know and who purchased the product. Our sessions have no discernible impact on product knowledge or household welfare. We argue that social interaction, through imitation, can represent an important channel for increasing take-up and diffusion.

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1 Introduction

The process of technology adoption and diffusion is driven by several factors beyond market mechanisms (Foster and Rosenzweig, 2010). Across diverse fields such as agriculture, health, and financial decision-making, social interaction and peer effects play a relevant role in the decision to adopt a new technology (Foster and Rosenzweig, 1995; Moser and Barrett, 2006; Conley and Udry, 2010; Kremer and Miguel, 2007; Oster and Thornton, 2012; Godlonton and Thornton, 2012; Duflo and Saez, 2003; Beshears et al., 2015; Bursztyn et al., 2014)[1]. Less documented are the mechanisms through which peer effects encourage technology diffusion.

In this paper, we provide evidence of how social interaction affects the decision to adopt improved cookstoves (ICSs), a technology that could bring about efficiency gains through fuel savings. The study takes place in Bamako, the capital of Mali, which has relatively low levels of adoption of ICSs and where most people rely on solid fuels and traditional technologies to cook.

Globally, about 2.74 billion people (40% of the world population) still rely on traditional fuels and inefficient technologies to cook, with severe health consequences due to indoor air pollution (IEA, 2016). The use of wood as the main energy source also negatively impacts the local environment through deforestation, soil degradation, and erosion. Further, inefficient biomass combustion is a major determinant of black carbon, a contributor to global climate change. Emissions from cookstoves continue to be a major component of global anthropogenic particulate matter, particularly in developing regions where they account for well over 50% of such emissions (UNEP/WMOO, 2011; Bond et al., 2004). Access to

[1] Beyond technology adoption, peer effects are also important drivers of educational choices (Bobonis and Finan, 2009; Carrell and Hoekstra, 2010; De Giorgi et al., 2010), job seeking (Magruder, 2010; Beaman and Magruder, 2012), voting and political outcomes (Fafchamps and Vicente, 2013; Galeotti and Mattozzi, 2011), and energy choices (Allcott, 2011; Ayres et al., 2012; Costa and Kahn, 2013). For a comprehensive review of applications, see Jackson (2010).

inexpensive, more efficient technologies such as improved cookstoves (ICSs), can play a role in curbing emissions. An ICS can also bring about health and efficiency gains through fuel savings at the household level. However, despite their potential benefits, the take-up and sustained usage of ICSs remain low in various contexts including the one we focus on (Miller and Mobarak, 2013; Mobarak et al., 2012; Hanna et al., 2016).

We document the impact of a training session in which information on ICSs is provided, an ICS is compared with traditional technologies[2], and the opportunity to purchase one at the market price is given. We randomly selected geographical clusters within the city and randomly assigned women to receive the invitation to the training session. Within each cluster, treated and control women lived far apart to avoid major spillover effects. We find that being invited to the training session increases the probability of owning an ICS by 31 percentage points (160% increase over the baseline value of about 0.2). Similarly, we find that our training session has a positive and significant impact on the frequency and length of ICS usage. These results are obtained six to nine months post-treatment based both on self-reporting and on the Stove Usage Monitoring Systems (SUMS) installed on a subsample of ICSs. We identify and measure positive spillover effects of our intervention on ICS ownership and usage for the group of non-participants living near women who attended the session and for women who participated but did not buy an ICS within our intervention by comparing them with non-invited (control) women. We find that women who did not participate in the session but live close to those who did, as well as women who attended but did not purchase an ICS at the training, are more likely to own and use an ICS at the endline. Overall, we find no significant impact of owning an ICS on welfare, measured in terms of reported fuel expenditure, time spent on income-generating activities, and income. Although the comparison with non-experimental groups may be biased by self-selection, these effects represent suggestive evidence of the social interactions between participants and non-participants.

[2]An ICS is similar to commonly used charcoal stoves, but allows fuel savings of up to 30%.

To shed more light on the mechanisms underlying these social interactions, during the training session, before the purchase decision is individually made, we randomly assign half of the participants to receive an information treatment about another participant's purchase decision. The social relationships among the women attending the training session were mapped by asking each of the women in the sample information about their relationship with all the other women at the session. Women described peers as "unknown", "known by sight", or "whose opinion is respected". We exploit the variation in these pairwise relationships. We find that, conditional on receiving information, women are significantly more likely to buy when they are informed about someone who bought the product and who they know at least by sight. The take-up does not increase as the relationship with the peer becomes more intense: the effect is similar if the information is about an acquaintance known by sight or a respected peer. This effect is not significant when compared to the control group of those who receive no information. We also find that women receiving information on an unknown peer who purchased are significantly less likely to take up the product.

In this context, there are two main mechanisms that might serve as drivers of the observed peer effects: social learning about the technical features of the product, and imitation. While we cannot disentangle their respective role, our evidence is consistent with an imitation channel. Our argument is developed in different steps.

First, we show that the social learning mechanism alone, defined as learning from others about the function or benefits of the technology, is unlikely to be at play. Cookstoves have key characteristics that distinguish them from other technologies investigated in relation to peer effects. ICSs are an established technology known by the vast majority of women in the population (93.6% of the women in our sample knew about them at the baseline). Their design and usage are similar to those of traditional charcoal stoves and thus do not carry significant behavioral changes, adjustments to one's cooking technique, or important informational gaps.

Second, an ICS is a cheap and risk-free technology, which implies relatively little investment. This is widely different from the adoption of new seeds or agricultural practices, which can imply important risks and changes to fundamental sources of livelihood. Starting from these considerations, we test the effects of the training session on the level of knowledge about the product and find no significant effect. While we cannot rule out that some information was passed on through this channel and our measures may not capture all the dimensions of knowledge about ICSs, our results are consistent with the absence of an informational/knowledge gap on this product. We argue that the process of ICS adoption is mostly led by behavioural imitation, where women purchase the product to "keep up with the Joneses"[3].

The first contribution of this study is to examine the influence of peers in the context of ICS adoption. We provide empirical evidence from an intervention in which individuals receive information on a peer living in the same neighborhood. We find that women adopt significantly less when matched to a peer who bought and is unknown. We also show that conditional on receiving information, women adopt significantly more when the peers buy and are known at least by sight. Comparatively, interventions disseminating information about peer behavior have mostly been studied in the form of "social information". These work by informing people about the behavior of an aggregate reference group to motivate them to engage in the behavior of interest. Such designs have been used to study public good contribution, energy use, and financial decisions, and individual behavior shifts toward the peer norm in most cases (Frey and Meier, 2004; Chen et al., 2010; Ayres et al., 2012; Costa and Kahn, 2013; Bonan et al., 2019; Bursztyn et al., 2014; Cai et al., 2015). Our study differs in that we offer information about a specific community member's decision[4]. The design is similar to recent studies in which new technologies are promoted through network injection points (Miller and Mobarak, 2014; Kondylis et al., 2017; Beaman et al., 2018; BenYishay and Mobarak, 2019; Banerjee et al., 2019). Hence, we complement the

[3]Social status related imitation has been investigated by Akerlof (1980); Bernheim (1994); Abel (1990); Bursztyn et al. (2017). See Bursztyn and Jensen (2017) for a review of the empirical findings on social image.

[4]Similarly, Miller and Mobarak (2014) convey purchase decisions by locally identified opinion leaders.

characterization of injection points. Similarly to the role of contact and peer farmers in agriculture, known women represent credible and accessible examples to follow, allowing the technology to spread naturally.

Second, from a policy perspective, we shed light on a potentially important driver of the diffusion of ICSs. The literature on the drivers of and barriers to ICS adoption is still nascent, but liquidity constraints, intra-household preferences, information inefficiencies, and marketing strategies seem to play significant roles (Hanna et al., 2016; Miller and Mobarak, 2014; Mobarak et al., 2012; Bensch et al., 2015; Levine et al., 2018; Meredith et al., 2013; Berkouwer and Dean, 2019; Bensch and Peters, 2020). We complement existing investigations by exploring the role of peer effects in the adoption of ICSs (Miller and Mobarak, 2014; Beltramo et al., 2015a; Adrianzén, 2014) and find results suggestive of an imitation mechanism. We also combine self-reported and objectively measured data for product usage. The results complement a nascent literature looking at monitored usage as a way to mitigate demand and social desirability effects possibly arising when self-reported measures are used (Beltramo et al., 2018).

Third, we contribute to the debate on the impacts of ICSs on welfare-related outcomes (Hanna et al., 2016; Bensch and Peters, 2015; Smith et al., 2011; Berkouwer and Dean, 2019). Consistent with Bonan et al. (2017), who show that the documented impacts from the adoption of ICSs are inconclusive, we do not find greater fuel expenditure savings, additional time for income-generating activities, or increased income among purchasers. Although usage appears relatively high, the substitution of one traditional stove for a more efficient one in our context, where meals are prepared for large families using multiple stoves, is insufficient to significantly climb the "energy ladder"[5].

The rest of the article is organized as follows. Section 2 presents the context, experimental design, sample, and data collection. In Section 3, the data and summary statistics are

[5]This concept implies the movement of households toward more sophisticated energy sources and cooking tools, as their income increases. This may occur through a linear process of fuel switching (Heltberg, 2004; Hanna and Oliva, 2015) or energy stacking (i.e., using both modern and traditional fuels and cookstoves at the same time) (Ruiz-Mercado et al., 2011; Masera et al., 2000).

presented, followed by a description of the identification strategies, results, and mechanisms in Section 4. Section 5 concludes.

2 Study design

2.1 Context and background

Over 95% of the urban Malian population use solid fuels (wood, biomass, or charcoal) for cooking and only 6% have access to clean sources of energy (kerosene, gas, or electricity). Indeed, less than 0.5% of the population use ICSs, that burn charcoal or wood with greater efficiency[6]. Panels *a*, *b*, and *c* of Figure 1 show examples of traditional wood and charcoal cookstoves.

The ICS used in this study, shown in Panel *d* of Figure 1, is produced by local artisans using recycled materials[7]. It has a metal structure and a combustion chamber made of baked clay that retains more heat and saves fuel. Similar to traditional metal stoves, the ICS uses charcoal, is portable, and is typically used to cook outside the house. Its potential benefits are linked more to efficiency than to health. Laboratory tests report that the product allows the user to gain a potential charcoal saving of 30% to 45%. The market price of 3,500 CFA (USD 6) is higher than that of traditional charcoal cookstoves (2,500–3,000 CFA; USD 4.20–5). However, it has been estimated that this price difference can be recovered after around three months of full usage because of its fuel efficiency. The product is available in most local markets. No other models of ICSs with characteristics similar to that examined in this study were available on the markets in Bamako at the time of the study. Appendix A.1 provides more product information.

[6]These figures are from the Global Alliance for Clean Cookstoves. See cleancookstoves.org/country-profiles/26-mali.html, accessed in January 2017.

[7]Similar ICSs have been investigated in Senegal by Bensch and Peters (2013) and Bensch and Peters (2015).

2.2 Experimental design, sampling, and data collection

2.2.1 Training session experiment

From October 2014 to January 2015, we conducted a baseline survey of 1080 women from 36 neighborhood clusters in Bamako: each cluster includes both treated (invited) and control (not invited) households. To obtain a representative sample of the population of the city, we adopted a clustered multi-stage probability sampling. We first constructed a random selection of 36 starting points identified on the map, then associated each to a cardinal point. From each starting point, 25 contiguous houses were selected[8]; then, after a 10-minute walk in the prescribed direction (cardinal point), another five houses were selected adjacent to the arrival location, following the same household selection procedure (see Appendix C.2 for the details of the sampling procedure). Hence, of the 30 houses forming each cluster, 25 were assigned to the treatment and five to the control sample. Some houses are structured as an extended household, locally known as a *gwa*. These include several nuclear family units living in the same compound. Members of a *gwa* eat together from the same pot and may organize a cooking rotation where a different woman in turn (daily or weekly) has to prepare for the entire *gwa*[9]. In a selected nuclear household the woman most informed about cooking issues was selected. Similarly, in an extended household (*gwa*), where several women participate in a cooking rotation, the most informed woman was selected. Appendix A.2 provides more details on Malians' cooking habits and *gwa* structure. For the rest of the paper, we refer to a *gwa* simply as a household. This sampling strategy ensures that within each cluster, treatment and control women live in relatively similar settings. The distance to control households was selected to avoid spillover effects from treated to non-treated areas

[8] Enumerators walked orthogonally to the assigned direction and selected 15 contiguous inhabited compounds on either side of the street and 15 in the opposite walking direction (i.e., including the 25 desired houses as well as five backup ones). If the desired number of households had not been reached by the end of the housing block, the team turned right and continued the counting process.

[9] In our sample, 62% of households have only one woman involved in cooking matters, while in 38% there are more than one. Anecdotal evidence and pilot data suggest that women tend to own and use their own cooking tools without sharing them with other women involved in a cooking rotation.

within clusters[10].

The nine-hundred women in the treatment arm received an invitation to a training session to be held in a nearby school on a Saturday in ten days' time[11]. The invitation flyer, delivered by hand, contained a preview of the topic to be discussed (energy efficiency and ICSs); the contact details of our field supervisor; and the date, time, and address of the session[12]. Participants were also informed that they would be reimbursed 1,000 CFA (i.e. the usual taxi fare for a return trip within a neighborhood) of transport costs.[13] One day before the session, all invited women received a reminder call. Each session was specifically organized to gather women only from the same cluster. Training sessions were conducted by a professional product promoter[14] and were held either in the morning or in the afternoon for about three hours. For the session, women gathered in one room, and hence also had the opportunity to interact socially while the promoter conducted the demonstration.

During the sessions, we provided information on the importance of hygiene while cooking, health consequences of indoor air pollution, efficiency gains and economic advantages (e.g., fuel saving, reduced health care needs) of using ICSs, and information on how to use and maintain them correctly and on their market price. The promoter set up a cooking demonstration where the same traditional dish was prepared using a traditional cookstove and an ICS to demonstrate the charcoal savings. After sharing the meal, women were invited one by one, in random order, to another room where an enumerator proposed the purchase of an ICS at the market price of 3,500 CFA. Women could decide whether to buy an ICS immediately, buy it in five days (the next Thursday) by leaving a deposit of 500 CFA, or

[10]The average distance between a treated and a control individual is about 600 meters (minimum of 200 and maximum of 1,200 meters), while the mean distance among treated individuals is about 90 meters.

[11]Saturday was chosen to maximize the presence of women, based on a dedicated question asked during the pilot phase.

[12]The flier contained the following text "You are kindly invited to participate in a training session on improved cookstoves and their benefits. This is organized by a team of economics researchers from international universities. *Date, time and venue details.* This invitation is personal and cannot be given to any other person. Each participant will receive 1,000 CFA to cover transport fees. If you have any questions, please contact *details of the manager.*"

[13]We do not have data on the actual usage of the money provided.

[14]We employed two promoters with past experience of conducting ICS marketing events with the NGO GERES.

not buy one. The second option was introduced to relieve cash constraints at the time of purchase. In other words, women without sufficient cash at the time of the session had the opportunity to purchase during the home visit by our staff five days later[15]. Women then left the session by a separate entrance to eliminate the possibility of influencing the women still waiting[16]. Still, it is plausible that participants could have waited for each other outside and returned home together. Five days after the training session, all women who did not buy on the spot (including both those who left a deposit and those who did not want to buy) were visited by our staff and again offered the chance to purchase an ICS at the market price of 3,500 CFA.

2.2.2 Peer information experiment

A randomly selected 50% of the women were individually invited to purchase an ICS (or leave a deposit for a purchase in five days' time) without further information. The other half were provided with information about a randomly selected peer's purchase decision[17]. For the peer info treatment group, the content of the information included the peer's identity and her purchase decision. A woman received either positive information (i.e., the peer purchased a stove during the session—this includes only a full purchase on Saturday and not leaving a deposit) or negative information (i.e., the peer did not purchase an ICS in the session). These purchase decisions were made in a separate area to provide privacy. We also ensured that women leaving the training venue after their purchase decision could not be seen by the other women still sitting in the session space to prevent them from influencing other women and thus preserve the experimental setting. The experiment involved 353 women in 32 of 36 sessions[18].

[15] In the case of non-purchase five days after, the deposit was lost.

[16] While waiting for their turn in the main room, women were not prevented from interacting.

[17] The randomization protocol was incorporated into software that we designed for data collection and treatment administration on tablets. In this way, half of participating women were randomly assigned to the treatment group ("peer info treatment group") and the other half to the control group ("peer info control group").

[18] Appendix D provides more details about the attrition in the various stages of the experiment and their consequences on the results.

During the training sessions, we also collected data on the social links among attendants. Each woman was asked whether she knew, at least by sight, each of the other attendants. She was also asked to name the women attending the session whose opinion she respected. The interviewer would take note separately of the names mentioned in reply to these two questions; combining the answers allowed us to identify three types of relationships: unknown, known by sight (but without respecting opinion), and opinion respected (which necessarily implies knowledge by sight).

2.2.3 Data collection

At the time of the invitation to the training session, all women are administered a 40-minute baseline survey in the local language. It included questions on the demographic composition of the household, socioeconomic status, education, income, working conditions, time allocation, savings, sources of energy for different purposes, household expenditure on energy, available appliances and cooking stoves (type and fuel used), knowledge about ICSs, and participation in informal groups.

In June 2015 (the endline, which was six to nine months after the baseline), we administered a questionnaire similar to the baseline survey. Figure 2 shows the timeline of our study.

A representative sample of ICSs sold to participants during our intervention were equipped with SUMSs that recorded temperatures over time and hence allowed us to measure usage[19]. This makes such monitoring feasible and reliable across a large number of households, while mitigating the risk of the Hawthorne effect, which could arise if measurements were made through frequent household visits. The SUMSs we used, iButtons™, are small sensors, the size of a coin, which can be easily attached to the stove. These have previously been used in studies of cookstove efficiency (Ruiz-Mercado et al., 2011; Beyene et al., 2015). The SUMSs

[19] Women willing to buy an ICS were informed that the product could be endowed with such a monitoring system. We have no anecdotal evidence suggesting that these SUMSs influenced the purchase decision. Enumerators did not explicitly mention its presence. The presence of the SUMS was obvious for all concerned in the first collection of monitoring data.

were attached to the cookstoves at the time of sale (between October 2014 and January 2015) and each recorded the temperature every 47 minutes over approximately four months. Our staff made a reading of these temperatures halfway through and at the end of this period (see Appendix E.2 for more details).

3 Data and summary statistics

The study sample includes 1077 individuals, 898 of which were invited to the training session, while 179 were not. About 46% of the women invited to the session attended, with an average of 11 women per session. In the 32 training sessions in which the informational treatment was implemented, 164 women received the peer information treatment, while 189 did not[20]. We were able to successfully track 989 individuals at the endline (839 in the treated and 150 in the control sample), for an overall attrition rate of 8.1%. Appendix D analyzes and discusses the implications of differential attrition for our analysis.

3.1 Baseline characteristics

In our baseline sample, study participants are on average 33 years old and 88% of them live with their husband. The average size of a household is about 13 members. More than 40% of respondents had no schooling, 15% attended primary school, 11% secondary school, and 30% beyond secondary school. Over 43% of women have some income-generating activity (mostly in the informal sector), dedicating on average five to six hours per week to it and earning a personal average monthly income of around 16,500 (USD 22–28) and 20,000 CFA (USD 27–34) for non-invited and invited women, respectively[21]. We compute a wealth index using principal component analysis, as suggested by Filmer and Pritchett (2001), by aggregating

[20] The main reason for the imbalance in the number of women in the two groups is that the non-treated group is larger by one unit in sessions in which the number of participants is odd.

[21] The averages presented for working time and income are for the full sample, not conditional on having work. The average monthly income for the head of the household is about 60,000 CFA (USD 81–102). The purchase of an ICS at the market price of 3,500 CFA would thus represent 17.5% and 5.6% of the monthly income of the women and head of the household, respectively.

the information on all assets into a single synthetic index. About 30% of women use either a formal (bank or MFI account) or informal (rotating credit and saving associations) saving device. More than half of women in the sample are members of informal groups such as *roscas*, discussion groups, or neighborhood groups.

We find that more than 80% of households rely on charcoal as their main fuel for cooking and the remaining 19% use wood, while fewer than 1% use gas. All women interviewed, except three, had previously cooked with charcoal and could thus easily use an ICS if given the opportunity. At the baseline, 97% of women declared owning at least one traditional cookstove (on average more than three), 19.7% own an ICS (among those, the average is 1.4),[22] and 50% own at least one small gas stove typically used for quickly heating water for baths or heating up leftovers[23]. Overall, we find about four stoves per household. During the dry season, about two-thirds of the sample mainly cook outside, while 42% do so during the rainy season (July to September).

The ICS used in our intervention is known by the majority of women surveyed (91–94%). More than 75% correctly attribute to ICSs characteristics related to efficiency and fuel saving. The main source of knowledge about the product comes from members of women's social network who own one. This was mentioned by 61% of women, followed by the market (56%) and promotional campaigns in the media (34%). We asked women to list some of the positive and negative characteristics of ICSs. The majority of respondents mentioned features related to efficiency and fuel savings (77%), while others focused on quality and durability (37%) and health (16%). The most prominent negative aspects are the lack of durability (54%) and high price (16%). The main reasons for not owning an ICS are related to the difficulty finding one (39%) and its high price (31%). However, the average estimated price of an ICS reported by women was 4,700 CFA, above the actual market price of 3,500 CFA.

[22]Given the large variety of traditional models available in the market and lack of clear definitions and certifications of "improved" models, the most common models for each category were shown to respondents through pictures, which are shown in Figure 1.

[23]In most cases, gas stoves are just gas cylinders with a nozzle on which people place a cooking pot. Only 5% of the sample have proper gas stoves.

Panel A of Table B.1 in Appendix B shows the baseline characteristics by invitation status, compliance (conditional on invitation), and differences across subsamples. The samples of invited and non-invited also appear balanced across most of the observable baseline characteristics. Participation in the session is the outcome of a self-selection process. Participants are, on average, significantly older, living in larger households, less educated, and less wealthy than those who do not attend.

Panel A of Table B.2 in Appendix B shows the baseline characteristics for the treated and control households in the peer information experiment, showing that the most observable characteristics appear similar across the two groups.

Appendix B provides more details on the construction of the variables.

3.2 Outcomes

Panel B of Table B.1 in Appendix B reports the short- and long-term outcomes of the training session. The difference in ICS ownership between invited and non-invited women is not statistically different at the baseline in terms of both the share of women owning ICSs (20.3% and 17.3%, respectively) and the average number of ICSs owned (0.32 in both groups). About 17% and 14% of the women invited to the session eventually purchased an ICS on Saturday and Thursday, respectively. This sums to a 31% overall purchase rate for our intervention. At the endline, the share of households owning an ICS increases by 26 percentage points (significant at the 1% level) over the control sample. The number of ICSs per household increases to 0.6 in the treated group at the endline, a 100% increase.

A set of variables relating to ICS knowledge is then displayed. We asked women whether they know the product, its main features, where it can be purchased, and its potential efficiency benefits. Appendix B shows more details on the construction of the variables. We also asked respondents if they know other people owning ICSs and classified them as "family members or friends" and "neighbors". The following variables are used to test the potential welfare impacts of an ICS: monthly fuel expenditure at the household level, whether women have an income-generating activity, the number of hours spent working in a typical week, and individual monthly income.

As far as ICS usage is concerned, we combine self-reported information with monitoring data on usage. For the latter, we obtain high-frequency data on the usage of 75 ICSs in 17 clusters. Appendix E presents an attrition analysis and shows that this sample is representative of women who purchased an ICS at our sessions. We construct measures of frequency and length of usage[24]. Panel A of Table E.2 reports the descriptive statistics the usage variables from the SUMSs. We find that about 73% (55 of 75) of women use the ICS at least once. An ICS is used, on average, for 35% of the days covered by our monitoring. If ever used, an ICS is used on average for 267 minutes per day (more than four hours) and during more than two cooking events, which last about 90 minutes each. Panel B of Table E.2 reports the descriptive statistics based on the self-reported measures of usage. About three-quarters of women owning an ICS at the endline report using it daily (49% for every meal, 27% at least once a day), about 10% use it one to four times a week, 9.5% use it rarely, and 5% never. Appendix E.3 shows that self-reported measures are good predictors of actual usage, as monitored by the SUMSs. We generate out-of-sample predictions of effective usage. The bottom of Table B.1 reports the descriptive statistics of both self-reported and predicted ICS usage at the endline for the different samples.

The outcome of the peer information treatment is a dummy variable that takes the value of one if the woman purchased an ICS on the spot. Panel B of Table B.2 reports that about 38% and 36% of women in the peer information and control samples, respectively purchased an ICS at the end of the Saturday session. The difference is not statistically significant.

[24] Section E.2 provides the details.

4 Results

4.1 Direct and indirect effects of the training session

We investigate the extent to which our training session directly and indirectly influences women's ICS ownership and usage. For the direct effects, we examine the intention to treat (ITT) effect of the invitation to the training session and the local average treatment effect (LATE) of participating in the session on the whole study sample. To measure the indirect or spillover effects in clustered randomized trials, the literature suggests comparing the outcomes of non-treated individuals in treated clusters with those of non-treated individuals in non-treated clusters (see Baird et al., 2017). This is not possible in our design, as we do not have non-treated clusters. Instead, we consider various samples: 1) women who did not participate in the training session (non-participants) and non-invited women and 2) women who participated in the training session (participants) but did not buy an ICS and non-invited women. We are aware that the participant and non-participant groups may be formed as an outcome of a self-selection process and discuss the consequences of this on our results below. We run the following reduced-form estimation on our various samples:

$$Y_i = \beta_0 + \beta_1 Invited_i + \gamma X_i + \epsilon_i \tag{1}$$

where β_1 provides the ITT of our intervention on ICS ownership and usage. In all the specifications, we cluster standard errors at the level of the 36 sampling points[25].

4.1.1 ICS ownership

Columns 1 and 5 of Table 1 refer to the whole non-attrited sample of the study and show that being invited to the training session increases the likelihood of owning an ICS by 31

[25] The results are unaffected if we consider invited and non-invited areas separately (i.e., use 72 clusters). A formal likelihood ratio test of the multi-level model vs. simple linear regression never rejects the null that the between-subject variation is zero. All the results presented below are also robust to the use of wild-bootstrapped clustered standard errors (Cameron et al., 2008). The results are not reported but are available on request.

percentage points (this represents a 160% increase on the baseline value) and the number of ICSs owned by 0.5 units (156% increase). Columns 2 and 6 show the LATE of participating in the session. This is obtained using instrumental variables, where participation is instrumented by invitation to the session[26]. We find that participating in the training session increases the likelihood of owning an ICS by 67 percentage points and the number of ICSs owned by about 1 unit for the population of participants. These represent 290% and 320% increases over the baseline values, respectively. The results are compelling compared with other interventions aimed at encouraging technology adoption[27]. Appendix F presents the cost-effectiveness calculations based on the estimated impacts.

In columns 3 and 7, we repeat the exercise for the sample of women who were invited but did not participate and the non-invited who form the control sample. We find that non-participants are about 9 percentage points more likely to own an ICS and own 0.3 more ICSs than control women at the endline.

These results provide suggestive evidence that the spillover effects from our treatment are underestimated. Although non-participants self-selected not to attend, they do not differ from control women for most of the baseline characteristics, as reported in column 1 of Table G.1 in Appendix G. We find no evidence that the group of non-participants could have higher demand for ICSs and hence would be negatively selected. They do not own more ICSs or know more about them than participants at the baseline. Moreover, when asked about which item a woman would prioritize to buy among an established list of kitchen tools (fridge, gas stove, pots, and ICS), no significant difference in the share of women preferring ICSs arises at the baseline between participants and non-participants. Similarly, when asked to rank health problems including malaria, respiratory diseases due to exposure to indoor

[26]In the first step, not reported, invitation strongly predicts participation in the session, with Cragg–Donald Wald F-statistics that exceed 95 for all the specifications.

[27]In the context of ICSs, Levine et al. (2018) find that a free trial period significantly raised ICS take-up from 4% to 29% in Kampala, while Beltramo et al. (2015b) show that marketing messages conveying the benefits of an ICS did not affect willingness to pay for it. More generally, interventions based on information dissemination do not unambiguously lead to higher technology adoption (Meredith et al., 2013; Ashraf et al., 2013; Bonan et al., 2016, 2017).

air pollution, gastrointestinal disease, and flu, 31% of women named respiratory diseases due to indoor air pollution as the most pressing problem in both groups, the difference between them being non-significant. Columns 4 and 8 of Table 1 show that women who participated in the training sessions but did not buy an ICS (neither on the spot nor five days later) are still more likely to own an ICS and own more ICSs at the endline than women in control areas. This finding suggests that the indirect effect of the training session lasted beyond the time window of our intervention.

4.1.2 ICS usage

Table 2 reports the effects of the invitation to the training session on self-reported usage (Panel A) and predicted actual usage (Panel B). Appendix E.3 explains how these predicted values are computed. The results found on ownership are confirmed for most of the usage outcomes. We find that invited women are 25 percentage points more likely to use an ICS every day than control ones (column 1, Panel A). This represents a 148% increase over control households. The effect doubles for women who participated in the training session (column 2, Panel A). Non-participants and non-buying participants are 8 and 17 percentage points respectively more likely to use an ICS every day than control women (columns 3 and 4, Panel A), which represent 61% and 130% increases over control households, respectively. The results show the same pattern when we consider the frequency usage score in columns 5 to 8 of Panel A. As far as predicted actual usage is concerned, we find that the share of days of ICS usage increases by 12 percentage points (200% increase) as the effect of being invited to the training session (column 1, Panel B) and by 25 percentage points for participants over control households (column 2, Panel B). Non-participants and non-buying participants use ICSs 2 and 8 percentage points more frequently than control women, respectively. However, only the latter is statistically significant (columns 3 and 4, Panel B). Positive and strongly significant effects are also found for the average daily time of usage. Invited and participating women use ICSs for 32 and 69 more minutes per day than control ones, respectively (columns

5 and 6, Panel B). These represent 246% and 530% increases over control households. Non-participants and non-buying participants use ICSs for 9 and 21 more minutes per day than control households, respectively (columns 7 and 8, Panel B)[28].

We must be cautious here, as the results presented in the current and previous subsections have some limitations. First, we have different attrition rates for the groups of invited and non-invited women. We address this by checking the sensitivity of our results to different assumptions on the distribution of treatment effects among attriters, following Karlan and Valdivia (2011), and by estimating Lee bounds (Lee, 2009). Appendix D presents and discusses the results. We find that while the overall effect of the training session on the whole sample is robust to different missing data scenarios, the results for the samples of both "non-participants and controls" and "non-buying participants and controls" do not remain significant, as the assumptions on attriters become more severe for the outcomes related to ICS ownership. The results on usage appear more robust to severe assumptions on attrition than those on ownership. Second, the group of non-participants received an invitation to the training session, while the control households did not. This may have generated some effect on purchase. However, our questionnaire, administered to control and treated women equally, largely inquired about households' cooking habits and included detailed questions on ICSs. Cookstoves were mentioned as the focus in the introduction of our survey and about 30% of the survey questions were related to topics including cooking, kitchens, stoves, and fuel. Hence, the impact of receiving an invitation to the training session via our flier at the end of the survey was negligible compared with the effect of the questionnaire, if any. Appendix G presents and discusses some robustness checks of the results presented in the current and previous subsections.

[28] We use alternative estimation models for the results in Table 2. We use the probit model for columns 1–4 of Panel A, ordered probit for columns 5–8 of Panel A, and Tobit model for columns 1–8 of Panel B. The results are qualitatively similar, as reported in Table G.2 in Appendix G.

4.1.3 Impacts on welfare

We test the impact of our intervention on a set of variables expected to be influenced by the adoption and use of ICSs: fuel expenditure, income-generating activities, time allocation, and monthly income. The first dimension is related to the fuel efficiency of ICSs, while the intuition behind the remaining ones is that a more efficient cookstove would affect the amount time spent on meal preparation. As an initial step, we estimate the reduced form, namely the impact of being invited to the session on the differences in outcomes between the endline and baseline values. The results reported in Table 3 are based on the entire study population. Receiving an invitation to the training session does not seem to have any significant impact on overall monthly fuel expenditure at the household level[29], individual propensity to have an income-generating activity, time spent on income-generating activities, or monthly income[30].

Several reasons can explain these findings. First, despite the positive results of our intervention in increasing ICS ownership and usage documented in Section 4.1, ICSs continue to be scarce in the overall set of stoves used by households. Our data show that the number of ICSs as a share of the number of stoves (of any type) in the household grows from about 7% at the baseline (not statistically different between the treatment and control households) to about 12% at the endline in the treatment group (the difference being significant with a p-value below 0.01). This finding can be explained by the fact that meals are prepared for large families and often require the use of several cookstoves at the same time. Indeed, only 13% of women use it exclusively, while 71% of women use it with other traditional stoves. As a further check of this interpretation, we estimate the impact of ICS ownership after the training session as well as ICS usage and ownership at the endline on welfare outcomes.

[29] Miller and Mobarak (2014) find that fuel savings from the use of an ICS with characteristics similar to ours are not perceived as significant. We do not have data on effective and perceived fuel savings by stove type, only an aggregate measure for all the technologies used within the household.

[30] For some of the variables, the sample size is reduced because of missing data. We do not find any systematic pattern in the missing observations related to our treatment allocation. For the continuous outcome in columns 3 and 4, the results remain unchanged after using standard trimming or winsorization to control for outliers.

We use the invitation to the training session as the instrumental variable for these variables. Column 1 of Table G.3 in Appendix G reports the first-stage results (the Cragg–Donald Wald tests always reject the hypothesis that the instrument is weak). The results confirm the null effects reported in Table 3. Moreover, ICS ownership may not necessarily imply exclusive or continuous usage. One reason may lie in the fact that women in large households only use their own ICSs during their turn in the cooking rotation. We explore the heterogeneous effects of "cooking alone" vs. "cooking rotation" (i.e., involving multiple women) on the set of usage outcomes described in Section 4.1.2 and find that ICSs are significantly more frequently used in the former than in the latter. However, no significant differential effect arises for welfare outcomes along this dimension[31].

Such a behavioral gap in ICS adoption has also been underlined in other works (Lewis and Pattanayak, 2012; Hanna et al., 2016). In our context, among ICS owners at the endline, 44% declared that they use an ICS for every meal, 23% use one daily, and 15% one to four times per week. Bensch and Peters (2013) find similar self-reported usage in urban Senegal for an ICS type comparable to the one investigated in this study. To have a sizable impact, besides being regularly used and well maintained, ICSs should completely replace traditional stoves. Indeed, energy transition is often carried out through energy stacking (i.e., when both modern and traditional fuels and cookstoves are used simultaneously) (Ruiz-Mercado et al., 2011; Masera et al., 2000).

One can argue that the non-significant coefficients shown in Table 3 may be due to a lack of statistical power. However, the power calculation suggests that our design (i.e., considering the invitation to the training session as the treatment) is powered to detect the standardized effect size in the range of 0.17 and 0.24 (for significance levels ranging from 0.05 to 0.1 and power between 0.7 and 0.8)[32]. These are commonly considered small effects. For example, in the case of fuel expenditure, our design would allow us to detect at least a 15%

[31] The results are not shown but are available on request.

[32] The exercise takes into consideration partial compliance, attrition, and explanatory power from the baseline covariates.

reduction, about half the minimum efficiency gains measured by GERES through laboratory tests.

4.2 Peer information and ICS purchase

We evaluate the effect of receiving information on the purchase decision of a peer within the same session by estimating the following equation using the full sample of participants:

$$Y_i = \beta_0 + \beta_1 RI_i \times (1 - PB_i) + \beta_2 RI_i \times PB_i + \gamma X_i + \epsilon_i \tag{2}$$

where Y_i is the outcome of woman i, which takes the value of one if she buys on Saturday and zero otherwise (including both not buying and leaving a deposit for a potential Thursday purchase). RI_i is a dummy equal to one if the woman received the information treatment and zero otherwise. PB_i represents the content of the information provided and is expressed as a dummy. It takes one as value if the peer assigned to i bought an ICS at the session on Saturday, and zero otherwise. β_1 (β_2) captures the impact of receiving information on the peer when such information is negative (positive), i.e. no take-up (take-up), compared to receiving no information. X_i is a vector of individual baseline characteristics[33]. Results are presented in columns 1 and 2 of Table 4.

We then assess the extent to which knowing (at least by sight) the matched peer influences individual take-up decisions by extending our model as follows:

$$\begin{aligned} Y_i = \beta_0 + \beta_1 RI_i \times (1 - PB_i) \times Unknown_i + \beta_2 RI_i \times PB_i \times Unknown_i + \\ \beta_3 RI_i \times (1 - PB_i) \times Known_i + \beta_4 RI_i \times PB_i \times Known_i + \gamma X_i + \epsilon_i \end{aligned} \tag{3}$$

The two new coefficients show the respective marginal impacts with respect to receiving no information. These are linked to being in one of the mutually exclusive categories determined

[33] These include age, marital status, household size, dummies for education levels, participation in informal groups, having an income-generating activity, any savings, an index for wealth, knowing about and already owning an ICS, normalized distance from the drop-off point, the number of people known and the number of people whose opinion is respected present at the session.

by the combination of whether or not the peer purchased, i.e. PB_i vs $(1-PB_i)$, and whether or not she is known at least by sight by woamn i, i.e. $Known_i$ vs $Unknown_i$. $Known_i$ takes value one if for the matched peer either one of the two variables "known by sight" or "whose opinion is respected" takes value one. Results are presented in columns 3 and 4 of Table 4.

Finally, we assess the differential impact of the intensity of the linkage with a matched peer. We do this by separating the impact of peers only "known by sight" from those "whose opinion is respected":

$$\begin{aligned} Y_i = \beta_0 + \beta_1 RI_i \times (1-PB_i) \times Unknown_i + \beta_2 RI_i \times PB_i \times Unknown_i + \\ \beta_3 RI_i \times (1-PB_i) \times Sight_i + \beta_4 RI_i \times PB_i \times Sight_i + \\ \beta_5 RI_i \times (1-PB_i) \times Respect_i + \beta_6 RI_i \times PB_i \times Respect_i + \gamma X_i + \epsilon_i \end{aligned} \tag{4}$$

The dummies $Unknown_i$, $Sight_i$ and $Respect_i$ are mutually exclusive and take the value of one if i is related to her assigned peer according to the various intensity levels and zero otherwise. All coefficients show the respective marginal impacts with respect to receiving no information. Results are presented in columns 5 and 6 of Table 4. The analysis is conducted on the sample participants in the training session using OLS with robust standard errors[34].

There could be a dimension of endogeneity due to assortativity with respect to unobservables. For instance, women with modern attitudes (or more likely to be receptive to new technologies) may be simultaneously more socially connected and more likely to purchase an ICS. This can be erroneously attributed to peer information[35]. Clusters/sessions may also have specific features that affect both participation and take-up such that, for example, in a session in which most participants are interested in ICSs, those receiving the peer-info treat-

[34] The results are similar when using probit or logit models.

[35] Table B.2 shows that the average number of women in a session whose opinion a participant respects is balanced across peer-info treatment and peer-info control women. In that respect, both groups appear to have similar peer connections within a session. Furthermore, this variable, together with the number of women known by sight, are included as individual controls in the regressions. Table B.2 also shows that women receiving and not receiving the peer information treatment appear similar in terms of their modern attitudes: both show non-significantly different means of suitable proxies, namely ICS ownership at the baseline and knowing about ICSs.

ment would be more likely to receive positive peer information. This raises an endogeneity concern because of omitted variables, as we could not distinguish session characteristics from the effect of receiving the peer information treatment.

We address this problem in two ways: by adding session controls (odd columns) and by including session fixed effects (even columns). In column 1, we include these controls: 1) the share of women in the peer-info control group who purchased an ICS at the session; 2) the number of participants; 3) a measure of connectivity in a session[36]; 4) the share of ICS owners at the baseline, and 5) the share of those knowing about the existence of ICSs[37]. In column 3, besides the five session controls included in column 1, we add the share of women in the peer-info control arm who are known (either by sight or respected) by at least one woman in the session and the share of known women (either by sight or respected) in the peer-info control group who eventually purchased an ICS in the session. In column 5, besides the five session controls included in column 1, we add the shares of women in the peer-info control arm who are known by sight and who are respected by at least one woman in the session and the shares of women known by sight and respected in the peer-info control group who eventually purchased an ICS in the session.

Compared to receiving no information, being informed about a peer who purchased at the session (measured by β_2 in columns 1 and 2) leads to a negative and barely insignificant effects (p-values of 0.11 and 0.12, respectively). Similarly, we find no impact from receiving negative information about a peer's behaviour (did not purchase) as β_1 is not significantly different from zero.

In columns 3 and 4, where we compare peers who are unknown and known (i.e. known by sight or whose opinion is respected)[38], we find that women who received the information

[36] Connectivity is measured as the density (i.e., the number of existing links over the number of possible links, $\frac{2L}{N^2-N}$, in a network of N subjects with L reported relations) of the social network of knowledge. In our case, a link is considered as present when at least one of the two parties reports knowing the other.

[37] Excluding this set of five session controls does not affect the result in column 1. These results are not shown but are available on request.

[38] The analysis is based on the comparisons across five mutually exclusive groups with the following sample sizes: no info (N=189), info on an unknown peer who did not buy (N=38), info on an unknown peer who bought (N=30), info on a known peer who did not buy (N=64), info on a known peer who bought (N=32).

that an unknown peer bought (β_2) tend to buy significantly less than women who received no information. Women who received information that an unknown peer did not buy (β_1) and those who received information that a known peer bought (β_4) did not buy differently from those who received no information. Within the treatment group, women who received information that a known peer bought did not buy more than those who received information that a known peer did not buy (β_4-β_3). However, women who received information that a known peer bought did buy significantly more than those who received information that an unknown peer bought (β_4-β_2). The relevant p-values for these tests are shown at the bottom of Table 4.

In columns 5 and 6 we look at the heterogeneous effects across all possible levels of relationships between peers: we compare between peers who are unknown, known by sight and whose opinion is respected[39]. Our results are in line with those from columns 3 and 4. They show that women who received information that an unknown peer bought (β_2) tend to buy significantly less than women who received no information. Women who received information that an unknown peer did not buy (β_1) and those who received information that a peer known by sight or whose opinion is respected bought (respectively β_4 and β_6) did not buy differently from those who received no information.

Within the treatment group, women who received information that a peer known by sight (or whose opinion is respected) bought did not buy more than women who received information that a peer known by sight (or whose opinion is respected) did not buy (respectively β_4-β_3 and β_6-β_5). However, women who received information that either a peer known by sight or whose opinion is respected bought did buy significantly more than those who received information that an unknown peer bought (respectively β_4-β_2 and β_6-β_2). Finally, women who received information that a peer whose opinion is respected bought did not buy

[39] The analysis is based on the comparisons across seven mutually exclusive groups with the following sample sizes: no info (N=189), info on an unknown peer who did not buy (N=38), info on an unknown peer who bought (N=30), info on a peer known by sight who did not buy (N=31), info on a peer known by sight who bought (N=16), info on a respected peer who did not buy (N=33), info on a respected peer who bought (N=16).

differently than those who received information that a peer known by sight bought (β_6-β_4).

Overall, our results show that treated women, when compared to the control group, negatively react (less likely to buy an ICS) to information that an unknown peer bought. Conditional on being treated, they positively react to information on peers who purchased when there exists some level of acquaintance (either known by sight or whose opinion is respected). However, take-up does not increase significantly as the relationship with the peer becomes more intense. Miller and Mobarak (2014) find that conveying information on opinion leaders' adoption (rejection) of non-traditional stoves in Bangladesh leads to higher (lower) take-up by residents in the same village. However, their result highlights the asymmetric importance of negative information which has a stronger and more robust impact than the positive one. Our findings corroborate the importance of the positive information and highlight that, when the signal comes from unknown peers, the reaction may go in the opposite direction. Given our experimental design, the results in Table 4 focus on the decision to purchase on Saturday only. In this way, we can assess the short-term impact of our informational treatment within a controlled setting. Exercises aiming at measuring the impact of the experimental treatment on outcomes measured outside the session would likely violate the Stable Unit Treatment Value Assumption.

4.3 Possible mechanisms

Few recent studies have tried to disentangle imitation effects from social learning[40]. Our research setting does not allow us to clearly identify the mechanism responsible for the interaction effects in ICS diffusion. However, the peculiarities of the ICS, our context, and the additional evidence presented below lead us to think that imitation is likely to explain the effects we observe.

[40] Bursztyn et al. (2014) provide socially connected pairs (friends or family members) information about a peer's (i) intention to purchase a new financial asset and actual capability of owning it (which is randomized) or (ii) intention to purchase only. They find that both social learning and imitation effects are economically significant. Bernard and Torero (2015) find evidence of peer effects in the decision to connect to electricity in rural Ethiopia and, by excluding the social learning channel, conclude that imitation effects offer the most reasonable explanation.

The first set of supporting evidence relates to the impacts on knowledge. Table 5 shows the ITT effects of the invitation to the training session on four variables related to knowledge about ICSs and on a score obtained from the sum of them (Appendix B provides more details). The regressions include the whole sample of both invited and non-invited women (control sample). For the first two columns, both the baseline and the endline measures are available. We thus estimate the change over time. The outcomes in columns 3 to 5 instead are based on measures taken at the endline only. We do not find any significant effect on the knowledge of ICSs in column 1. Similarly, we find no significant impact on the dummy indicating whether an individual agrees that an ICS is a more efficient technology than traditional cookstoves (column 2), nor for whether women provided the correct estimate of the potential fuel savings of using an ICS compared with a traditional cookstove (column 4). Women do not seem to improve their knowledge of where an ICS can be bought by being invited to the training session (column 3). Indeed, the lack of significant effect persists when we consider the aggregate index of knowledge (column 5). Overall, our invitation does not seem to significantly raise knowledge on ICSs, as measured through our survey questions. We attribute these results to the fact that ICSs appear to be widely known (93.6% know about them at the baseline) and thus only a small information gap likely existed in the first place. Furthermore, ICS design and usage are similar to the traditional charcoal stove, which is widely used. Adopting ICS technology does not require significant behavioral changes, adjustment in cooking techniques, or important informational gaps to be filled. Of the 1078 women involved in our study, only three had no previous experience of cooking with a traditional charcoal stove. In that respect, ICSs are similar to rubber shoes (Meredith et al., 2013) and different from index insurance, menstrual cups, and contraceptives, where social learning is a major driver of adoption (Cai et al., 2015; Oster and Thornton, 2012; Munshi and Myaux, 2006). An ICS is also a relatively cheap and risk-free technology, which implies little investment or risk[41]. This and the fact that ICSs were already well known before our

[41] This is different from adopting new seeds or agricultural practices that can entail risks and changes to fundamental sources of livelihood and require social learning (Conley and Udry, 2010). Other studies

intervention indicate that ICS adoption is unlikely to benefit from social learning. However, we acknowledge that the measures used may not fully capture the dimension of knowledge of ICSs and all its attributes; for example, we lack price expectations at the endline. Hence, the social learning about the attributes cannot be ruled out completely.

This makes us lean toward imitation effects. Given the setup of our "peer information treatment", the results in Section 4.2 are suggestive of this channel. We show that, conditional on being treated, participants are more likely to purchase when there exist a minimum relational link with the matched women. Conversely, without such minimum relationship, treated women react negatively when matched to an unknown peer who bought. Within the context of our study, one's purchase decision was made without other participants' knowledge and without discussing the purchase with any participant, thereby preventing any social learning effect.

Non-participants can experience such imitation effects through similar peer information naturally spreading within neighborhoods after the actual experiment was over. Suggestive evidence of such increased interaction is offered by the results on the increased number of known people owning ICSs at the endline following the training session. Results are reported in Table G.6. In particular, the share of women knowing someone owning an ICS at endline significantly increases by 18 percentage points as an effect of being invited to the session (Column 1). Such direct effect is likely to be the mechanical outcome of our training session. However, we also find positive, but non-significant, effects among non-participants. In columns 3-4 and 5-6 we classify people as members of the family vs friends or neighbours. We find that the result, for the whole sample, seems to be driven by the number of people owning ICS among neighbours (column 5), rather than among friends and relatives (column 3). We also find that non-participants are significantly more likely to know neighbours owning ICS than control women. These results suggest that non-participants gain

have shown that peer effects present a channel for social learning about the benefits of technologies such as deworming pills, anti-malaria bed nets, and a piped water connection (Kremer and Miguel, 2007; Dupas, 2014; Conley and Udry, 2010; Maertens, 2017).

and exchange information on ICS ownership over the longer run.

The desire to imitate ICS owners, although isolated from one another, may spread among non-participants over time and this effect could become significant over the months following our training sessions at the sample level. Given that non-participants live in proximity to participants, imitation effects over time are likely. More generally, two reasons may drive an individual to mimic peers' behavior. First, individuals may think others' behavior reflects private and valuable information they do not have. This would lead them to imitate regardless of the private information or preferences (Banerjee, 1992; Bikhchandani et al., 1992). Second, individuals may interpret others' decisions as a social norm to which they should conform (Munshi and Myaux, 2006). This may be due to taste for social status, fear of sanctions, social identity, or reference-dependent consumption preferences (Bernheim, 1994; Akerlof, 1980; Benjamin et al., 2010; Abel, 1990; Luttmer, 2005; Fafchamps and Shilpi, 2008; Bursztyn et al., 2017). Finally, an additional and non-related mechanism could be at play in our context: intra-household bargaining. However, we argue lengthily in Appendix A.2 that this is unlikely to be the case.

5 Conclusion

Our study investigates, in a context characterized by energy poverty, the role of peer effects in the adoption of a fuel-saving cooking technology. We find suggestive evidence that individuals are potentially influenced by information received on the purchase decision of another peer. Following our training session, which increased markedly product ownership and usage six to nine months afterward, we find evidence that the technology naturally spread locally among people who did not participate in our intervention. We interpret this as suggestive evidence of peer effects. Additional results on product knowledge and various welfare variables suggest that such an interaction occurs more in the form of imitation than social learning. Depending on the characteristics of the technology, different policies can be implemented to speed up the process of adoption. In our case, and with other technologies that show similar characteristics (e.g., ease of use, low cost, and similarity to already widespread products), the focus should be on direct market penetration rather than informational campaigns.

We extrapolate from our results that once geographical penetration has occurred, a sufficient number of women, through peer effects, can help diffuse the technology by imitation. Furthermore, to generate significant welfare impacts on a population coping with energy poverty, interventions should consider the local context carefully in two main ways: (i) designing ICS models to make them fit local tastes and (ii) considering cooking habits. The latter relates particularly to the practice of energy stacking, which requires greater efforts to achieve a successful energy transition.

Figures and Tables

Figure 1: Different models of cookstoves used

(a) Traditional three stone stove (using wood)

(b) Traditional metal stove (using wood)

(c) Traditional metal stove (using charcoal)

(d) Improved cookstove (using charcoal)

Figure 2: Timeline of the study

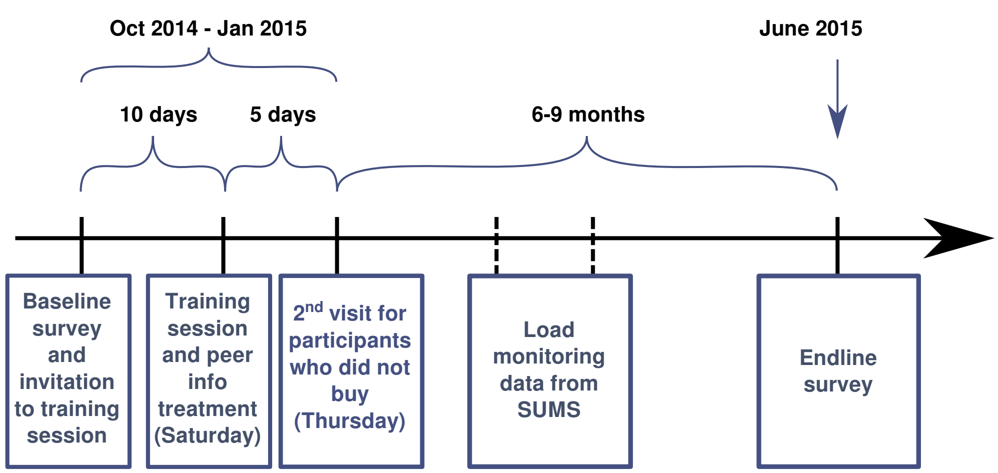

Table 1: Direct and spillover effects of the training session on ICS ownership at the endline

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ICS ownership				N. of ICS owned			
	All		Non-participants & control	Non-buying participants & control	All		Non-participants & control	Non-buying participants & control
Invited	0.311***		0.0886*	0.208***	0.477***		0.286**	0.351***
	(0.0449)		(0.0447)	(0.0590)	(0.0961)		(0.112)	(0.106)
Participated		0.670***				1.026***		
		(0.0888)				(0.195)		
Observations	989	989	587	277	989	989	587	277
Control Mean	0.186				0.31			

Note: All outcomes are measured at the endline. Individual controls include age, marital status, household size, number of women in the cooking rotation, dummies for education levels, participation to informal groups, having an income generating activity, use of saving device, an index for wealth, knowledge about ICS, owning an ICS at baseline, normalized distance from the drop-off point. Specifications in columns 2 and 6 are obtained via IV, the remaining via OLS. Invitation to the session is used as instrumental variable for participation. The Cragg-Donald Wald F statistic for weak instrument in the first stage is 97.88. The sample "All" is formed by all non-attriter women (both invited and non-invited to the training session). "Non-participants & control" includes invited women who did not attend the training and non-invited ones. "Non-buying participants & control" includes attending women who did not buy and non-invited ones. Standard errors, in parenthesis, are clustered by 36 sampling points, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2: Direct and spillover effects of the training session on ICS usage at the endline

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	High frequency usage (every day)				Frequency usage score (0-5)			
Panel A: Self-reported usage	All		Non-participants & control	Non-buying participants & control	All		Non-participants & control	Non-buying participants & control
Invited	0.251***		0.0760**	0.175***	1.390***		0.377**	0.949***
	(0.0444)		(0.0372)	(0.0471)	(0.218)		(0.185)	(0.235)
Participated		0.534***				2.962***		
		(0.0792)				(0.388)		
Observations	953	953	563	268	953	953	563	268
Control Mean			0.127				0.693	

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Share of days of usage				Avg daily usage time			
Panel B: Predicted actual usage	All		Non-participants & control	Non-buying participants & control	All		Non-participants & control	Non-buying participants & control
Invited	0.116***		0.0249	0.0833***	32.47***		9.113**	21.40***
	(0.0187)		(0.0159)	(0.0250)	(5.029)		(4.297)	(6.255)
Participated		0.248***				69.17***		
		(0.0344)				(9.199)		
Observations	953	953	563	268	953	953	563	268
Control Mean			0.152				13.04	

Note: All models include individual controls: age, marital status, household size, number of women in the cooking rotation, dummies for education levels, participation to informal groups, having an income generating activity, use of saving device, an index for wealth, knowledge about ICS, owning an ICS at baseline, normalized distance from the drop-off point. The sample is restricted to individuals with non-missing self-reported usage. Invitation to the session is used as instrumental variable for participation. The Cragg-Donald Wald F statistic for weak instrument in the first stage is 96.89. Standard errors, in parenthesis, are clustered by 36 sampling points, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3: Welfare impacts, ITT estimates

	(1) Δ_tMonthly fuel expenditure	(2) Δ_tHave income generating activity	(3) Δ_tWeekly time working	(4) Δ_tIndividual monthly income
Invited	-3,268	0.00366	-2.733	-7,760
	(2,839)	(0.130)	(2.448)	(8,314)
Observations	971	989	987	823
Control Mean	11,724	0.493	4.28	14,599

Note: Outcomes in columns 1 and 4 are expressed in FCFA. The last row reports the means of outcome levels at the endline in the control group. All models include individual controls: age, marital status, household size, number of women in the cooking rotation, dummies for education levels, participation to informal groups, having an income generating activity, use of saving device, an index for wealth, knowledge about ICS, owning an ICS at baseline, normalized distance from the drop-off point. Standard errors, in parenthesis, are clustered by 36 sampling points, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: Effects of information received on peer's purchase on ICS take-up

	(1)	(2)	(3)	(4)	(5)	(6)
			Purchases at the session			
$\beta_1 : RI \times PB = 0$	0.046	0.038				
	(0.056)	(0.055)				
$\beta_2 : RI \times PB = 1$	-0.080	-0.083				
	(0.069)	(0.070)				
$\beta_1 : RI \times PB = 0 \times Unknown$			0.084	0.088		
			(0.084)	(0.083)		
$\beta_2 : RI \times PB = 1 \times Unknown$			-0.218**	-0.222**		
			(0.092)	(0.095)		
$\beta_3 : RI \times PB = 0 \times Known$			0.027	0.012		
			(0.068)	(0.069)		
$\beta_4 : RI \times PB = 1 \times Known$			0.047	0.048		
			(0.087)	(0.088)		
$\beta_1 : RI \times PB = 0 \times Unknown$					0.084	0.089
					(0.084)	(0.083)
$\beta_2 : RI \times PB = 1 \times Unknown$					-0.226**	-0.222**
					(0.093)	(0.096)
$\beta_3 : RI \times PB = 0 \times Sight$					0.048	0.024
					(0.095)	(0.096)
$\beta_4 : RI \times PB = 1 \times Sight$					0.056	0.050
					(0.127)	(0.130)
$\beta_5 : RI \times PB = 0 \times Respect$					0.007	0.001
					(0.092)	(0.098)
$\beta_6 : RI \times PB = 1 \times Respect$					0.046	0.047
					(0.114)	(0.116)
Session Controls	Yes	No	Yes	No	Yes	No
Cluster FE	No	Yes	No	Yes	No	Yes
$\beta_3 = \beta_4$			0.841	0.725	0.950	0.860
$\beta_2 = \beta_4$			0.026	0.027	0.065	0.084
$\beta_5 = \beta_6$					0.781	0.750
$\beta_4 = \beta_6$					0.948	0.989
$\beta_2 = \beta_6$					0.049	0.057

Note: The table reports estimations of models 2, 3 and 4 in columns 1-2, 3-4 and 5-6, respectively. RI: "Received Information on peer's purchase"; PB: "Peer bought ICS at the session". All models include individual controls: age, marital status, household size, n. of women in the cooking rotation, dummies for education levels, participation to informal groups, having an income generating activity, use of saving device, an index for wealth, knowledge about ICS, owning an ICS at baseline, normalized distance from the drop-off point, n. of women known and n. of women whose opinion is respected in the session. Models in odd columns include session controls: n. of participants, avg share of ICS ownership and knowledge about ICS, session connectivity measure, share of women in the peer-info control group who purchased. Session controls in column 3: share of women in the peer-info control arm who are known and share of known women in the peer-info control group who purchased an ICS. Session controls in column 5: shares of women in the peer-info control arm who are known by sight and who are respected, shares of women known by sight and respected in the peer-info control group who purchased ICS in the session. The mean outcome in the peer information control group is 0.365. The sample is formed by women who participated to the training session and who were successfully involved in the final experimental phase (N=353). Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 5: Effects of invitation on knowledge of ICS

	(1) Δ_t Know ICS	(2) Δ_t ICS allows to save fuel	(3) Know where to buy ICS	(4) Correct estimate of fuel saving (20-40%)	(5) ICS knowledge score (0-4)
Invited	0.0504	0.0808	-0.0219	0.0568	0.164
	(0.0803)	(0.122)	(0.0912)	(0.108)	(0.186)
Observations	989	989	989	989	989
Control Mean	0.893	0.72	0.727	0.213	2.55

Note: The outcomes in columns 1 and 2 are calculated as difference between endline and baseline values. Outcomes in columns 3 to 5 are only measured at the endline. The last row reports the means of outcome levels at the endline in the control group. All models include individual controls: age, marital status, household size, number of women in the cooking rotation, dummies for education levels, participation to informal groups, having an income generating activity, use of saving device, an index for wealth, knowledge about ICS, owning an ICS at baseline, normalized distance from the drop-off point. Mean dependent variable for columns 1-2 and 3-5 is the unconditional mean for the control sample at the baseline and endline, respectively. The analysis is performed on the whole non-attrited sample. Standard errors, in parenthesis, are clustered by 36 sampling points, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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Appendix

A Product and context

A.1 Improved cookstoves

We identified the ICS, locally known as "Fourneau Seiwa", in collaboration with the French NGO "Groupe Energies Renouvelables, Environnement et Solidarités" (GERES). GERES has supported and supervised the value chain of the product and certified its advantages, in terms of fuel efficiency, with laboratory tests. Such tests have been conducted by an external institution, the "Centre National de l'Energie Solaire et des Energies Renouvables" in January 2014, following international standards. The ICS underwent a boiling and cooking test and its performance was compared to the traditional charcoal cookstove. The results indicate that the thermal performance of ICS was 26.19% against 18.05% of the traditional cookstove. This allows the ICS to gain a potential charcoal saving of 30% to 45% and save 0.62 tCO2e/year, which have been certified by UNFCCC and Gold Standard within GERES's plan of activities.

A.2 Malian households' cooking habits and expenditures

Cooking is carried out exclusively by women and is one of the activities which is usually organized at the level of the extended household (*gwa*) in order to exploit economies of scale. Meals are prepared for all members. Women often participate in a cooking rotation where every day (or week) a different woman has to prepare for the whole household in turn. In our sample, on average about two women are involved in a cooking rotation (62% rotations have only one woman, 20% two, 9% three and 9% four or more). This shows that cooking rotations with more than one woman do not represent the vast majority of our sample (38% of households). During our pilot phase we collected anecdotal evidence from the field which strongly and clearly indicate that the vast majority of women participating in cooking rotations, where there are at least two women involved, own and use their own cooking tools, not sharing them with the other women involved in the rotation. Furthermore, the control variable "number of women in the cooking rotation" is not significant in tables 4, 1 and 2, where we present our main results. It thus has no significant impact on ownership and usage of ICS. These coefficients are not shown but are available upon request. With this body of evidence, we are inclined to conclude that free-riding at the household level or other intra-household bargaining issues are unlikely, or play a marginal role.

We also need to look into issues that might be linked to intra-household bargaining. A household (*gwa*) is composed of various nuclear families in our context. The decision to own and use an ICS may also be impacted by intra-nuclear family factors, specifically the interactions and bargaining between the wives of a husband, and not between a husband and his wife or wives. Traditionally, and according to the anecdotal evidence we collected, women have a large degree of autonomy on decisions related to food and cooking. In our overall sample, only 20% of the women we have surveyed live in a household where the husband has two or more wives, therefore these intra-nuclear household issues only concern a minority of cases. We look at the addition of two controls: a dummy "husband is polygamous" and a dummy which takes value one if the women surveyed is the second or higher ranked wife (the first wife usually commands greater respect within a nuclear family than the others). Both of these proxies for intra-nuclear family bargaining/interactions between wives have no significant effects when included separately or when included as an interaction variable (with "peer bought") in specifications 1 to 3 of table 4. Again, with this evidence, we are inclined to conclude that cases of free-riding at the intra-household level and bargaining issues, are unlikely to play a significant role.

As a typical feature of Malian society, household expenditures are rigidly divided across members. Heads are mostly in charge of paying for food, while fuel expenditure are assigned to women in about 70% of cases. However, all women are endowed with a certain monthly budget for the provision of goods for the household. They are in charge of shopping food and fuel at the market. This endowment complements the individual female earnings from productive activities, if any. This explains the relatively high levels of monthly fuel expenditure at household level which we observe (about 13,000 CFA), compared to respondents' income. All women of the cooking rotation are expected to contribute to fuel expenditures of the household.

B Variables construction and sample balance

For the construction of education categories, we considered that, in Mali, primary school is intended for children aged 7 to 12 and is called Enseignement Fondamental Premier cycle; what we denote as secondary school is the Enseignement Fondamental Second cycle for children aged 13-15; "beyond secondary school" includes mainly those who attended the Lycée (for pupils aged 16-18) and a small share who attended university.

The wealth index, which uses the first principal component of the set of variables introduced, assigns a larger weight to assets that vary the most across households, and can take positive as well as negative values. The categorical variables expressing house facilities are transformed into ordinal and treated as continuous, as suggested by Vyas and Kumaranayake

(2006). The items considered in the index are: type of floor, type of roof, toilet facilities, drinking water facilities, number of sleeping rooms in the dwelling, ownership of fridge, camera, TV, sofa, table and chairs, bike, motorbike, car, sewing machine, wood or iron bed, air conditioning, fan.

Regarding the various knowledge questions: first, women were shown a picture of the product and asked if they knew what it was. If a woman could identify the ICS, we coded the variable "Know ICS" as equal to one. If a woman could identify the stove, we asked a follow-up open ended question: "What are the main features of an ICS?" We coded their answers accordingly: a dummy variable "ICS allows to save fuel" took value one when a woman answered along the line that it "allows to save fuel/money" or that they "are more efficient as they retain the heat"[42]. All these questions were asked both at the baseline and at the endline. Two additional knowledge questions were asked at the endline. First, women were asked whether they knew where they could get an ICS. The variable "know where they can buy ICS" is equal to zero if women answered "no" (taking value one otherwise). A large majority reported that such products could be found in most markets in Bamako, which was actually the case. Second, the following hypothetical question was administered: "If you consume ten packs of charcoal per month with a traditional cookstove, how many packs are you expected to consume with an ICS used for the same time and same quantity of food?". The variable "Correct estimate of fuel saving (20-40%)" takes the value of 1 if the estimated saving is in a reasonably correct range (that is between 20 and 40%) and 0 otherwise. An index for ICS knowledge is constructed summing four dummy variables. Variables take the value of zero in cases of non-response[43].

[42]The other popular options were "expensive", "more long lasting", "good material/ design", "produce less smoke", "work well", "do not work well", "they alter the taste of food", "never used", "do not know".

[43]While this may represent an arbitrary simplification, in our context, the vast majority of women (about 93%) were knowledgeable about the product (i.e. the variable "know ICS").

Table B.1: Summary statistics and sample balance related to the invitation and participation to the training session

	Invited	Non invited	P-value of diff	Participant	Non Participants	P-value of diff
N. of observations	898	179		415	483	
Participant to training session	0.462					
Endline survey not administered	0.066	0.162	0.000	0.031	0.095	0.000
Panel A: Baseline characteristics						
Respondent age	33.2	32.2	0.298	34.3	32.3	0.011
Live in couple	0.873	0.894	0.431	0.860	0.884	0.280
Size of HH	12.836	13.073	0.721	13.458	12.302	0.042
N. of women in cooking rotation	1.818	1.771	0.687	1.827	1.812	0.866
No schooling	0.438	0.408	0.453	0.496	0.387	0.001
Primary school	0.147	0.151	0.877	0.149	0.145	0.834
Secondary school	0.109	0.128	0.445	0.104	0.114	0.611
Beyond Secondary school	0.306	0.313	0.844	0.251	0.354	0.001
Have income generating activity	0.455	0.436	0.616	0.448	0.462	0.672
Weekly time working (hours)	6.373	5.056	0.108	5.853	6.822	0.157
Respondent monthly income (CFA)	19753	16538	0.258	17922	21323	0.154
Wealth index	0.014	-0.068	0.632	-0.255	0.245	0.001
HH monthly fuel expenditure (CFA)	13414	13574	0.851	13347	13471	0.853
Use saving device	0.323	0.274	0.192	0.316	0.329	0.653
Member of informal groups	0.546	0.536	0.803	0.542	0.549	0.829
ICS in the HH	0.203	0.173	0.359	0.195	0.209	0.593
N. of ICS in the HH	0.323	0.318	0.926	0.301	0.342	0.427
N. of stoves in the HH	4.371	4.425	0.811	4.284	4.445	0.409
Panel B: Outcomes						
Purchase ICS at the session (Sat)	0.175			0.378		
Left deposit at the session (Sat)	0.113			0.282		
Purchase ICS at Thurs visit	0.141			0.306		
Purchase ICS after intervention (Sat+Thur)	0.314			0.680		
Own ICS at the endline[a]	0.447	0.187	0.000	0.662	0.249	0.000
N. ICS owned at the endline[a]	0.611	0.307	0.000	0.818	0.421	0.000
Know ICS	0.941	0.916	0.209	0.949	0.934	0.315
ICS allows to save fuel	0.786	0.760	0.426	0.831	0.747	0.002
Know where they can buy ICS[a]	0.751	0.727	0.519	0.731	0.769	0.205
Correct estimate of fuel saving(20-40%)[a]	0.241	0.213	0.458	0.284	0.201	0.005
ICS knowledge score (0-4)[a]	2.741	2.553	0.017	2.913	2.584	0.000
Know people owning ICS: all[a]	0.535	0.373	0.000	0.682	0.400	0.000
Know people owning ICS: family or friends[a]	0.293	0.307	0.725	0.366	0.227	0.000
Know people owning ICS: neigbours[a]	0.448	0.147	0.000	0.612	0.297	0.000
Reported high frequency usage (every day)[a]	0.309	0.127	0.000	0.468	0.162	0.000
Frequency usage score (0-5)[a]	1.789	0.693	0.000	2.734	0.920	0.000
Share of days of usage (predicted)[a]	0.155	0.057	0.000	0.234	0.081	0.000
Avg daily usage time (predicted)[a]	39.624	13.039	0.000	59.229	21.332	0.000

Note: Values reported refer to the whole sample (36 clusters). [a]Variables measured only at the endline on the non-attrited sample (N=989).

Table B.2: Summary statistics and sample balance related to the treatment giving info on a peer's ICS ownership or decision to purchase

	Info	No info	P-value of diff
N. of observations	164	189	
Panel A: Baseline characteristics			
Respondent age	35.354	34.709	0.604
Live in couple	0.872	0.868	0.889
HH size	12.896	14.116	0.187
N. of women in cooking rotation	1.799	1.899	0.505
No schooling	0.457	0.497	0.445
Primary school	0.165	0.153	0.759
Secondary school	0.091	0.111	0.533
Beyond Secondary school	0.287	0.238	0.296
Have income generating activity	0.470	0.434	0.493
Weekly time working (hours)	6.500	5.254	0.202
Repondent monthly income (CFA)	23718	13875	0.008
Wealth index	-0.299	-0.297	0.972
HH monthly fuel expenditure (CFA)	12419	14184	0.178
Use saving device	0.341	0.317	0.621
Member of informal groups	0.579	0.540	0.447
ICS in the HH	0.177	0.180	0.922
N. of ICS in the HH	0.262	0.296	0.623
N. of stoves in the HH	4.421	4.217	0.494
Know ICS	0.951	0.958	0.757
ICS allows to save fuel	0.829	0.857	0.463
Distance from drop-off point	0.076	0.074	0.620
N. of women known in the session	6.323	6.127	0.726
N. of women whose opinion is respected in the session	3.402	3.783	0.386
Peer bought ICS at the session	0.378		
Peer's opinion respected	0.299		
Panel B: Outcomes			
Purchase ICS at the session (Sat)	0.384	0.365	0.699

Note: The sample is based on 32 sessions where the experiment was implemented and 353 attendants who received our treatment giving info on a peer's decision to purchase or own. Of those, 340 were successfully tracked at the endline.

C Sampling design and survey protocols

C.1 Sampling clusters

The first step in the sampling design is to subdivide each of the six communes of Bamako into rectangular blocks covering the entire area of the city. We use Google Maps to delimit each of the six communes and then overlay rectangles within each of them which we call cells (later referred to collectively as *"grid"*). Non-residential areas such as industrial zones, parks, rivers, ponds, sports areas etc. are excluded from this coverage. In the course of overlaying this grid, we ensure that the cells cover actual blocks of houses and are uniform in size.

Within each commune, each cell is then assigned a number, and a random number generator is used to select a sub-sample. The number of starting points selected (or clusters) for each commune is proportional to the population of each commune according to the 2009 census of Mali. Therefore, we select 6 clusters in commune 1, 5 clusters in commune 2, 4 clusters in commune 3, 9 clusters in commune 4, and finally 7 clusters in communes 5 and 6.

Wealthy neighborhoods are excluded from the sampling, and whenever a randomly selected cluster is deemed too wealthy to be relevant for the study of energy poverty, a replacement cluster is selected within the same commune. Such a procedure leads to a sample which is not fully representative of the entire population of Bamako. However, selected clusters are representative of the population of interest for our study, i.e., non-wealthy families using cookstoves.

Our procedure to select the geographic coordinates of a cluster follows the second-best routine recommended in the Afrobarometer survey manual[44]. That is, in the absence of the list of households within the cluster, we use the map of the cell to determine the starting point, by identifying it with its GPS coordinates. First, a ruler is overlaid over each side of the chosen cluster. Afterwards, a random number generator provides a digit for each of the two dimensions. The intersection of the two lines drawn at those digits is the sampling starting point of our cluster.

The day before the survey, our team of supervisors use first Google Earth and then a GPS device to determine the starting point on the field. They then take pictures and note landmark points for the subsequent deployment of the survey teams. When a designated point does not correspond to a residential area, the team then moves to the nearest housing block. In addition, to anticipate the possibility that the designated starting point or its vicinity may not be suitable for the survey, our supervisors have a back-up starting point.

[44] Afrobarometer Round 6 Survey Manual, https://www.afrobarometer.org/sites/default/files/survey_manuals/ab_r6_survey_manual_en.pdf

C.2 Selection of households

The supervisors then proceed with the selection of households which will be assigned to enumerators the next day. The direction from which to start the selection is chosen by turning away from the closest line of the grid (border of the rectangular cluster) on the map, and looking right from that position. We choose this method to ensure that in all neighborhoods, the selected households fall within the starting point's cell. Since the starting point is chosen at random and in some cases is at the edge of the cell, randomly choosing a direction could in practice lead to the selection of households from another cell, and possibly already selected. In particular, this method also ensures that households which are part of the control sample fall within the same cell (as described in the next paragraph).

Once the initial direction is chosen, we select 15 contiguous, inhabited compounds, on either side of the street, and 15 in the opposite walking direction. Each is assigned an alpha-numeric ID. In each walking direction, if the desired number of households is not reached by the end of the housing block, the team always turns right and continues its counting process. Once these 30 households are selected for our treatment sample (5 are registered as backups), we proceed to select those for the control sample. From the initial starting point, again facing away from the closest line in the grid, our team is required to walk straight for 10 minutes. In case of obstacles preventing this, the team alternated between turning right and left. The position at the end of the ten minutes walk is the starting point for the selection of 10 new households which are part of the control sample. The selection of the households (5 in each walking direction) happens with the same rules as for the treatment sample: 5 are selected for the interviews, the remaining 5 are registered as backups. Once again, an alpha-numeric numbering system is used for these contiguous households.

In general, the protocol for the selection of drop-off points satisfies the primary requirement of being entirely non-discretionary (once the random starting point is selected, the entire set of both treated and control households follows deterministically, with the only exception represented by selected households where no occupant is found). It also satisfies the secondary requirement of having the treated and the control sample come from comparable areas of Bamako, while at the same time avoiding the problem of spillover effects across samples[45]. As an added benefit, treated and control points in each clusters are visited in the same week, hence controlling for any time-specific phenomena which might affect specific parts of the city.

[45] If control points had been selected in a random fashion independently from treated points, they could happen to be very close to treated points, making the problem of spillovers real. Vice-versa, if they had been selected by just restricting to clusters not containing treated points, the risk of systematic differences between treated and control points would have been maximized.

C.3 Baseline survey protocol

Each selected household is identified by its GPS coordinates. The enumerator entering a house, after introducing herself and shortly describing the aim of the project, asks to talk to the woman responsible for the cooking rotation (the woman who is most knowledgeable about the family's meal decisions). She asks for her consent and proceeds with the survey. For the households in the treatment sample, an invitation to attend a training session on the use and advantages of ICS is then handed out. The sessions are held in a venue in the neighborhood and women are told that they will receive 1,000 CFA to cover their transportation fees if they show up. For the control sample, no invitation is given to the interviewees.

If the targeted individual is not at home, the enumerator inquires about an approximate time when she will be home, and returns then for the interview. The enumerator can also request the phone number of that individual and ask her an appointment. After two unsuccessful attempts to contact the selected individual within the household, a replacement procedure kicks in. The household is then replaced by a backup household (see the selection procedure outlined above).

C.4 Endline protocol

The endline survey protocol is performed during two consecutive days. During the first, we use GPS coordinates of households along with personal identification information (name, address, phone number) collected during the baseline to locate the women who were surveyed at the baseline. Once the identification of households is completed in a given starting point and the women are identified, we notify them of the visit of enumerators in the next day. This process is completed for both control and treatment samples. When a targeted woman is likely to be absent for a long period, we use a replacement procedure and interview the oldest woman within the same household who is knowledgeable about the cooking rotation. Following this, our team of enumerators administer the follow-up questionnaire.

D Dealing with attrition

The study is characterized by different degrees of data completeness which influence our different samples of analysis. In what follows, all steps leading to the different samples considered in the analysis are presented, together with a discussion on their impact on internal and external validity.

We expected an overall sample of 1080 and a control sample of 180 women. However, we discarded three observations as respondents did not complete the questionnaire or refused to answer to a majority of questions. This led to a final sample of 1077 individuals, 898 assigned to the training session, and 179 to the control sample.

We find significant differential attrition rates in our invitation treatment sub-samples: 16% of women not invited to the training session and 6.5% of those invited were not reached at the endline (the difference is significant at 1% level in a univariate test[46]). This seems to be the outcome of small sample size and relatively high attrition in few control clusters[47]. The protocol for households and women identification, using baseline information, has been followed uniformly throughout the administration of the endline questionnaire. The most common reasons for attrition were related to the temporary or permanent displacement of women, together with a few cases of deaths. According to column 1 of table D.1, attriters and non-attriters appear as balanced samples along almost all observable characteristics.

In four out of thirty six training sessions, our field team faced technical problems with the software for data collection and treatment administration. Thus, these sessions are not included in the analysis of peer information in the present section, but they are in the rest of our analysis. These sessions were concentrated on a few successive dates and in a particular geographic area (Commune 5). Such loss of data does not represent a threat to the internal validity of the peer information experiment, because these sessions are not included in the sample for the relevant estimations. Column 2 of table D.1 shows that participants to the training sessions where the peer information treatment was implemented are on average older, more educated and less likely to own ICS at the baseline. However, it turns out that none of these characteristics systematically correlates with the outcome variable reported in table 4 (results not shown but available upon request). This said, we cannot exclude that this may affect the external validity of the results.

Finally, 14 women (3.9%) who attended the training session were not involved in its final

[46] Results from multivariate analysis in column 1 of table D.1 with clustered standard errors lead to a coefficient of 0.078, significant at 10% level.

[47] We verified that by excluding the five sampling points (out of 36) where the highest attrition in control cluster was experienced, we would not reject the null hypothesis of no differential attrition (results available on request).

phase, when the peer information treatment was administered. This was mainly due to two reasons. First, some women only partially attended the training session and left the venue in advance. Second, some women arrived late and could not be registered for the final phase. Column 3 of table D.1 shows that these women were slightly older and more knowledgeable about ICS. They are excluded from the analysis of the peer information treatment but are included in the rest of the analysis.

To take into account the extent to which differential attrition has an impact on the internal validity of results in tables 1 and 2, we run sensitivity analysis to different data missing scenarios. Following Karlan and Valdivia (2011), we create two scenarios where control attriters are imputed the non-attriter control households mean plus 0.25 or 0.5 standard deviations of the observed distribution for controls; for the treatment group, we impute a low outcome, the non-attrited treatment group mean minus 0.25 or 0.5 standard deviations of the observed treatment distribution. We also implement Lee bounds (Lee, 2009), where bounds are estimated by trimming a share of the sample, either from above or from below. We report ITT estimates for model estimated in tables 1 and 2 for the different sub-samples of interest in table D.2. All the effects estimated on the whole sample (columns 1 and 4) are robust in all scenarios. The results for the sample of non-participants and controls (columns 2 and 5) are robust for 0.25 standard deviations for ownership and predicted usage, while they do not remain significant for 0.5 standard deviations. Also, the Lee lower bound turns non-significant. The results for the sample of non-buying participants and control (columns 3 and 6) are robust to the imputation exercise, both with 0.25 and 0.5 SD imputations, but the Lee lower bound is not statistically significant[48].

[48] One can notice that in some cases the point estimate is not included within the Lee Bounds. This is due to the fact that the Lee bound exercise is performed without individual controls. Point estimates in specifications without controls are always lower than the ones shown in tables 1 and 2, and are always included in in the Lee bounds ranges.

Table D.1: Attrition analysis

	(1) Attriter [whole sample]	(2) Participant 32 sessions [Participants 36 sessions]	(3) Participant reaches final phase [Participants 32 sessions]
Invited	-0.0787*		
	(0.0414)		
Respondent age	-1.65e-05	0.00298**	0.00192**
	(0.000800)	(0.00124)	(0.000759)
Live in couple	-0.00242	0.0144	0.0413
	(0.0227)	(0.0462)	(0.0374)
HH size	-0.00291*	-0.000246	-0.00190
	(0.00148)	(0.00266)	(0.00178)
N. of women in cooking rotation	0.00665	0.0200*	0.00497
	(0.00772)	(0.0118)	(0.00947)
Primary school	-0.0157	0.0939**	0.0123
	(0.0245)	(0.0403)	(0.0291)
Secondary school	0.00273	0.0149	0.0199
	(0.0251)	(0.0607)	(0.0355)
Beyond Secondary school	-0.0229	0.0973**	0.0167
	(0.0239)	(0.0401)	(0.0258)
Have income generating activity	-0.0167	-0.00644	-0.0163
	(0.0198)	(0.0389)	(0.0289)
Wealth index	0.00219	-0.00543	-0.00500
	(0.00500)	(0.00801)	(0.00604)
Use saving device	-0.0214	0.0568	-0.00880
	(0.0183)	(0.0423)	(0.0292)
Member of informal groups	-0.0157	-0.00304	0.0336
	(0.0228)	(0.0363)	(0.0301)
Know ICS	0.00592	-0.0808	0.154*
	(0.0308)	(0.0557)	(0.0907)
ICS in the HH	0.0228	-0.0991**	0.00738
	(0.0253)	(0.0497)	(0.0268)
Distance from drop-off point	0.0162	0.0449	0.00118
	(0.0121)	(0.0349)	(0.0133)
Constant	0.181***	0.742***	0.710***
	(0.0590)	(0.0908)	(0.113)
Observations	1,077	415	367
Mean Dependent Variable	.081	.884	.962

Note: Standard errors, in parentheses, are clustered by 36 sampling points in column 1, while are robust in columns 2 and 3. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The header for each column is the probability of one observation being part of a sample, and the line below (between brackets) represents the overall sample used for the estimation.

Table D.2: Impact of the training session, sensitivity to attrition

	(1)	(2)	(3)	(4)	(5)	(6)
	ICS ownership			N. of ICS owned		
Panel A: Ownership	All	Non-participants & control	Non-buying participants & control	All	Non-participants & control	Non-buying participants & control
Mean ± 0.25 SD	0.278***	0.0737*	0.173***	0.390***	0.201**	0.254***
	(0.0390)	(0.0373)	(0.0498)	(0.0752)	(0.0840)	(0.0884)
Mean ± 0.5 SD	0.255***	0.0473	0.152***	0.340***	0.143	0.207**
	(0.0395)	(0.0373)	(0.0479)	(0.0781)	(0.0856)	(0.0898)
Lee lower bound	0.199***	0.00295	0.00581	0.0862	-0.110	-0.150
	(0.0428)	(0.0487)	(0.0638)	(0.0896)	(0.106)	(0.0938)
Lee upper bound	0.313***	0.0826**	0.145**	0.376***	0.148	0.151
	(0.0417)	(0.0402)	(0.0573)	(0.0891)	(0.0935)	(0.112)
Observations	1,077	662	312	1,077	662	312
	High frequency usage (every day)			Frequency usage score (0-5)		
Panel B: Self-reported usage	All	Non-participants & control	Non-buying participants & control	All	Non-participants & control	Non-buying participants & control
Mean ± 0.25 SD	0.243***	0.0780**	0.166***	1.360***	0.410**	0.915***
	(0.0369)	(0.0311)	(0.0490)	(0.186)	(0.157)	(0.232)
Mean ± 0.5 SD	0.235***	0.0678**	0.160***	1.323***	0.358**	0.888***
	(0.0370)	(0.0309)	(0.0486)	(0.187)	(0.156)	(0.230)
Lee lower bound	0.109**	-0.0284	-0.0426	0.768***	-0.0827	-0.108
	(0.0428)	(0.0479)	(0.0615)	(0.209)	(0.238)	(0.306)
Lee upper bound	0.227***	0.0525	0.1015**	1.360***	0.322*	0.613**
	(0.0358)	(0.0351)	(0.0509)	(0.184)	(0.178)	(0.259)
Observations	1,041	638	303	1,041	638	303
	Share of days of usage			Avg. daily usage time		
Panel C: Predicted actual usage	All	Non-participants & control	Non-buying participants & control	All	Non-participants & control	Non-buying participants & control
Mean ± 0.25 SD	0.114***	0.0293**	0.0813***	31.19***	9.703**	20.43***
	(0.0173)	(0.0136)	(0.0232)	(4.565)	(3.607)	(5.760)
Mean ± 0.5 SD	0.110***	0.0245*	0.0787***	30.30***	8.442**	19.76***
	(0.0175)	(0.0136)	(0.0231)	(4.598)	(3.609)	(5.726)
Lee lower bound	0.0577***	-0.00978	-0.0214	15.58***	-1.005	-5.707
	(0.0209)	(0.0236)	(0.0250)	(-5.503)	(6.272)	(6.193)
Lee upper bound	0.116***	0.0301*	0.0564**	31.24***	9.751**	13.18**
	(0.0167)	(0.0161)	(0.0240)	(-4.150)	(4.054)	(5.835)
Observations	1,041	638	303	1,041	638	303

Note: Each cell reports ITT estimates of model 1 on the three sub-samples reported in the headings. In lines 1 and 2, we impute missing dependent variable with mean + (-) 0.25 and 0.5 standard deviation for missing control (treatment) individuals, respectively, following Kling et al. (2007). Standard errors, in parentheses, are clustered by 36 sampling points, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. In the subsequent lines, we report Lee lower and upper bounds (Lee, 2009) and their respective estimated standard error. No covariates are employed.

E ICS usage

E.1 Sampling and attrition

We have monitoring data on usage for 17 out of 36 clusters. SUMS were randomly attached to 100 ICS, out of the 282 sold during the intervention, i.e. on about 36% of ICS that were sold at the training sessions on Saturday and also during our Thursday visits. We were able to successfully track 75 of them[49]. On average, we have data on the usage of 4 ICS per cluster (minimum of 1 and maximum of 10) which cover about 58% of ICS sold in those clusters (minimum of 25% and maximum of 100%).

In order to ascertain the representativeness of the actually monitored sample, we look at the determinants (along the observable baseline characteristics used throughout the analysis) of the probability of purchasing a ICS on which is installed a SUMS out of all ICS bought, on either Saturdays or Thursdays. This is done in column 1 of table E.1. One can notice that none of the characteristics, apart from the indicator for secondary school education, seems to significantly predict the dependent variable.

Out of 100 SUMS installed, we were able to successfully obtain data (from at least one wave)[50] for 75 of them (about 25% attrition rate). The main reasons for the attrition are breakage (15 cases), loss/inability to find the SUM (6 cases), inability to find the ICS sold (4 cases). Several reasons could justify the relatively high attrition rate we face. SUMS were installed on the bottom of the stove. A special tape designed to resist high temperatures was used to secure SUMS to the stove. In that, we followed the guidelines of our SUMs reseller (Berkley Air Monitoring Group) and the best practices from other studies. However, differently from many of those studies, the particular model of ICS we consider is portable and suitable for both indoor and outdoor cooking. As such, it is often moved from one place to another. This makes SUMS particularly vulnerable to blows and scratching, which may cause their damage or loss. Column 2 of table E.1 reports the determinants of owning an ICS with a SUMS from which data were collected, conditional of being in the sample of the 100 ICS on which a SUMS was installed. One may notice that the only significant predictor, out of around 15, is the size of the extended household (negatively).

We have about 9% missing observations (including both "do not know" answers and actual missing) in the question on self-reported usage of ICS at the endline. Column 3 of

[49] ICS are often carried around for either indoor or outdoor cooking. It appears that the 25 we could not track have been scratched away while being used. This happened despite us following the manufacturer's protocol while attaching them to the surface of our ICS.

[50] Because of attrition between the first and the second wave, we do not have data for each SUM from both waves. In total we have temperature measurements from 129 "missions", where any mission is composed by measurements from a given SUMS in a given wave.

table E.1 shows the probability of having a non-missing observation in the sample of women owning ICS at the endline. No particular pattern seems to arise: most importantly, we do not see any differential data missingness along the invitation to the session dimension.

E.2 Measurements of usage

We configured the SUMS so that they would take a measurement every 47 minutes, allowing us to have an homogeneous coverage over different times of the day, and allowing their memory, able to hold up to 2048 measurements, to record temperatures for 66 days. Near the end of this period we ran a monitoring pass, where data were collected from the devices, and a new recording of 66 days was initiated. Thus, we have two waves of temperature data for each SUMS. Different algorithms have been proposed in the literature to convert temperature measurements from SUMS into usage statistics: our approach draws from Simons et al. (2014), and was specifically calibrated for our measurement configuration through visual investigation of temperature profiles over time. We construct a set of variables capturing the share of days of usage and the average daily usage time.

Figure E.2 shows that the distribution of maximum temperatures measured in each mission is bimodal. A mission is composed of measurements from a given SUMS in a given wave; Figure E.1 shows the timing of our two waves. Following Simons et al. (2014) we define a distinct usage as a temperature peak such that:

1. temperature is over 50°C,
2. two distinct usages are separated by at least 141 minutes in time (2 other measurements),
3. between two distinct usages, there are at least a drop and a raise of 4°C each between subsequent measurements.

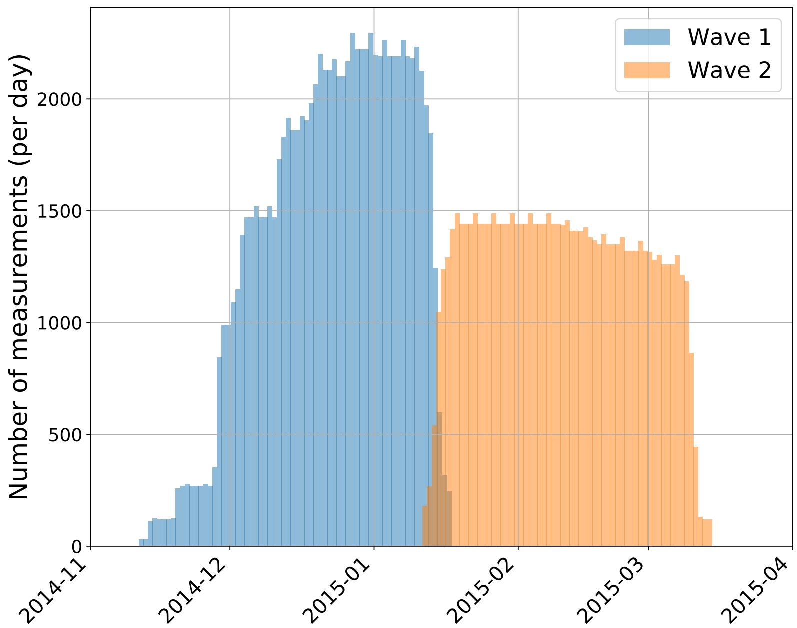
This allows us to count the number of days where at least one usage is made and thus to compute the share of days of usage over the number of days for which measurements were made. We also look at the number of measurements for which temperature is over 50°C. We use these to then compute an "average daily time of usage" measure in minutes. Table E.2 reports the descriptive statistics of monitored usage over the monitoring period.

Table E.1: Sampling and attrition on ICS usage data

	(1) Pr(ICS with installed SUM) [All ICS purchased Sat+Thurs]	(2) Pr(ICS with SUM data collected) [ICS with installed SUM]	(3) Pr(ICS usage reported) [ICS owned at the endline]
Invited at the training session			0.0956
			(0.0858)
Respondent age	5.10e-05	0.00339	-0.000815
	(0.00236)	(0.00379)	(0.00137)
Live in couple	-0.0625	0.0491	-0.0715**
	(0.0868)	(0.121)	(0.0272)
HH size	-0.00756	-0.0193***	0.000876
	(0.00491)	(0.00535)	(0.00209)
N. of women in cooking rotation	0.0520	0.0689	0.00108
	(0.0323)	(0.0418)	(0.00948)
Primary school	-0.0339	-0.155	0.0231
	(0.111)	(0.141)	(0.0381)
Secondary school	-0.196*	0.0952	-0.0372
	(0.0973)	(0.168)	(0.0469)
Beyond Secondary school	-0.0198	-0.0583	-0.0427
	(0.101)	(0.103)	(0.0386)
Have income generating activity	-0.0596	0.0668	0.0187
	(0.0600)	(0.113)	(0.0315)
Wealth index, all sample	-0.0115	-0.0277	0.0111
	(0.0229)	(0.0314)	(0.00976)
Use saving device	0.0764	-0.00317	0.0311
	(0.0731)	(0.0948)	(0.0358)
Member of informal groups	-0.0124	0.0759	0.0375
	(0.0668)	(0.126)	(0.0349)
Know ICS	-0.0466	-0.164	0.0544
	(0.151)	(0.214)	(0.0727)
ICS in the HH	0.0458	0.126	0.0377
	(0.0855)	(0.0966)	(0.0289)
Constant	0.500**	0.806***	0.806***
	(0.217)	(0.222)	(0.141)
Observations	275	100	403
Mean Dependent Variable	0.364	0.750	0.911

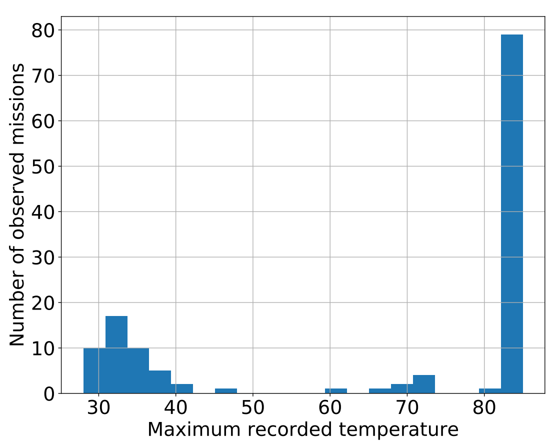
Note: Standard errors, in parentheses, are clustered by 36 sampling points, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Each column reports the sample used in square brackets.

Figure E.1: Timing of our two waves of temperature measurements



Note: Timing of daily measurements density for wave 1 and 2. A mission initialization denotes the beginning of up to 2048 measurements for a given SUMS.

Figure E.2: Peak and average temperatures



Note: maximum temperature reached during each mission.

E.3 Monitored vs self-reported usage

We construct a set of variables capturing both the frequency and length of ICS usage, whcih are reported in panel A of table E.2. Panel B of table E.2 reports the descriptive statistics based on self-reported measures of usage. We focus on the non-attrited sample of women who owned ICS at the endline with non-missing self-reporting information (N=367)[51].

We investigate the extent to which self-reported measures are good predictors of actual objective usage, as monitored through SUMs. Table E.3 shows the results of a set of regressions where the dependent variables are the share of days of usage and the average time of usage. We use the available measures of self-reported usage, namely the six dummies obtained from the questionnaire as regressors and the self-reported usage score, together with the usual controls used throughout the paper. We find that reported usage significantly predicts monitored usage throughout the models. We use the estimated coefficients of models 1 and 3, the ones with higher explanatory power, to make out-of-sample linear predictions of effective usage, for the whole population of women owning ICS at the baseline. We correct the predicted values as follows: negative shares and time are transformed into zero and we set the variables to be equal to zero when ICS is not owned at the endline. Descriptive statistics of the two variables are presented in table B.1.

[51] We have missing information on self-reported usage for 9% of women owning ICS at the endline. The analysis of missing data is done in appendix E.

Table E.2: ICS usage summary statistics

	N	mean	sd	min	max
Panel A: Monitored ICS usage					
Days of monitoring	75	71.97	29.15	12.63	112.2
N. of days with at least one usage	75	23.27	20.46	0	62
Share of days of usage, over monitoring period	75	0.354	0.291	0	0.970
At least one usage event	75	0.733	0.445	0	1
Avg time of usage, mins/day of usage above 50° C	55	263.7	114.9	107.8	698.2
N. of usage events per day of usage	55	2.779	0.817	1.048	5.016
Avg duration of usage event in day of usage, in mins	55	95.67	27.67	32.18	148.9
Panel B: Self-reported ICS usage					
Frequency of ICS use: always	367	0.488	0.501	0	1
Frequency of ICS use: daily	367	0.270	0.444	0	1
Frequency of ICS use: 3-4 times/week	367	0.0572	0.233	0	1
Frequency of ICS use: 1-2 times/week	367	0.0381	0.192	0	1
Frequency of ICS use: rarely	367	0.0954	0.294	0	1
Frequency of ICS use: never	367	0.0518	0.222	0	1
Non-missing self-reported ICS usage	403	0.911	0.285	0	1

Table E.3: Monitored vs self-reported usage

	(1)	(2)	(3)	(4)
	Share of days of usage		Avg daily usage time (mins)	
Frequency of ICS use:				
always	0.337***		67.46**	
	(0.0769)		(26.39)	
daily	0.486***		113.2***	
	(0.107)		(29.81)	
3-4 times/week	0.491***		118.7**	
	(0.133)		(48.55)	
1-2 times/week	0.174*		37.76	
	(0.0909)		(31.29)	
rarely	0.00968		-10.08	
	(0.0590)		(31.66)	
Frequency usage score (0-5)		0.348***		78.96***
		(0.0766)		(22.94)
	(0.196)	(0.140)	(43.61)	(40.69)
Observations	75	75	75	75
Mean Dependent Variable	0.354	0.355	90.20	90.21

Note: All models include individual controls: age, marital status, household size, number of women in the cooking rotation, dummies for education levels, participation to informal groups, having an income generating activity, use of saving device, an index for wealth, knowledge about ICS, owning an ICS at baseline, normalized distance from the drop-off point.Standard errors, in parenthesis, are clustered by 36 sampling points, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

F Cost-effectiveness of the training session

Our intervention consists of training sessions in different neighborhoods, normally two per day. We sold the ICS at 3,500 CFA, the same price of procurement and thus did not incur any profit or loss on the actual sale. We can disaggregate the costs of our intervention as follows: 1) the distribution of our invitations: women were personally given a flyer at their door; 2) if they attended each received 1,000 CFA for transport fees; 3) session costs including place rental and set-up, food and refreshment, enumerators and presenters' time; 4) costs of ICS home delivery on Thursdays (including transport costs and enumerators' time). Overall, the total cost per woman invited is estimated at 3,000 CFA (slightly less than USD 5). The invitation to the training session increases take-up by about 31 percentage points. The effect is more than double for those who participated in the session. This means that to increase the take-up by one ICS in our sample, it costs us on average about 9,000 CFA. This figure could certainly be reduced as an organization running similar interventions could minimize costs further, through more efficient bulk purchases (ICS, food, refreshment), by having larger sessions and by allowing participants to buy more than one ICS each.

G Further results and robustness checks

One can argue that the decision to adopt and use ICS may be different for women already owning an ICS at the baseline. As a robustness check, we repeat the exercise testing the direct and indirect effects of the training session on the sub-sample of women who did not own ICS at the baseline. Results, reported in table G.4 and G.5, are qualitatively similar. The ownership of ICS at the baseline did not seem to affect the decision to participate to the training session either. Another issue concerns the identity of the respondent. In 12.3% of cases, the respondent at the endline is not the same of the baseline. In most cases the new respondent is another woman of the cooking rotation (either a co-spouse, another woman of the same household or a daughter). This may lead to biased estimates if the new respondent has a different informational set compared to the original one – although it is unlikely that the new respondent is unaware of the presence of ICS at household level (our main outcome). We re-estimate the impact and spillover exercise on the sub-sample of observations where the respondent was the same and the results remain similar to the ones discussed above.

Table G.1: Sub-sample comparisons at the baseline

	(1) Non-participants & control	(2) Non-buying participants & control
Respondent age	0.000844	0.00311
	(0.00172)	(0.00271)
Live in couple	-0.0714	-0.202**
	(0.0633)	(0.0941)
HH size	-0.00642*	-0.00194
	(0.00352)	(0.00621)
N. of women in cooking rotation	0.0214	0.0314
	(0.0133)	(0.0298)
Primary school	0.00725	-0.100
	(0.0654)	(0.0788)
Secondary school	-0.00158	-0.0477
	(0.0773)	(0.103)
Beyond Secondary School	0.0331	0.0114
	(0.0454)	(0.0758)
Have income generating activity	0.00411	0.0565
	(0.0491)	(0.0773)
Wealth index	0.0117	-0.0470***
	(0.0127)	(0.0171)
Use saving device	0.0756*	0.0555
	(0.0425)	(0.0775)
Member of informal groups	-0.0411	-0.0969
	(0.0431)	(0.0635)
Know ICS	0.0931	0.344**
	(0.0995)	(0.145)
ICS allows save fuel	-0.0855	-0.0401
	(0.0658)	(0.0991)
ICS in the HH	0.0238	0.125
	(0.0434)	(0.0821)
First purchase priority is ICS	0.0712	0.141*
	(0.0483)	(0.0725)
Health priority: consequences of IAP	0.0539	0.0266
	(0.0483)	(0.0855)
Constant	0.745***	0.148
	(0.124)	(0.203)
Observations	587	277

Note: Standard errors, in parenthesis, are clustered by 36 sampling points, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table G.2: Direct and spillover effects of the training session on ICS usage at the endline, robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)
	High frequency usage (every day)			Frequency usage score (0-5)		
Panel A: Self-reported usage	All	Non-compliers & control	Non-buying compliers & control	All	Non-compliers & control	Non-buying compliers & control
Invited	1.045***	0.442*	1.034***	1.099***	0.394*	0.981***
	(0.210)	(0.242)	(0.285)	(0.195)	(0.228)	(0.252)
Observations	953	563	268	953	563	268
Method		Probit			Ordered Probit	
	Share of days of usage			Avg. daily usage time		
Panel B: Predicted actual usage	All	Non-compliers & control	Non-buying compliers & control	All	Non-compliers & control	Non-buying compliers & control
Invited	0.116***	0.0249	0.0833***	32.47***	9.113**	21.40***
	(0.0185)	(0.0157)	(0.0243)	(4.990)	(4.239)	(6.077)
Observations	953	563	268	953	563	268
Method		Tobit			Tobit	

See notes to table 1. The sample is restricted to individuals with non-missing self-reported usage.

Table G.3: Welfare impacts, LATE estimates

	(1) First stage	(2) Δ_t Monthly fuel expenditure	(3) Δ_t Has income generating activity	(4) Δ_t Weekly time working	(5) Δ_t Individual monthly income
ICS owned after the session	0.283***	-229.1	-0.0365	0.885	-11,817
	(0.0298)	(5,744)	(0.221)	(4.902)	(17,293)
		[84.46]	[90.41]	[90.67]	[90.49]
Share of days of usage	0.116***	-513.9	-0.0634	3.891	-26,589
	(0.0187)	(14,316)	(0.565)	(12.16)	(42,709)
		[35.67]	[38.93]	[37.51]	[31.83]
Avg. daily usage	32.47***	-1.828	-0.000227	0.0139	-95.00
	(5.029)	(50.91)	(0.00203)	(0.0436)	(151.8)
		[38.47]	[41.68]	[40.19]	[35.84]
Own ICS at the endline	0.311***	-211.4	-0.0332	0.809	-10,152
	(0.0449)	(5,306)	(0.203)	(4.453)	(15,015)
		[43.29]	[47.99]	[46.68]	[45.62]
N. ICS owned at the endline	0.477***	-138.7	-0.0217	0.527	-6,908
	(0.0961)	(3,484)	(0.133)	(2.903)	(10,496)
		[21.72]	[24.59]	[23.82]	[19.64]
Observations	989	971	989	987	823

Note: Column 1 reports the coefficient of "Invited" from each first stage regression on the whole non-attrited sample. Coefficients in columns 2-5 are obtained from separate regressions using the different instrumented variables reported. Kleibergen-Paap Wald rk F statistics are reported in square brackets. All models include individual controls include age, marital status, household size, number of women in the cooking rotation, dummies for education levels, participation to informal groups, having an income generating activity, use of saving device, an index for wealth, knowledge about ICS, owning an ICS at baseline, normalized distance from the drop-off point. Standard errors, in parenthesis, are clustered by 36 sampling points, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table G.4: Direct and spillover effects of the training session on ICS ownership, sample of women not owning ICS at the baseline

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ICS ownership				N. of ICS owned			
	All		Non-participants & control	Non-buying participants & control	All		Non-participants & control	Non-buying participants & control
Invited	0.317***		0.0714*	0.205***	0.488***		0.267**	0.361***
	(0.0460)		(0.0393)	(0.0659)	(0.0868)		(0.110)	(0.103)
Paricipated		0.672***				1.033***		
		(0.0906)				(0.185)		
Observations	797	797	473	227	797	797	473	227

Note: See notes to table 1. Invitation to the session is used as instrumental variable for participation. The Cragg-Donald Wald F statistic for weak instrument in the first stage is 83.69.

Table G.5: Direct and spillover effects of the training session on ICS usage, sample of women not owning ICS at the baseline

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	High frequency usage (every day)				Frequency usage score (0-5)			
Panel A: Self-reported usage	All		Non-participants & control	Non-buying participants & control	All		Non-participants & control	Non-buying participants & control
Invited	0.240***		0.0529	0.145***	1.394***		0.288	0.878***
	(0.0474)		(0.0350)	(0.0453)	(0.237)		(0.178)	(0.227)
Paricipated		0.503***				2.927***		
		(0.0724)				(0.377)		
Observations	769	769	455	218	769	769	455	218
	Share of days of usage				Avg. daily usage time			
Panel B: Predicted actual usage	All		Non-participants & control	Non-buying participants & control	All		Non-participants & control	Non-buying participants & control
Invited	0.119***		0.0196	0.0725**	32.22***		6.689**	17.16**
	(0.0189)		(0.0127)	(0.0271)	(4.945)		(3.222)	(6.669)
Paricipated		0.249***				67.64***		
		(0.0346)				(8.823)		
Observations	769	769	455	218	769	769	455	218

Note: See notes table 1. The sample is restricted to individuals with non-missing self-reported usage. Invitation to the session is used as instrumental variable for participation. The Cragg-Donald Wald F statistic for weak instrument in the first stage is 82.39.

Table G.6: Direct and spillover effects of the training session on knowledge of other people owning ICS, measured at endline

	(1)	(2)	(3)	(4)	(5)	(6)
	Know people owning ICS		Know people owning ICS: family or friends		Know people owning ICS: neigbours	
	All	Non-participants & control	All	Non-participants & control	All	Non-participants & control
Invited	0.178**	0.0717	0.0255	0.00572	0.285***	0.146**
	(0.0695)	(0.0767)	(0.0547)	(0.0557)	(0.0605)	(0.0642)
Observations	989	587	989	587	989	587
R-squared	0.037	0.029	0.025	0.057	0.066	0.036
Control Mean	0.373		0.306		0.146	

Note: All outcomes are measured at the endline. Individual controls include age, marital status, household size, number of women in the cooking rotation, dummies for education levels, participation to informal groups, having an income generating activity, use of saving device, an index for wealth, knowledge about ICS, owning an ICS at baseline, normalized distance from the drop-off point. Specifications in columns 2 and 6 are obtained via IV, the remaining via OLS. Invitation to the session is used as instrumental variable for participation. The Cragg-Donald Wald F statistic for weak instrument in the first stage is 97.88. The sample "All" is formed by all non-attriter women (both invited and non-invited to the training session). "Non-participants & control" includes invited women who did not attend the training and non-invited ones. "Non-buying participants & control" includes attending women who did not buy and non-invited ones. Standard errors, in parenthesis, are clustered by 36 sampling points, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.