Social interaction and technology adoption

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

Abstract

Easy-to-use and low-risk 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 strong effects from our invitation to the session on ICS ownership and usage while no discernible effects on product knowledge or household welfare. We find that some diffusion occurs beyond the intervention and provide evidence on the role of social interaction, mostly through imitation.

Keywords: Technology Adoption, Social Interaction, Imitation Effects, Cookstoves, Mali

JEL classification: D91, O33, O13, M31

1 Introduction

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 Global Burden Disease study estimates that, worldwide, almost four million people die prematurely every year from indoor air pollution due to the use of traditional cooking fuels and stoves (Lim and et al., 2012; Martin et al., 2011). 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 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. Given their potential benefits, ICSs have been promoted worldwide, notably through an initiative led by the Clean Cooking Alliance hosted by the United Nations Foundation. ICSs are considered a tool in helping achieve the seventh Sustainable Development Goal set by the United Nations in 2016 to ensure access to affordable, reliable, sustainable and modern energy for all by 2030. Accordingly, similar interventions to ours have been implemented in various countries (Miller and Mobarak, 2013; Mobarak et al., 2012; Hanna et al., 2016)[1]. Despite this, the take-up and sustained usage of ICSs remain low including in the context we focus on.

In this article, we provide evidence of the impact of an intervention aimed at increasing the take-up of improved cookstoves. Further, we investigate how social interaction affects the decision to adopt ICSs and its diffusion. The study takes place in Bamako, the capital

[1]The Berkeley Lab (managed by the University of California) has been active on that front and offers a number of publications related to take-up and usage of improved cookstoves: cookstoves.lbl.gov/publications/).

of Mali, which at the time had relatively low levels of adoption of ICSs and where most people rely on solid fuels (predominantly charcoal – 80% – and wood – 19% – according to our survey) and traditional technologies to cook.

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 20%). 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 sub-sample of ICSs. We test the effects of the training session on the level of knowledge about the product and its main features and find very limited effect. While our measures may not capture all the dimensions of knowledge about ICSs and the learning challenges related to its effective usage, our results are consistent with the absence of a relevant informational/knowledge gap on this product. Overall, we also 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.

We provide evidence that ICSs diffused beyond our intervention. We find that women who did not participate in the session but live close to those who did, are more likely to own and use an ICS at the endline than the women in our control group. This is particularly the case when non-participants live close to many neighbours owning ICSs. Although the comparison may be biased by self-selection, these effects represent suggestive evidence of the social interactions between participants and non-participants. To shed more light on the

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

mechanisms underlying these social interactions, we implemented a randomized treatment during the training session: before the purchase decision was individually made, we provided half of the participants with information 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" or "known". We exploit the variation in the intensity of these pairwise relationships. We find evidence that women tend to delay the purchase in order to collect more information when the content of the nudge is not informative. This seems to be the case when the information is about an unknown peer who purchased. Such negative effect is offset when the information is about a known peer who purchased. Our results also point at the importance of the relevance of reference groups in social information nudges (Bicchieri and Dimant, 2019). When the characterization of the reference group improves, in our case along the wealth dimension, the extent of imitation increases.

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 rigorously disentangle their respective role, our evidence is suggestive of the prevalence of the 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 in the ICS take-up decision. 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 involve significant behavioral changes, adjustments to one's cooking technique, or important informational gaps. Second, an ICS is a cheap technology and bears relatively low risk (it will deliver heat and cook but has a small probability of breaking). It implies relatively little investment, i.e. less than 5% of household monthly income. 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. Our results on the (lack of) impact of training session on knowledge suggest that social learning is unlikely to be the predominant channel for ICS take-up, while it can play a role in its efficient usage. 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 document the impact of a training session including the opportunity to purchase ICS on the spot on ICS take-up and usage. Our experimental design does not allow to disentangle the effect of providing information during the training session from that of offering the opportunity to purchase an ICS. However, similar interventions are implemented in different fields with the aim of introducing and spread new technologies (Meredith et al., 2013; Bonan et al., 2017; Ashraf et al., 2013; Bonan et al., 2017). The evidence on the impact of such interventions is still scant for ICS, while the literature on the drivers and barriers to ICS adoption suggests that liquidity constraints, intra-household preferences, information inefficiencies, and marketing strategies play significant roles (Hanna et al., 2016; Miller and Mobarak, 2015; Mobarak et al., 2012; Bensch et al., 2015; Levine et al., 2018; Meredith et al., 2013; Berkouwer and Dean, 2019; Pattanayak et al., 2019; Bensch and Peters, 2020).

Second, 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; Beltramo et al., 2019; 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

[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.

stoves, is insufficient to significantly climb the "energy ladder"[4].

Third, we complement existing investigations exploring the role of peer effects in the adoption of ICSs (Miller and Mobarak, 2015; Beltramo et al., 2015a; Adrianzén, 2014) and find results suggestive of an imitation mechanism[5]. We also provide empirical evidence from an information treatment in which individuals are nudged about the purchase decision of a peer living in the same neighbourhood. 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 contributions, 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., 2013; Costa and Kahn, 2013; Bonan et al., 2020; Bursztyn et al., 2014; Cai et al., 2015). Our study differs in that we offer information about a specific community member's decision[6]. Our results point at the importance of the identification of a relevant reference group in designing effective social information nudges (Bicchieri and Dimant, 2019).

The rest of the article is organized as follows. Section 2 introduces the context, experimental design, sample, and data collection. In Section 3, the data and summary statistics are presented. Section 4 presents identification strategies and results related to the direct impact of the training session, while Section 5 gathers the evidence on social interaction effects. Section 6 discusses the results and Section 7 concludes.

[4]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; Beltramo et al., 2019).

[5]For a broader review of peer influence on household energy behaviours see Wolske et al. (2020).

[6]Similarly, Miller and Mobarak (2015) convey purchase decisions by locally identified opinion leaders.

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[7]. 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[8]. 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% compared to the traditional charcoal stove[9]. 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.

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

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

[9] Laboratory 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.

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)

2.2 Experimental design

2.2.1 Training session experiment

From October 2014 to January 2015, we conducted a baseline survey of 1080 women from 36 neighbourhood 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 direction. Further details on our sampling design can be found in Appendix C.

From each starting point, 25 contiguous houses were selected[10]; then, after a 10-minute walk in the prescribed cardinal direction, another five houses were selected adjacent to the arrival location, following the same household selection procedure. Hence, of the 30 houses forming each cluster, 25 were assigned to the treatment and five to the control sample. The rationale for assigning 25/5 households to the treatment and control group is one that comes from statistical power as well as financial and organizational reasons. First, the total sample size was limited due to financial constraints. Second, we designed the study in two steps: the first was the invitation to the training, the second was the peer information treatment. The second one was conditional on attendance at the training session. We anticipated partial compliance: i.e. only a fraction of invited individuals would attend our training sessions. We thus needed a larger treatment group to ensure a large enough pool of participants to run the peer-info experiment. 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*[11]. 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.1 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, while the distance to control households is determined to avoid spillover effects from treated to non-treated areas within clusters[12].

[10]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.

[11]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.

[12]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.

The nine-hundred women in the treatment arm received an invitation to a training session (one per cluster) to be held in a nearby school on a Saturday in ten days' time[13]. 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[14]. Participants were also informed that they would be reimbursed 1,000 CFA (i.e. the usual taxi fare for a return trip within a neighbourhood) of transport costs[15]. One day before the session, all invited women received a reminder call. Each session was specifically organized to only gather women from a same cluster. Training sessions were conducted by a professional product promoter[16] 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 a separate 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 not buy one. The second option was introduced to relieve cash constraints at the time of

[13] Saturday was chosen to maximize the presence of women, based on a dedicated question asked during the pilot phase. We conducted four training sessions per week.

[14] 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*."

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

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

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[17]. Women then left the session by a separate entrance to eliminate the possibility of communicating and influencing the women still waiting[18]. 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

During the purchase phase, once the information session was completed, a randomized treatment was implemented. The women were called one by one to a separate room, in a randomly predetermined order. Half of them were individually invited to purchase an ICS (or leave a deposit for a purchase in five days' time) without further information. After making their decision, they left the room and the session so that they could not communicate with any other remaining participants. These formed the control group. The other half of participants were offered the same purchase options, but only after being provided with information (name, surname and purchase decision) on a specific peer (from the first half of the group) who had already made the decision and left[19]. A woman assigned to this treatment arm would receive 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.,

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

[18] While waiting for their turn in the main room, women were not prevented from interacting among themselves. However, the use of cellphones was not allowed during the entire session which means that they could not communicate with women who had left the session.

[19] The randomization protocol was incorporated into a software that we designed for data collection and treatment administration on tablets. All participants were randomly allocated a number (ranging from one up to the total number of participants in a given session). Half of participants – women who occupied even positions – were assigned to the treatment group ("peer info treatment group") and the other half – those in odd positions – to the control group ("peer info control group"). For each woman in the peer info treatment group, the woman on whom information was received was the one immediately preceding her in the predetermined order.

the peer did not purchase an ICS in the session). The reason to hold purchase decisions in a separate area was to control available information on the peers' purchase choices, avoiding women from influencing each other outside of the treatment assignment, and hence preserve the experimental setting. For the same reason, we 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.

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. The interviewer would take note separately of the names mentioned in reply to this question; combining the answers allowed us to identify two types of relationships: unknown and known by sight.

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.

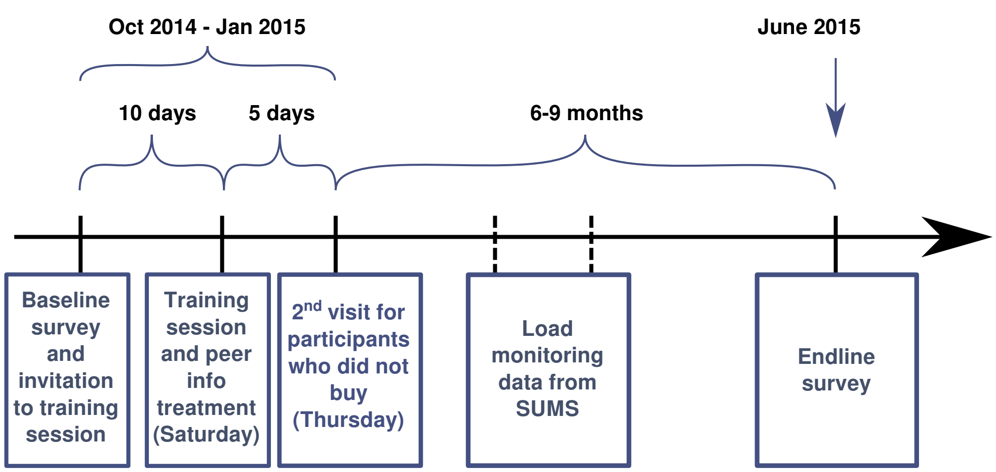
In June 2015 (the endline, which was six to nine months after the baseline), we administered a questionnaire similar to the baseline survey. Figure 2 shows the timeline of our study.

A random sample of 100 ICSs were equipped with SUMSs that recorded temperatures over time and hence allowed us to measure usage[20]. This makes such monitoring feasible and reliable across a large number of households, while mitigating the risk of the Hawthorne effect,

[20]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 their presence. The presence of the SUMS was obvious for all concerned in the first collection of monitoring data.

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 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).

Figure 2: Timeline of the study



2.4 Samples and attrition

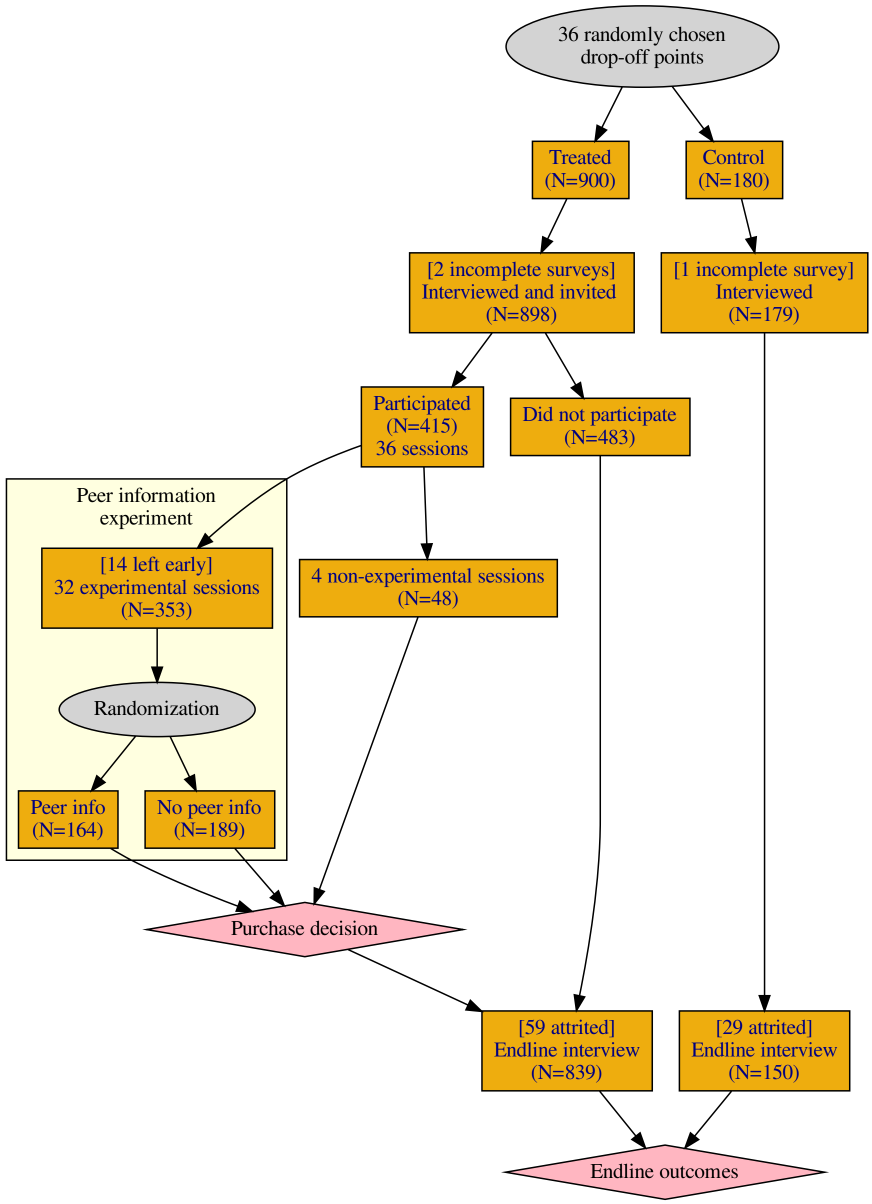
The study sample target was 1080 individuals in 36 neighbourhood clusters, 900 individuals are invited to the training session and 180 are in the control group. After the first survey round, three incomplete questionnaires (two in the treatment and one in the control) were discarded. Hence, the baseline 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 eventually attended, with an average of 11 women per session and a total of 415 participants. These represent the target sample for the peer information experiment, however in 4 out of 36 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. In the 32 training sessions in which the informational treatment was correctly implemented, 14 participants left the training early and did not take part to the experiment. Out of the 353 remaining participants, 164 women are randomly assigned to receive the peer information treatment, while 189 did not get it. At the endline survey, we are able to successfully track 989 individuals (839 in the treated and 150 in the control sample), for an overall attrition rate of 8.1%. Figure 3 summarizes the sample composition and size at different stages of the study.

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[21]). This seems to be the outcome of small sample size and relatively high attrition in few control clusters[22]. 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. 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. Appendix D provides further details about the attrition in the various stages of the experiment and their consequences on the internal and external validity of the results.

[21] 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.

[22] 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).

Figure 3: Summary of study flow



3 Data and summary statistics

In our baseline sample, study participants are on average 32 years old and 89% 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[23]. We compute a wealth index using principal component analysis, as suggested by Filmer and Pritchett (2001), by aggregating 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 neighbourhood 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),[24] and 50% own at least one small gas stove typically used for quickly heating water for baths or heating up leftovers[25]. 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 at the baseline 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,

[23]The averages presented for working time and income are for the full sample, not conditional on working. 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.

[24]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.

[25]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.

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.

Table 1 shows individual and household level summary statistics and assesses balance across groups in the different treatments. The samples of invited and non-invited appear balanced across all the observable baseline characteristics considered (column 3). Similarly, baseline characteristics for the treated and control households in the peer information experiment also appear similar across the two groups (column 7). Appendix B provides more details on the construction of the variables.

As far as ICS ownership is concerned, the difference 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.

As far as ICS knowledge is concerned, we asked women whether they know the product, its main features, where it can be purchased, and its potential efficiency benefits. 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

Table 1: Summary statistics and sample balance related to the invitation and peer information treatments

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Invitation treatment				Peer information treatment			
	Mean	SD	ITT	SE	Mean	SD	ITT	SE
Participated to training session	0	0	0.462***	0.029				
Endline survey not administered	0.162	0.369	-0.096**	0.037	0.0265	0.161	0.022	0.022
Respondent age	32.25	10.91	0.975	1.048	34.71	11.74	0.645	1.225
Live in couple	0.894	0.309	-0.021	0.038	0.868	0.340	0.004	0.034
Size of gwa	13.07	8.865	-0.236	1.07	14.12	9.164	-1.220	0.968
N. of women in cooking rotation	1.771	1.491	0.048	0.16	1.899	1.535	-0.101	0.124
No schooling	0.408	0.493	0.030	0.037	0.497	0.501	-0.040	0.053
Primary school	0.151	0.359	-0.004	0.03	0.153	0.361	0.011	0.035
Secondary school	0.128	0.336	-0.019	0.035	0.111	0.315	-0.020	0.036
High-school or above	0.313	0.465	-0.007	0.044	0.238	0.427	0.048	0.047
Have income generating activity	0.436	0.497	0.020	0.053	0.434	0.497	0.036	0.048
Weekly time working (hours)	5.056	8.606	1.317	1.011	5.254	8.699	1.246	0.861
Respondent monthly income (CFA)	16,538	28,243	3,214	2,644	13875	26,190	9,842***	2,822
Head monthly income (CFA)	55,408	60,159	7,763	9,006	65022	75,527	-5,130	8,424
Wealth index	-0.0684	2.272	0.082	0.277	-0.297	2.023	-0.002	0.253
Monthly hh fuel expenditure (CFA)	13,574	13,480	-160	1,027	14184	15,079	-1,764	1,269
Personal savings	0.274	0.447	0.049	0.045	0.317	0.467	0.024	0.047
Member of informal groups	0.536	0.500	0.009	0.045	0.540	0.500	0.040	0.057
ICS in the gwa	0.173	0.379	0.029	0.034	0.180	0.385	-0.003	0.038
N. of ICS in the gwa	0.318	0.864	0.005	0.078	0.296	0.720	-0.034	0.072
N. of stoves in the gwa	4.425	3.222	-0.054	0.402	4.217	2.705	0.204	0.303
Know ICS	0.916	0.278	0.025	0.029	0.958	0.202	-0.006	0.022
ICS allows to save fuel	0.760	0.428	0.026	0.058	0.857	0.351	-0.028	0.031
N. of women known in the session					6.127	5.101	0.196	0.266

Note: The Table reports individual and household level summary statistics and assesses balance across groups using data from the baseline survey. Mean and standard deviation for the control group at baseline in the invitation treatment and peer information treatment are shown in columns 1-2 and 5-6, respectively. Columns 3 and 7 report an Intention to Treat (ITT) estimate of the difference in means between the treatment and control group. Hence, column 3 shows the difference between invited (N=989) and non-invited individuals (N=179). Columns 7 shows the difference between women receiving the peer information treatment (N=164) and those not receiving it (N=189). Robust standard errors of ITT estimates clustered at the cluster level and robust standard errors are reported in columns 4 and 8, respectively. Asterisks denote statistical significance: *p $<$ 0.10, **p $<$ 0.05, ***p $<$ 0.01.

of an ICS?” We coded their answers with a set of dummy variables taking the value of one if the following features were mentioned: 1. Expensive; 2. “ICS efficient, allows to save fuel” 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”; 3. “more long lasting” or “good material/design” or “work well”; 4. “produce less smoke”, "more healthy"; 5. “do not work well” or "low quality". All these questions were asked both at the baseline and at the endline. Hence, the first difference is used as outcome variable. 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", 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.

We assess the extent to which women interact, influence and are influenced by others, in relation to ICS adoption. First, we asked respondents if they know other people owning ICSs and classified them as "family members or friends" and "neighbours". We also construct a dummy for whether women talked about ICS with other people after the intervention (for treated women the question refers to the session, for control ones it refers to the baseline survey) in general, with family or friends and with neighbours. Finally, we generate a dummy equal to one if the woman claims that she convinced (or influenced) someone else in the decision to purchase ICS and a variable with the number of women convinced. 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[26]. Panel A of Table E.2 reports the descriptive statistics for usage variables from the SUMSs. We find that about 73% (55 of 75) of women use the ICS

[26] Section E.2 provides the details.

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 main 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. About 38% and 36% of women in the peer information and control samples purchased an ICS at the end of the Saturday session, respectively. We also measure whether a woman left the deposit (28% of participants) with a binary variable. Out of those, about 80% eventually purchased ICS at the second visit, on Thursday.

4 The impact of the training session

4.1 ICS ownership and usage

We investigate the extent to which our training session influences women's ICS ownership and usage. We examine the Intention To Treat (ITT) effect of the invitation to the training session. We run the following reduced-form estimation on the whole sample:

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

where $Invited_i$ is a dummy equal to one if the individual is invited to attend the training session and zero otherwise and Y_i is the outcome of interest, expressed either in levels at the endline or in first difference. 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[27]. We also compute the Local Average Treatment Effect (LATE) of participating in the session on the outcomes of interest. This is obtained using instrumental variables, where participation is instrumented by invitation to the session, which is randomly assigned, hence uncorrelated with the outcomes.

Panel A of Table 2 shows that being invited to the training session increases the likelihood of owning an ICS by 31 percentage points (this represents a 160% increase on the baseline value) and the number of ICSs owned by 0.48 units (156% increase). All outcome variables in this table are expressed in levels. Columns 2 and 6 show the LATE of participating in the session[28]. 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.

Panels B and C of Table 2 report the effects of the invitation to the training session on self-reported usage and predicted actual usage, respectively[29]. 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 (Panel B, column 1). This represents a 148% increase over control households. The effect doubles for women who participated in the training session (Panel B, column 2). As far as predicted actual usage is concerned, we find that the share of days of ICS usage increases by 11 percentage points (about 200% increase) as the effect of being invited to the training session (Panel C, column 1) and by 25 percentage points for participants (Panel C, column 2) over control households. Positive and strongly significant effects are also found for the average daily time of usage.

[27] 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.

[28] 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.

[29] Appendix E.3 explains how these predicted values are computed.

Invited and participating women use ICSs for 32 and 69 more minutes per day than control ones, respectively (Panel C, columns 3-4). These represent 246% and 530% increases over control households[30]. Few reasons can explain why receiving an invitation and participating in the training may lead to higher self-reported and predicted usage. For some women who owned an ICS before the invitation and who had limited usage at the baseline, the invitation may have acted as a reminder, or a signal that would put usage at the forefront of their preoccupation. In that sense we could think of a salience effect. Similarly, this salience effect can be at play for women who bought an ICS following our treatment. For first time buyers, we can also think of a novelty effect whereby owners are more inclined to use a recently acquired technology.

We must be cautious here, as the results presented have some limitations. First, we have significantly different attrition rates for the groups of invited and non-invited women (Table D.1, column 1). 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 the effect of the training session on ownership and usage is overall robust to different missing data scenarios (Table D.2). Second, one can argue that the decision to adopt and use an ICS may be different for women already owning one at the baseline. As a robustness check, we repeat the exercise on the sub-sample of women who did not own ICS at the baseline. Results, reported in Table G.3, are qualitatively similar. Finally, we repeat the analysis including individual controls in Table G.4. Estimates do not change in terms of degree of significance and appear slightly larger in magnitude compared to the ones without covariates, which then appear conservative.

[30] We use alternative estimation models for the results in Table 2. We use the probit model for binary outcomes, ordered probit for Panel B, columns 3-4, and Tobit model for columns 1–8 of Panel B. The results are qualitatively similar, as reported in Table G.2, Panel A, in Appendix G.

Table 2: Impact of the training session on ICS ownership and usage

	(1)	(2)	(3)	(4)
Panel A: Ownership	ICS ownership		N. of ICS owned	
Invited	0.311***		0.477***	
	(0.045)		(0.096)	
Participated		0.670***		1.026***
		(0.089)		(0.195)
Observations	989	989	989	989
Control Mean	0.186		0.307	
Panel B: Self-reported usage	High frequency usage (every day)		Frequency usage score (0-5)	
Invited	0.251***		1.390***	
	(0.0442)		(0.209)	
Participated		0.534***		2.962***
		(0.079)		(0.388)
Observations	953	953	953	953
Control Mean	0.131		0.717	
Panel C: Predicted actual usage	Share of days of usage		Avg daily usage time	
Invited	0.116***		32.47***	
	(0.019)		(5.029)	
Participated		0.248***		69.17***
		(0.034)		(9.199)
Observations	953	953	953	953
Control Mean	0.057		13.04	

Note: The Table reports estimates of model 1. All outcomes are level variables and measured at the endline. The regressions do not include controls. Specifications in columns 2 and 4 are obtained via IV, the remaining via OLS. Invitation to the session is used as instrumental variable for participation. In Panels B and C the sample is restricted to individuals with non-missing self-reported usage. Control mean refers to the mean outcome in the control group (non-invited). Standard errors, in parenthesis, are clustered by 36 sampling points, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

4.2 Knowledge and learning

We investigate the impact of the training session on knowledge about ICS, its use and main features. Table 3 shows the ITT effects of the invitation to the training session on two sets of proxies related to ICS knowledge (Appendix B provides more details). In Panel A all outcome variables are expressed in levels except $\Delta_t KnowICS$, which is in first difference. We do not find any significant effect on the difference in knowledge of ICSs (column 1), where to buy it (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). In Panel B, we analyze the responses to an open-ended question inquiring the main features of ICS[31]. We find mild evidence of a decrease in the share of individuals believing ICS is expensive (p=0.08), while no significant impact is found for the other attributes (efficiency, good material, less smoke, not working well).

As a robustness check, we repeat the analysis adding the set of individual and household controls. Results, shown in Table G.5, mostly confirm the findings. However, we find mild evidence of increased knowledge of the correct estimate of fuel saving (Panel A, column 3, p=0.08), while we lose significance in the share of those believing ICS are expensive (p=0.12).

Overall, our invitation does not seem to significantly raise knowledge on ICSs, as measured through our survey questions. However, we acknowledge that the measures used may not fully capture the dimension of knowledge of ICS and all its attributes.

[31]The question was asked at both survey waves, hence we analyze the difference in time, Δ_t.

Table 3: Effects of invitation on knowledge of ICS

	(1)	(2)	(3)	(4)	(5)
Panel A: ICS general and specific knowledge of its use					
	Δ_t Know ICS	Know where to buy ICS	Correct estimate of fuel saving (20-40%)		
Invited	-0.006	0.024	0.027		
	(0.052)	(0.054)	(0.057)		
Observations	989	989	989		
Control Mean	0.893	0.727	0.213		
Panel B: ICS main features (Δ_t)					
	Expensive	Efficient, allow fuel saving	Good and lasting material, work well	Less smoke, more healthy	Not working well, low quality
Invited	-0.130*	0.087	-0.071	0.054	-0.002
	(0.075)	(0.073)	(0.088)	(0.039)	(0.014)
Observations	989	989	989	989	989
Control Mean	0.233	0.627	0.420	0.080	0.013

Note: The Table reports estimates of model 1. All outcomes in Panel A are measured at the endline. Except for "Know ICS" and all outcomes in Panel B which represent the difference between endline and baseline values (Δ_t). The regressions do not include controls. The analysis is performed on the whole non-attrited sample. Control mean refers to the mean outcome in the control group (non-invited). In the case of outcomes expressed in differences, the control mean is the mean in the control group at the endline. Standard errors, in parenthesis, are clustered by 36 sampling points, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

4.3 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 of time spent on meal preparation. We estimate the reduced form on the differences in outcomes between the endline and baseline values. The results are reported in Table

4[32]. Receiving an invitation to the training session does not seem to have any significant impact on overall monthly fuel expenditure at the household level[33], individual propensity to have an income-generating activity, time spent on income-generating activities, or monthly income[34].

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. We use the invitation to the training session as the instrumental variable for these variables. Column 1 of Table G.7 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 4. 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 and find that ICSs are significantly more frequently

[32]In Table G.6 we repeat the analysis adding covariates. Results do not change.

[33]Miller and Mobarak (2015) 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.

[34]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.

used in the former than in the latter. However, no significant differential effect arises for welfare outcomes along this dimension[35].

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; Beltramo et al., 2019).

One can argue that the non-significant coefficients shown in Table 4 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)[36]. 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% reduction, about half the minimum efficiency gains measured by GERES through laboratory tests.

5 Social interaction effects

In this section we present different pieces of evidence pointing at the role of social interaction in ICS diffusion. First, we report on the peer information experiment, where we explore the role of information on a peer's behaviour on individual decision in a controlled environment, albeit with reduced external validity. Second, we look at the impact of our intervention on

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

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

Table 4: Welfare impacts

	(1) Δ_t Monthly fuel expenditure	(2) Δ_t Have income generating activity	(3) Δ_t Weekly time working	(4) Δ_t Respondent monthly income
Invited	-64.4	-0.010	0.250	-3,353
	(1,649)	(0.064)	(1.416)	(5,022)
Observations	971	989	987	823
Control Mean	11,725	0.493	4.280	14,599

Note: The Table reports estimates of model 1. Outcomes in columns 1 and 4 are first-difference expressed in FCFA. The regressions do not include controls. Control mean refers to the mean outcome in the control group (non-invited). Standard errors, in parenthesis, are clustered by 36 sampling points, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

those who did not directly benefit from it, by comparing non-participants and control. We also investigate whether non-participants surrounded by more participants and ICS owners are more likely to own an ICS at the endline. Finally, we assess the impact of the training session on one's likelihood to interact and discuss in relation to ICS. The evidence outside the experimental session provides more externally valid results. However, they do not allow us to clearly isolate channels given the many confounding factors.

5.1 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:

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

where Y_i is the outcome for woman i, defined in four different ways: (1) it takes value one if she buys or leaves the deposit on Saturday and zero otherwise. The related sample includes all women involved in the peer info experiment (N=353); (2) it takes value one if she buys and zero if she leaves a deposit; (3) it takes value one for purchase and zero for non-purchase; (4) it takes value one for leaving the deposit and zero for non-purchase.

Models two, three and four imply a sample reduction: only women satisfying two of the three mutually exclusive outcome choices are included. All variables are expressed in levels. 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. Results are presented in odd columns of Table 5.

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:

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

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 by the combination of whether or not the peer purchased, i.e. PB_i vs $(1-PB_i)$, and whether or not woman i knows her peer at least by sight, i.e. $Known_i$ vs $Unknown_i$. Results are presented in even columns of Table 5[37].

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[38]. Clusters/sessions may also

[37] The analysis is based on the comparisons across five mutually exclusive groups. For the full sample (columns 1-2) the sample sizes are: 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).

[38] Table 1 shows that the average number of women known by sight in a session 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. Table 1 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.

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 treatment 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 by including session fixed effects.

Compared to receiving no information, being informed about a peer who purchased at the session (measured by β_2 in odd columns) does not lead to a significant effect on the likelihood of purchasing and/or leaving the deposit. This result holds when the relation with the peer is more intense: women informed that a known peer purchased do not buy or leave the deposit more than peer-info control ones (β_4 is never statistically significant for all specifications). Instead, we find that women who received the information that an unknown peer bought tend to buy significantly less, as opposed to leaving the deposit, compared to women who received no information (β_2 is negative and significant in column 4) or who receive the information that an unknown peer did *not* buy ($\beta_1 = \beta_2$ is rejected in column 4). A similar pattern seems to arise for the impact of the information that an unknown peer bought on the decision to purchase (as opposed to non-purchase), but it is not significant (β_2 in column 6 has p-value=0.167, $\beta_1 = \beta_2$ is rejected in column 6). We also find that women who received information that a known peer bought, did buy significantly more than those who received information that an unknown peer bought (β_2=β_4 is rejected). This holds when considering purchase both as opposed to leaving the deposit (column 4) and as opposed to non-purchase (column 6). No significant treatment effects are detected in relation to the decision to leave the deposit vs non-purchase (columns 7-8).

In Table G.8 we replicate the exercise including individual controls. The results are qualitatively similar[39]. One could be concerned that results shown in columns 3 to 8 of Table

[39]The inclusion of covariates allows us to look at the determinants of the willingness to buy ICS at the session. In particular, we find that women who purchase or leave the deposit at the session are significantly more likely to live in couple (p=0.04), to be involved in smaller cooking rotations (p=0.014), are wealthier (p=0.05) and significantly less knowledgeable about ICS at the baseline (p = 0.003). Results are not shown

5 come from the analysis of endogenously defined subsamples of our experimental population. We repeat the exercise using different alternative strategies implying the use of the full sample (N=353). In particular, we construct a new outcome variable, a willingness to buy index, equal to zero for non-purchase, to one for leaving the deposit and to two for purchase at the session. Based on this variable, we run multinomial logistic, ordered probit and OLS regressions. Overall, the results, shown in Table G.9 are qualitatively similar in terms of signs and significance levels to the ones described above. Given our experimental design, the results in Table 5 focus on the decisions taken 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, like the purchase five days afterwards (during our Thursday visit), would likely violate the Stable Unit Treatment Value Assumption (SUTVA).

Overall, the treatment nudging peer's purchase decisions fail to deliver higher take-up rates. In our context, bad advertising may be a factor for some individuals suspicious of an offer to buy when they are told that an unknown individual has bought. Such negative effect is cancelled for individuals receiving information on a known peer who purchased. Overall, the average treatment effect is non significant, possibly because of the inability of our treatment to provide information on the actions of a relevant reference group. Bicchieri and Dimant (2019) claim that failing to identify relevant reference networks may lead to ineffective information nudges which may even backfire, as they tend to favour self-serving interpretations. This seems to occur in many empirical applications (Schultz et al., 2007; Goldstein et al., 2008; Dimant et al., 2020)[40].

but are available upon request.

[40]In a context similar to ours, Miller and Mobarak (2015) 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. Still, they detect an asymmetric importance of negative information, which has a stronger and more robust impact than positive one.

Table 5: Effects of information received on peer's purchase on ICS take-up

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Purchase or deposit vs no purchase		Purchase vs deposit		Purchase vs no purchase		Deposit vs no purchase		Discussed with other attendants about ICS before Thurs	
β_1: RI*PB=0	0.042		0.054		0.067		0.012		0.083	
	(0.056)		(0.085)		(0.067)		(0.072)		(0.064)	
β_2: RI*PB=1	0.008		-0.079		-0.004		0.041		-0.121	
	(0.060)		(0.083)		(0.079)		(0.086)		(0.085)	
β_1: RI*PB=0*Unknown		0.003		0.180		0.067		-0.051		0.111
		(0.080)		(0.120)		(0.091)		(0.113)		(0.100)
β_2: RI*PB=1*Unknown		-0.025		-0.208*		-0.164		0.076		-0.076
		(0.081)		(0.112)		(0.117)		(0.115)		(0.108)
β_3: RI*PB=0*Known		0.064		-0.024		0.067		0.045		0.068
		(0.070)		(0.100)		(0.086)		(0.086)		(0.076)
β_4: RI*PB=1*Known		0.041		0.053		0.085		-0.012		-0.191
		(0.077)		(0.101)		(0.090)		(0.121)		(0.132)
Left deposit									0.180***	0.180***
									(0.066)	(0.067)
Observations	353	353	232	232	253	253	221	221	221	221
$\beta_1 = \beta_2$	0.632	0.789	0.214	0.013	0.445	0.090	0.763	0.415	0.030	0.173
$\beta_3 = \beta_4$		0.803		0.559		0.874		0.672		0.070
$\beta_2 = \beta_4$		0.525		0.061		0.067		0.589		0.491

Note: The table reports OLS estimations of models 2 and 3 in two columns for each outcome. The outcomes are expressed in levels. RI: "Received Information on peer's purchase"; PB: "Peer bought ICS at the session". The reference category are peer-info control women (who received no info). The regressions include session fixed effects but not individual controls. The sample in columns 1 and 2 is formed by women who participated in the training session and who were successfully involved in the final experimental phase (N=353). The samples for columns 3-8 are formed by individuals satisfying the conditions set by the outcome variable. For example, columns 3-4 only include women who have either purchased or left a deposit at the session and exclude those who did neither. The sample in columns 9-10 includes women who did not buy at the training session and who received the second visit five days later. Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Our evidence seems to suggest that when the content of the nudge is not informative, women who are willing to buy, but are not sure enough during the session, postpone the decision in order to gather further information. Three findings seem to support this. First, when the nudge is about an unknown woman who purchased, women are more likely to leave the deposit instead of buying than those receiving no information (β_2 in column 4). Instead, when the information is on a known peer who purchased, such effect disappears ($\beta_2 = \beta_4$ is rejected in column 4). Second, for the sample of women who received the second visit from our staff, so those who did not buy at the session, we asked a question on whether or not they had discussed the ICS purchase with other women who participated in the same training session in the five days preceding the second visit (i.e. between the Saturday session and our second visit on Thursday). We use the resulting binary variable as dependent variable. Columns 9 and 10 of Table 5 show the results with this outcome variable for the usual models, with the addition of a dummy for leaving the deposit. We find in both columns that those who left the deposit are significantly more likely to discuss the purchase with other peers. Results in column 10 show that women receiving information about a known peer who did not buy are significantly more likely to discuss about ICS than those informed about a known peer who purchased ($\beta_3 = \beta_4$ is rejected in column 10 with p-value=0.07). Third, 80% of women who left a deposit eventually ended up buying an ICS on Thursday. Evidence presented in the next section confirms the role played by neighbours in the decision to purchase on Thursday. Finally, we further characterize the relevance of the reference group by looking at the wealth dimension. One could expect the peer information to be more relevant when the peer's purchasing power is similar to that of the treated woman receiving the information. We check this by adding a further dimension of heterogeneity. We generate a dummy equal to one when a pair (peer and treated woman) are in the same wealth quartile and zero otherwise. We interact with the regressors in equation 3. Results, reported in Table G.10, tend to confirm that the willingness to buy ICS is higher when women and their peers are in the same wealth quartile. This corroborates the evidence that the effectiveness of the nudge depends on the extent to which its contents are informative by conveying information on a relevant reference group [41].

5.2 Social interaction and ICS diffusion

First, we assess the impact of the invitation to the training session for women who were invited but did not attend compared to control ones. Non-participants did not directly benefit from session attendance, but could have been indirectly impacted through the social interaction with other participants in the neighbourhood. We are aware that the participant and non-participant groups may be formed as an outcome of an endogenous self-selection process and discuss the consequences of this on our results below. We run the reduced-form estimation as in equation 1 on such a sample: results are shown in Table 6 where all outcome variables are measured at the endline. 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 (columns 1 and 2). We also find positive impacts on ICS usage (columns 3-6). Although they appear smaller than the direct effects, these represent relevant increases with respect to the control mean.

These results remain suggestive. They are only significant at 10% confidence level, become insignificant if controls are omitted[42] and are not always robust to attrition (see Table D.2). Moreover, the group of non-participants received an invitation to the training session, while the control households did not. Such invitation may have raised awareness about ICS and fuel efficiency and consequently impact ICS purchase and usage. However, our questionnaire, administered to control and treated women equally, largely inquired about households' cooking habits and included detailed questions on ICSs and their usage[43]. Participants are, on average, significantly older, living in larger households, less educated, and less wealthy

[41] We thank an anonymous referee for useful suggestions related to this section.

[42] Results not shown but available upon request.

[43] Cookstoves were mentioned as the focus of our survey when introduced to all respondents and about 30% of the survey questions were related to topics including cooking, kitchens, stoves, and fuel. Finally, the decision to attend the session is endogenous.

than those who do not attend (see Table G.1, column 1). However, they appear similar to the pool of control individuals (see Table G.1, column 2). Moreover, under many observable dimensions, we find no indication that the group of non-participants could have higher demand for ICSs and hence would be negatively selected[44]. Many unobservable dimensions could still be relevant. For example, non-participants may be more curious, or more likely to search for information on their own (as they are more educated), and therefore, after hearing about ICS, may be more likely to ask for such stoves while visiting the nearby market.

Table 6: Impact of the training session on ICS ownership and usage, sample of non-participants and control

	(1)	(2)	(3)	(4)	(5)	(6)
	ICS ownership	N. of ICS owned	High frequency usage (every day)	Frequency usage score (0-5)	Share of days of usage	Avg daily usage time
Invited	0.089*	0.286**	0.076*	0.377*	0.025	9.113**
	(0.045)	(0.112)	(0.038)	(0.192)	(0.016)	(4.297)
Observations	587	587	563	563	563	563
Control Mean	0.187	0.307	0.131	0.717	0.0567	13.04

Note: The Table reports estimates of model 1. All outcomes are measured at the endline. The following individual controls are included: 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. Control mean refers to the mean outcome in the control group (non-invited). The sample includes (invited) non-participants at the training session and control individuals. Standard errors, in parenthesis, are clustered by 36 sampling points, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Second, we assess the extent to which women are influenced in their ICS purchase decision by the proximity of neighbours who participated and possibly bought an ICS during the intervention. We do this by exploiting a feature of our sampling strategy which randomly

[44] For example, they do not own more ICSs or know more about them than participants at the baseline. 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 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.

selects the starting points and truncates in space existing social networks (see section 2.2 and C.2 for details). By design this implies that women, regardless of their personal traits and social connections, are surrounded by a varying number of other women who have been invited, depending on their distance from the initial starting point. Such a number is deemed exogenous by construction[45]. We focus on two moments in time to assess the short and medium-term impacts of neighbour density of ICS take-up. First, we look at the decision to purchase ICS five days after the intervention, at the time of our second visit (on Thursday) for women who did not buy at the training (N=221). We look at the impact of the varying number of neighbours who: i. participated in the training; ii. purchased ICS at the training (Saturday); iii. purchased or left the deposit at the session (Saturday). We consider the variation in neighbours density for a range of distances from each woman. Given the differences in population density between areas of Bamako, we normalize distances by considering as a reference the mean pairwise distance among all women in any given cluster. We then multiply this value by a parameter α which ranges from 0.5 to 2 in order to smoothly vary the radius considered[46]. Figure 4 shows the marginal effects, with 90% confidence intervals, of having one additional neighbour who satisfies each one of the three characteristics listed above (participated in the training, etc.). This conditional on a certain radius around each woman. Results are depicted in panels (a), (b) and (c) of Figure 4. Second, we look at ICS ownership at the endline for women who did not participate to the training session (N=437). We look at the number of neighbours who: i. participated in the training; ii. participated in the training but did not purchase; iii. own ICS after the intervention. Results are depicted in panels (d), (e) and (f) of Figure 4.

We find that the number of participants to the training seems to have a positive impact

[45]Intuitively, within the same treated sampling point, we implicitly compare women at the boundary of the sampling areas with women at the core of it. The former are expected to have fewer neighbours who have been invited to the session compared to the latter.

[46]The mean pairwise distance within geographical clusters is about 95 meters (median 87 and maximum 388). This means that radii vary, on average, from 48 ($\alpha = 0.5$) to 190 meters ($\alpha = 2$). For values of α lower than 0.5 results are unreliable as a large number of women have no neighbours close enough. Notice that by definition a woman is *not* considered a neighbour of herself.

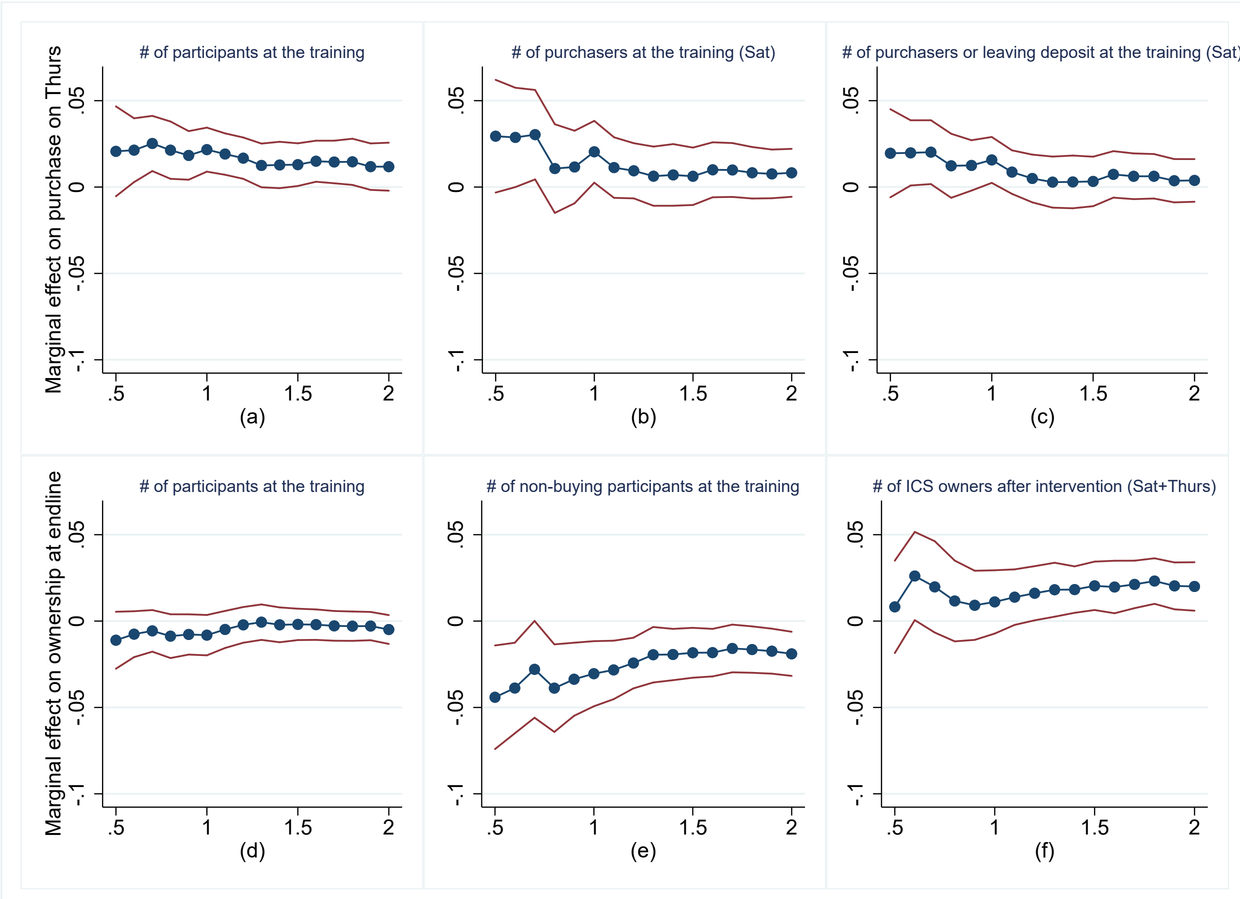
on short-term purchase (panel (a)), but none on ownership in the medium-term (panel (d)). The coefficients related to the number of ICS purchasers and those revealing a willingness to buy ICS at the session is consistently positive (but not always significant) in panels (b) and (c). The number of ICS owners after the session is somewhat positively and significantly related to ICS ownership at the endline (panel (f))[47]. In panel (e) we observe a clear negative effect for the coefficient for the number of participants in the training who did *not* buy. The absolute value of the coefficient decreases for larger α suggesting that proximity plays a role. This represents further suggestive evidence that negative/bad advertising can play a significant role in preventing the diffusion of this type of technology.

Third, we report the evidence on an increase in social interaction concerning ICS, following our intervention. Table 7 is based on outcomes measured at endline. Estimates in odd columns are based on the whole sample, those in even columns are on the sample of (invited) non-participants and controls. In Panel A, we find that the share of women knowing someone owning an ICS significantly increases by 16 percentage points as an effect of being invited to the session (column 1), mainly driven by neighbours (+30 percentage points, column 5). 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 (+3 percentage points, column 2), and positive and significant for neighbours (+15 percentage points, column 6). In Panel B we find that the session contributed to an increase in the discussion about ICS (+200%). In Panel C we show that, in some cases, such interaction between treated women and their neighbours lead to ICS purchase by the latter. All these later results only apply for the whole sample (columns 1 and 3), as no significant effects arise for the sample of non-participants vs control[48]. Overall, the results of Table 7 are suggestive

[47]It should be noted that for $\alpha = 2$, the majority of women in the cluster are considered each other's neighbour: the resulting effect can be considered akin to a session effect. The effect of ICS owners after the session is already significant for $\alpha = 1.2$ (which means considering on average around 12 women as neighbours): this means that the proximity of women who own an ICS is significantly related to the propensity to eventually own one

[48]Robustness checks including covariates are shown in Table G.11: significant coefficients typically increase in absolute value.

Figure 4: Impact of neighbours density on ICS take-up



Note: The graph depicts point estimates, together with 90% confidence intervals, for a variable counting the number of neighbours with the characteristics mentioned in the heading of each panel, living at different distances from each individual. Each point is obtained from a different regression, where the outcomes are: a binary variable for ICS purchase 5 days after the training (on Thursday) for panels (a), (b) and (c); a binary variable for the probability of owning ICS at the endline for panels (d), (e), (f). Regressions include the following 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. In the first three panels, the regressions also include a dummy for leaving the deposit at the session and one for discussing with others before Thursday. Women are considered neighbours if they live within a distance of α times the average pairwise distance in their cluster, with different values of α being shown along the horizontal axis. The analysis in panels (a), (b) and (c) is conducted on the sample of non-buying participants at the training, who received the second visit 5 days later (N=211); the sample in panels (d), (e), (f) includes non-participants to the training session (N=437).

of two main interpretations. First, the training session generated an increased interaction concerning ICS in the neighbourhoods. This is likely to concern participants to the session. Second, although apparently non directly involved in the active discussion about ICS (see non-significant coefficients in even columns for Panels B and C), non-participants see more people owning ICS around them (Panel A, column 6).

Table 7: Effects of the training session on social interaction concerning ICS

	(1)	(2)	(3)	(4)	(5)	(6)
	All	Non-part & control	All	Non-part & control	All	Non-part & control
Panel A	Know people owning ICS					
	All		Family and friends		Neighbours	
Invited	0.162***	0.027	-0.013	-0.080	0.301***	0.151***
	(0.057)	(0.062)	(0.058)	(0.059)	(0.049)	(0.049)
Observations	989	587	989	587	989	587
Control Mean	0.373	0.373	0.307	0.307	0.147	0.147
Panel B	Talked about ICS after intervention with					
	All		Family and friends		Neighbours	
Invited	0.209***	-0.002	0.148***	-0.009	0.170***	0.006
	(0.037)	(0.031)	(0.032)	(0.028)	(0.033)	(0.025)
Observations	989	587	989	587	989	587
Control Mean	0.100	0.100	0.073	0.073	0.053	0.053
Panel C	Someone bought ICS after discussion with woman		N. of people who bought ICS after discussion with woman			
Invited	0.134***	0.012	0.427***	0.089**		
	(0.025)	(0.017)	(0.075)	(0.043)		
Observations	989	587	989	587		
Control Mean	0.033	0.033	0.060	0.060		

Note: The Table reports estimates of model 1. All outcomes are measured at the endline. The regressions do not include controls. Estimates in odd columns are on the whole sample, those in even columns are on the sample of (invited) non-participants and controls.

6 Discussion

The results obtained by our intervention, in terms of ICS take-up and usage, are compelling compared with other interventions aimed at encouraging technology adoption[49]. 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

[49] Appendix F presents the cost-effectiveness calculations based on the estimated impacts.

messages conveying the benefits of an ICS did not affect willingness to pay for it. Our intervention combines information dissemination during the session with the opportunity to "act now" and buy ICS on the spot. Our experimental design does not allow to clearly separate the two effects. However, our results suggest different interpretations.

First, our training sessions may have raised knowledge about the product and its potential benefits. However, we find that our training sessions did not significantly improve knowledge on ICS, at least on the dimensions which we could measure. ICS is an easy to use product, because its functioning is similar to the widely diffused traditional cooking stoves. The level of knowledge about ICS existence, main attributes and potential benefits was already high among the population at the baseline. 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[50]. In that respect, ICSs are similar to rubber shoes (Meredith et al., 2013) and different from index insurance, menstrual cups, and contraceptives, where 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. 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 works find that interventions based on information dissemination alone are ineffective in raising technology adoption (Meredith et al., 2013; Bonan et al., 2017; Ashraf et al., 2013; Bonan et al., 2017).

Second, our training sessions offered the opportunity to buy on-the-spot at the market price. This may have lowered transaction costs for ICS purchase and thus help take-up. The literature suggests that: i) small reductions in transactions costs can dramatically affect technology adoption and ii) an on the spot offer can leverage on salience and impact on individuals with limited attention, commitment problems and mental constraints (Duflo

[50] Of the 1078 women involved in our study, only three had no previous experience of cooking with a traditional charcoal stove.

et al., 2011; Karlan et al., 2016; Hanna et al., 2014; Datta and Mullainathan, 2014). Some individuals in our sample may have limited attention ('scarcity of attention') to the necessity of buying an ICS. Drawing their attention to such a need, with our on-the-spot sale on Saturday or with a five-day delay on Thursday, may bring it to the 'top of mind'. Moreover, within our intervention we also offered a 1,000 CFA transportation refund for session participants. Such compensation, show-up or survey participation rewards are common features in field interventions. In our case, the refund may have been perceived as a subsidy for the ICS purchase by some participants. We have no evidence that the refund was used for buying a stove and not covering transportation costs. However, we cannot entirely dismiss that this could have further contributed to raising take-up. Thus, marginally, our refund may have helped mitigating any liquidity and credit constraints in ICS adoption (Berkouwer and Dean, 2019; Pattanayak et al., 2019; Levine et al., 2018; Ashraf et al., 2013; Meredith et al., 2013; Mobarak et al., 2012).

We also document that ICS diffusion occurs beyond our intervention, as non-participants are more likely to own than control women, particularly when they are surrounded by ICS owners. We argue that social interactions are likely to be a key driver. The process of technology diffusion through social interaction is likely to be outcome of social learning and imitation (Manski, 2000). Few recent studies have tried to disentangle imitation effects from social learning (Bursztyn et al., 2014; Bernard and Torero, 2015). Our research setup does not allow us to unambiguously identify the mechanism responsible for the interaction effects in ICS diffusion. However, the peculiarities of the ICS, its relatively ease-of-use, lack of major knowledge gaps in its characteristics and basic usage, the fact that ICSs were already well known before our intervention indicate that ICS adoption is unlikely to benefit from social learning. We acknowledge that the measures used may not fully capture the dimension of knowledge of ICS and all its attributes. This makes us lean toward imitation effects, although the social learning channel cannot be ruled out completely. The results of our "peer information treatment", showing that women react to the information about peer's behaviour, are suggestive of this channel. The evidence that non-participants are more likely to own ICS when they are surrounded by ICS owners, the fact that the training contributed to increase their knowledge of people owning ICS but did not influence their propensity to talk about ICS also suggest the relevance of the imitation channel. 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.1 that this is unlikely to be the case.

7 Conclusion

Our study investigates, in a context characterized by energy poverty, the role of a training session in the adoption and usage of a fuel-saving cooking technology. 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 social interaction 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. The use of social information campaigns can favour technology diffusion, but great care should be dedicated to its design, particularly in relation to the reference group which should be clear and relevant. 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 could also be on direct market penetration rather than complex informational campaigns.

We extrapolate from our results that once geographical penetration has occurred, a sufficient number of women, through social interaction, can help diffuse the technology. 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.

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Appendix

A Product and context

A.1 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% of rotations have only one woman, 20% two, 9% three and 9% four or more). This shows that cooking rotations with more than one woman represent a significant minority 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 never significant in our 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 the specifications of Table 5. 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

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.

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 neighbourhoods 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[51]. 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.

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 neighbourhoods, 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).

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

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 minute 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[52]. As an added benefit, treated and control points in each cluster are visited in the same week, hence controlling for any time-specific phenomena which might affect specific parts of the city.

[52]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 neighbourhood 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[53]). This seems to be the outcome of small sample size and relatively high attrition in few control clusters[54]. 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

[53]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.

[54]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).

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 5 (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 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 Table G.4, 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 Table G.4 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 again not statistically significant[55].

[55]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 Table G.4, 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	0.000	0.003**	0.002**
	(0.001)	(0.001)	(0.001)
Live in couple	-0.002	0.014	0.041
	(0.023)	(0.046)	(0.037)
HH size	-0.003*	-0.000	-0.002
	(0.001)	(0.003)	(0.002)
N. of women in cooking rotation	0.006	0.020*	0.005
	(0.007)	(0.012)	(0.009)
Primary school	-0.016	0.094**	0.012
	(0.024)	(0.040)	(0.029)
Secondary school	0.003	0.015	0.020
	(0.025)	(0.061)	(0.035)
Beyond Secondary school	-0.023	0.097**	0.017
	(0.024)	(0.040)	(0.026)
Have income generating activity	-0.017	-0.006	-0.016
	(0.020)	(0.039)	(0.029)
Wealth index	0.002	-0.005	-0.005
	(0.005)	(0.008)	(0.006)
Use saving device	-0.021	0.057	-0.009
	(0.018)	(0.042)	(0.029)
Member of informal groups	-0.016	-0.003	0.03
	(0.023)	(0.036)	(0.030)
Know ICS	0.006	-0.081	0.154*
	(0.031)	(0.056)	(0.091)
ICS in the HH	0.023	-0.099**	0.007
	(0.025)	(0.050)	(0.027)
Distance from drop-off point	0.016	0.045	0.001
	(0.012)	(0.035)	(0.013)
Constant	0.181***	0.742***	0.710***
	(0.059)	(0.091)	(0.113)
Observations	1,077	415	367
Mean Dependent Variable	0.081	0.884	0.962

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)
Panel A: Ownership				
	ICS ownership		N. of ICS owned	
	All	Non-participants & control	All	Non-participants & control
Mean 0.25 SD	0.278***	0.074*	0.390***	0.201**
	(0.039)	(0.037)	(0.075)	(0.084)
Mean 0.5 SD	0.255***	0.047	0.340***	0.143
	(0.039)	(0.037)	(0.078)	(0.086)
Lee lower bound	0.197***	0.003	0.060	-0.110
	(0.043)	(0.048)	(0.098)	(0.106)
Lee upper bound	0.312***	0.083**	0.375***	0.148
	(0.041)	(0.040)	(0.088)	(0.093)
Observations	1.077	662	1,077	662
Panel B: Self-reported usage				
	High frequency usage (every day)		Frequency usage score (0-5)	
Mean 0.25 SD	0.196***	0.031	1.126***	0.177
	(0.037)	(0.032)	(0.184)	(0.159)
Mean 0.5 SD	0.218***	0.056*	1.231***	0.300*
	(0.037)	(0.032)	(0.182)	(0.158)
Lee lower bound	0.114***	-0.018	0.792***	-0.0315
	(0.044)	(0.049)	(0.214)	(0.246)
Lee upper bound	0.225***	0.050	1.346***	0.310*
	(0.036)	(0.035)	(0.186)	(0.179)
Observations	1,041	638	1,041	638
Panel C: Predicted actual usage				
	Share of days of usage		Avg daily usage time	
Mean 0.25 SD	0.093***	0.009	26.335***	4.796
	(0.017)	(0.014)	(4.462)	(3.580)
Mean 0.5 SD	0.103***	0.020	28.744***	7.637**
	(0.017)	(0.013)	(4.430)	(3.568)
Lee lower bound	0.051**	-0.009	13.41***	-1.005
	(0.021)	(0.023)	(5.457)	6.272
Lee upper bound	0.115***	0.030*	30.97***	9.751**
	(0.016)	(0.016)	(4.137)	4.054
Observations	1,041	638	1,041	638

Note: Each cell reports ITT estimates of model 1 on the three sub-samples reported in the headings. All outcomes are level variables. 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). In the subsequent lines, we report Lee lower and upper bounds (Lee, 2009) and their respective estimated standard error. No covariates are employed. Standard errors, in parentheses, are clustered by 36 sampling points, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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[56]. 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 an 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)[57] 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

[56] 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.

[57] 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.

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 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.

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			0.095
			(0.086)
Respondent age	0.000	0.003	-0.001
	(0.002)	(0.004)	(0.001)
Live in couple	-0.062	0.049	-0.071**
	(0.087)	(0.121)	(0.027)
HH size	-0.007	-0.019***	0.001
	(0.005)	(0.005)	(0.002)
N. of women in cooking rotation	0.052	0.069	0.001
	(0.032)	(0.042)	(0.009)
Primary school	-0.034	-0.155	0.023
	(0.111)	(0.141)	(0.038)
Secondary school	-0.196*	0.095	-0.037
	(0.097)	(0.168)	(0.047)
Beyond Secondary school	-0.019	-0.058	-0.043
	(0.101)	(0.103)	(0.038)
Have income generating activity	-0.059	0.067	0.019
	(0.060)	(0.113)	(0.031)
Wealth index, all sample	-0.011	-0.028	0.011
	(0.023)	(0.031)	(0.009)
Use saving device	0.076	-0.003	0.031
	(0.073)	(0.095)	(0.036)
Member of informal groups	-0.012	0.076	0.037
	(0.067)	(0.126)	(0.035)
Know ICS	-0.046	-0.164	0.054
	(0.151)	(0.214)	(0.073)
ICS in the HH	0.046	0.126	0.038
	(0.085)	(0.097)	(0.029)
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.

E.2 Measurements of usage

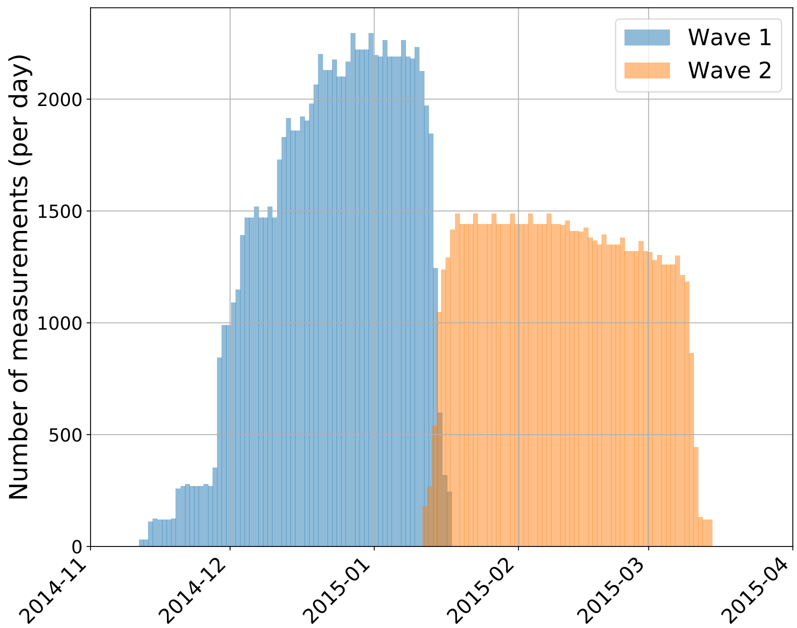
We configured the SUMS so that they would take a measurement every 47 minutes, allowing us to have homogeneous coverage over different times of the day, and allowing their memory, able to hold up to 2048 measurements, to record temperatures for 66 days. Near the end of this period we ran a monitoring 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.

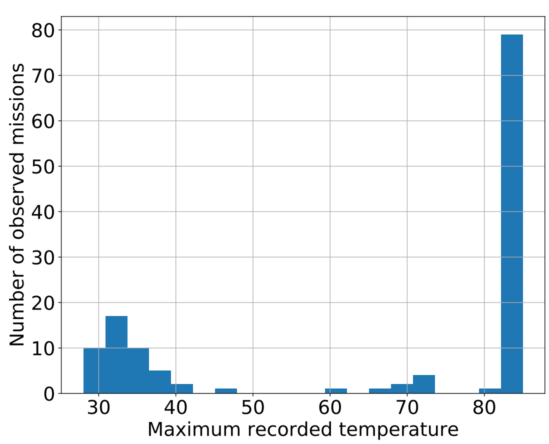
This allows us to count the number of days where at least one usage is made and thus to compute the share of days of usage over the number of days for which measurements were made. We also look at the number of measurements for which temperature is over 50°C. We use these to then compute an "average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

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, which 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 ICSs at the endline with non-missing self-reporting information (N=367)[58].

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 ICSs 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 an ICS is not owned at the endline.

[58]We have missing information on self-reported usage for 9% of women owning ICSs 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	0.329***		70.37***	
	(0.0582)		(22.54)	
daily	0.480***		115.4***	
	(0.104)		(30.62)	
3-4 times/week	0.473***		118.1***	
	(0.106)		(38.61)	
1-2 times/week	0.161*		40.18	
	(0.0897)		(27.49)	
rarely	-0.00596		-8.766	
	(0.0589)		(30.30)	
Frequency usage score (0-5)		0.363***		83.66***
		(0.0829)		(25.24)
Observations	75	75	75	75
Mean Dependent Variable	0.354	0.354	90.20	90.20

Note: All outcomes are level variables. 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 neighbourhoods, 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

As a robustness check, we repeat our test of the direct and indirect effects of the training session (Table 2) on the sub-sample of women who did not own an ICS at the baseline. Results, reported in Table G.3, are qualitatively similar

The ownership of ICSs 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 ICSs 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 (results not shown, but available upon request).

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

	(1) Participants vs non-participants	(2) Non-participants vs control
Respondent age	0.003**	0.001
	(0.001)	(0.002)
Live in couple	-0.096*	-0.071
	(0.049)	(0.063)
HH size	0.007***	-0.006*
	(0.003)	(0.004)
N. of women in cooking rotation	-0.020	0.021
	(0.018)	(0.013)
Primary school	-0.038	0.007
	(0.059)	(0.065)
Secondary school	-0.062	-0.002
	(0.051)	(0.077)
Beyond secondary school	-0.133***	0.033
	(0.046)	(0.045)
Have income generating activity	-0.033	0.004
	(0.041)	(0.049)
Wealth index	-0.029***	0.012
	(0.009)	(0.013)
Use saving device	-0.029	0.076*
	(0.046)	(0.043)
Member of informal groups	0.025	-0.041
	(0.037)	(0.043)
Know ICS	0.025	0.093
	(0.081)	(0.099)
ICS allows to save fuel	0.109**	-0.086
	(0.043)	(0.066)
ICS in the HH	0.044	0.024
	(0.048)	(0.043)
First purchase priority is ICS	0.037	0.071
	(0.041)	(0.048)
Health priority: consequences of IAP	0.005	0.054
	(0.038)	(0.048)
Constant	0.335***	0.745***
Observations	839	587

Note: In column 1 the dependent variable is a dummy for the participation to the training session. The exercise is run on the sample of invited individuals. In column 2 the dependent variable is a dummy for being invited to the training session. The exercise is run on the sample of (invited) non-participants and controls. Standard errors, in parenthesis, are clustered by 36 sampling points, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table G.2: Impact of the training session on ICS ownership and usage, robustness checks with different estimation methods

	(1)	(2)	(3)	(4)	(5)	(6)
	ICS ownership	N. of ICS owned	High frequency usage (every day)	Frequency usage score (0-5)	Share of days of usage	Avg. Daily usage time
Panel A: Whole sample						
Invited	1.032***	0.477***	1.045***	1.099***	0.116***	32.47***
	(0.178)	(0.0954)	(0.210)	(0.195)	(0.0185)	(4.990)
Observations	989	989	953	953	953	953
Method	Probit	Tobit	Probit	Ordered Probit	Tobit	Tobit
Panel B: Non-participants & control						
Invited	0.365*	0.286**	0.442*	0.394*	0.0249	9.113**
	(0.211)	(0.111)	(0.242)	(0.228)	(0.0157)	(4.239)
Observations	587	587	563	563	563	563
Method	Probit	Tobit	Probit	Ordered Probit	Tobit	Tobit

Note: The Table reports estimates of model 1. All outcomes are measured at the endline. The following individual controls are included: 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. Control mean refers to the mean outcome in the control group (non-invited). The sample is restricted to women not owning ICS at the baseline.

Table G.3: Impact of the training session on ICS ownership and usage, robustness checks with sample of women not owning ICS at the baseline

	(1)	(2)	(3)	(4)
Panel A: Ownership	ICS ownership		N. of ICS owned	
Invited	0.317***		0.488***	
	(0.0460)		(0.0868)	
Participated		0.672***		1.033***
		(0.0906)		(0.185)
Observations	797	797	797	797
Panel B: Self-reported usage	High frequency usage (every day)		Frequency usage score (0-5)	
Invited	0.240***		1.394***	
	(0.0474)		(0.237)	
Participated		0.503***		2.927***
		(0.0724)		(0.377)
Observations	769	769	769	769
Panel C: Predicted actual usage	Share of days of usage		Avg daily usage time	
Invited	0.119***		32.22***	
	(0.0189)		(4.945)	
Participated		0.249***		67.64***
		(0.0346)		(8.823)
Observations	769	769	769	769

Note: The Table reports estimates of model 1. All outcomes are measured at the endline. The following individual controls are included: 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. Control mean refers to the mean outcome in the control group (non-invited). The sample is restricted to women not owning ICS at the baseline.

Table G.4: Impact of the training session on ICS ownership and usage, robustness check with covariates

	(1)	(2)	(3)	(4)
Panel A: Ownership	ICS ownership		N. of ICS owned	
Invited	0.311***		0.477***	
	(0.045)		(0.096)	
Participated		0.670***		1.026***
		(0.089)		(0.195)
Observations	989	989	989	989
Control Mean	0.186		0.307	
Panel B: Self-reported usage	High frequency usage (every day)		Frequency usage score (0-5)	
Invited	0.251***		1.390***	
	(0.0442)		(0.209)	
Participated		0.534***		2.962***
		(0.079)		(0.388)
Observations	953	953	953	953
Control Mean	0.131		0.717	
Panel C: Predicted actual usage	Share of days of usage		Avg daily usage time	
Invited	0.116***		32.47***	
	(0.019)		(5.029)	
Participated		0.248***		69.17***
		(0.034)		(9.199)
Observations	953	953	953	953
Control Mean	0.057		13.04	

Note: The Table reports estimates of model 1. All outcomes are measured at the endline. The following individual controls are included: 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 4 are obtained via IV, the remaining via OLS. Invitation to the session is used as instrumental variable for participation. In Panels B and C the sample is restricted to individuals with non-missing self-reported usage. Control mean refers to the mean outcome in the control group (non-invited). Standard errors, in parenthesis, are clustered by 36 sampling points, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table G.5: Effects of invitation on knowledge of ICS, robustness check with covariates

	(1)	(2)	(3)	(4)	(5)
Panel A: ICS general and specific knowledge of its use					
	Δ_t Know ICS	Know where to buy ICS	Correct estimate of fuel saving (20-40%)		
Invited	0.016	0.040	0.093*		
	(0.042)	(0.059)	(0.053)		
Observations	989	989	989		
Control Mean	0.893	0.727	0.213		
Panel B: ICS main features (Δ_t)					
	Expensive	Efficient, allow fuel saving	Good and lasting material, work well	Less smoke, more healthy	Not working well, low quality
Invited	0.121	0.087	-0.140	0.050	-0.016
	(0.076)	(0.075)	(0.094)	(0.051)	(0.016)
Observations	989	989	989	989	989
Control Mean	0.233	0.627	0.420	0.0800	0.0133

Note: The Table reports estimates of model 1. All outcomes in Panel A are measured at the endline. Except for "Know ICS" and outcomes in Panel B which are the difference between endline and baseline values (Δ_t). The following individual controls are included: 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 analysis is performed on the whole non-attrited sample. Control mean refers to the mean outcome in the control group (non-invited). In the case of outcomes expressed in differences, the control mean is the mean in the control group at the endline. Standard errors, in parenthesis, are clustered by 36 sampling points, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table G.6: Welfare impacts, robustness check with covariates

	(1) Δ_t Monthly fuel expenditure	(2) Δ_t Have income generating activity	(3) Δ_t Weekly time working	(4) Δ_t Repondent monthly income
Invited	1,858	-0.019	0.663	-3,688
	(1,568)	(0.078)	(1.513)	(4,878)
Observations	971	989	987	823
Control Mean	11,725	0.493	4.280	14,599

Note: The Table reports estimates of model 1. Outcomes are expressed as the difference between endline and baseline values. Outcomes in columns 1 and 4 are in FCFA. 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. Control mean refers to the mean outcome in the control group(non-invited). Standard errors, in parenthesis, are clustered by 36 sampling points, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table G.7: 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
	(0.029)	(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	-0.063	3.891	-26,589
	(0.018)	(14,316)	(0.565)	(12.16)	(42,709)
		[35.67]	[38.93]	[37.51]	[31.83]
Avg. daily usage	32.47***	-1.828	-0.000	0.014	-95.00
	(5.029)	(50.91)	(0.002)	(0.043)	(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
	(0.045)	(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	-0.022	0.527	-6,908
	(0.096)	(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: 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.8: Effects of information received on peer's purchase on ICS take-up, robustness check with covariates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Purchase or deposit vs no purchase		Purchase vs deposit		Purchase vs no purchase		Deposit vs no purchase		Discussed with other attendants about ICS before Thurs	
β_1: RI*PB=0	0.044		0.006		0.045		0.014		0.082	
	(0.056)		(0.083)		(0.063)		(0.069)		(0.065)	
β_2: RI*PB=1	-0.006		-0.088		-0.004		0.035		-0.146	
	(0.058)		(0.078)		(0.082)		(0.083)		(0.091)	
β_1: RI*PB=0*Unknown		-0.039		0.102		-0.007		-0.051		0.087
		(0.082)		(0.127)		(0.089)		(0.120)		(0.104)
β_2: RI*PB=1*Unknown		-0.058		-0.213*		-0.155		0.029		0.080
		(0.084)		(0.115)		(0.126)		(0.115)		(0.078)
β_3: RI*PB=0*Known		0.094		-0.051		0.076		0.048		-0.124
		(0.069)		(0.099)		(0.084)		(0.083)		(0.114)
β_4: RI*PB=1*Known		0.041		0.036		0.087		0.050		-0.181
		(0.073)		(0.093)		(0.090)		(0.111)		(0.142)
Left deposit									0.164**	0.164**
									(0.072)	(0.073)
Observations	353	353	232	232	253	253	221	221	221	221
$\beta_1 = \beta_2$	0.468	0.863	0.354	0.0513	0.593	0.292	0.824	0.614	0.023	0.146
$\beta_3 = \beta_4$		0.558		0.455		0.924		0.988		0.084
$\beta_2 = \beta_4$		0.346		0.0781		0.0859		0.895		0.748

Note: The table reports OLS estimations of models 2 and 3 in the two columns of each outcome. The outcomes are expressed in levels. RI: "Received Information on peer's purchase"; PB: "Peer bought ICS at the session". The reference category are peer-info control women. The regressions include session fixed effects and the following 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, number of women known in the session. The sample in columns 1 and 2 is formed by women who participated to the training session and who were successfully involved in the final experimental phase (N=353). The samples for columns 3-8 are formed by individuals satisfying the conditions depicted in the headings. The sample in columns 9-10 includes women who did not buy at the training and who received the second visit five days later. Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table G.9: Effects of information received on peer's purchase on ICS take-up, robustness check using alternative estimation methods

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Purchase vs deposit		Purchase vs no purchase		Deposit vs no purchase		Willingness to buy index (0-2)			
	Multinomial logit						Ordered probit		OLS	
β_1: RI*PB=0	0.279		0.367		0.088		0.160		0.102	
	(0.360)		(0.339)		(0.368)		(0.148)		(0.097)	
β_2: RI*PB=1	-0.403		-0.111		0.292		-0.088		-0.051	
	(0.372)		(0.408)		(0.411)		(0.179)		(0.118)	
β_1: RI*PB=0*Unknown		0.991*		0.488		-0.503		0.227		0.140
		(0.546)		(0.485)		(0.590)		(0.226)		(0.146)
β_2: RI*PB=1*Unknown		-1.100**		-0.877		0.222		-0.334		-0.218
		(0.537)		(0.665)		(0.592)		(0.237)		(0.161)
β_3: RI*PB=0*Known		-0.173		0.272		0.446		0.122		0.079
		(0.429)		(0.419)		(0.427)		(0.176)		(0.116)
β_4: RI*PB=1*Known		0.319		0.322		0.004		0.183		0.108
		(0.566)		(0.458)		(0.563)		(0.251)		(0.158)
Observations	353	353	353	353	353	353	353	353	353	353
$\beta_1 = \beta_2$	0.142	0.004	0.299	0.073	0.673	0.358	0.220	0.064	0.256	0.076
$\beta_3 = \beta_4$		0.449		0.929		0.493		0.828		0.870
$\beta_2 = \beta_4$		0.051		0.116		0.776		0.118		0.128

Note: The table reports estimations of models 2 and 3. Columns 1 to 6 are estimated using multinomial logit, columns 7-8 with ordered probit and columns 9-10 with OLS. The dependent variables are buying on the spot vs not buying in columns 1-2; leaving the deposit vs not buying in columns 3-4; and buying on the spot vs leaving the deposit in columns 5-6. In columns 7-10 the outcome is a willingness to buy index, equal to zero for non-purchase, to one for leaving the deposit and to two for purchase at the session. RI: "Received Information on peer's purchase"; PB: "Peer bought ICS at the session". The reference category are peer-info control women. The regressions include session fixed effects but not individual controls. Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table G.10: Effects of information received on peer's purchase on ICS take-up, by peer's relative wealth

	(1) Purchasse or deposit vs no purchase	(2) Purchase vs deposit	(3) Purchase vs no purchase	(4) Deposit vs no purchase
β_1: RI*PB=0*Unknown*Different wealth quartile	0.078	0.098	0.167**	0.002
	(0.072)	(0.117)	(0.083)	(0.102)
β_2: RI*PB=0*Unknown*Same wealth quartile	-0.103	-0.008	-0.142	-0.047
	(0.100)	(0.178)	(0.107)	(0.112)
β_3: RI*PB=0*Known*Different wealth quartile	0.056	0.202	0.059	-0.025
	(0.131)	(0.132)	(0.133)	(0.176)
β_4: RI*PB=0*Known*Same wealth quartile	0.145	-0.185	0.027	0.206
	(0.141)	(0.169)	(0.243)	(0.179)
β_5: RI*PB=1*Unknown*Different wealth quartile	-0.033	-0.182*	-0.103	0.039
	(0.073)	(0.109)	(0.103)	(0.108)
β_6: RI*PB=1*Unknown*Same wealth quartile	0.216***	-0.014	0.170*	0.486***
	(0.077)	(0.125)	(0.102)	(0.154)
β_7: RI*PB=1*Known*Different wealth quartile	0.044	0.152	0.122	-0.146
	(0.131)	(0.149)	(0.143)	(0.156)
β_8: RI*PB=1*Known*Same wealth quartile	-0.204	0.334*	-0.141	-0.123
	(0.250)	(0.182)	(0.261)	(0.102)
Observations	353	232	253	221
$\beta_1 = \beta_2$	0.119	0.604	0.0116	0.731
$\beta_3 = \beta_4$	0.641	0.058	0.907	0.345
$\beta_5 = \beta_6$	0.007	0.268	0.030	0.013
$\beta_7 = \beta_8$	0.374	0.431	0.368	0.897

Note: The table reports OLS estimations of model 3 with an extra interaction with a dummy which is equal to one if the woman and the peer are in the same wealth quartile and zero if they are in different ones. The outcomes are expressed in levels. RI: "Received Information on peer's purchase"; PB: "Peer bought ICS at the session". The reference category are peer-info control women. The regressions include session fixed effects but not individual controls. The sample in column 1 is formed by women who participated to the training session and who were successfully involved in the final experimental phase (N=353). The samples for columns 2-4 are formed by individuals satisfying the conditions depicted in the headings. Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table G.11: Effects of the training session on social interaction concerning ICS, robustness with covariates

	(1) All	(2) Non-part & control	(3) All	(4) Non-part & control	(5) All	(6) Non-part & control
Panel A	Know people owning ICS					
	All		Family and friends		Neighbours	
Invited	0.178**	0.072	0.025	0.006	0.285***	0.146**
	(0.069)	(0.077)	(0.055)	(0.056)	(0.061)	(0.064)
Observations	0.037	0.029	0.025	0.057	0.066	0.036
Control Mean	0.373	0.373	0.307	0.307	0.147	0.147
Panel B	Discussed about ICS with					
	All		Family and friends		Neighbours	
Invited	0.200***	0.004	0.134***	-0.001	0.156***	0.002
	(0.046)	(0.042)	(0.040)	(0.037)	(0.035)	(0.029)
Observations	0.043	0.020	0.035	0.020	0.032	0.016
Control Mean	0.100	0.100	0.073	0.073	0.053	0.053
Panel C	Someone bought ICS after discussion with woman		N. of people who bought ICS after discussion with woman			
Invited	0.125***	0.016	0.428***	0.138		
	(0.029)	(0.027)	(0.085)	(0.083)		
Observations	0.029	0.019	0.021	0.016		
Control Mean	0.033	0.033	0.060	0.060		

Note: The Table reports estimates of model 1. Outcome variables represent the difference between endline and baseline values. Estimates in odd columns are on the whole sample, those in even columns are on the sample of (invited) non-participants and controls. All outcomes are measured at the endline. The regressions include 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, normalised distance from the drop-off point. Standard errors, in parenthesis, are clustered by 36 sampling points, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.