Peer and Social Interaction Effects on the Adoption of Improved Cookstoves: Experimental Evidence from Mali*

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Abstract

Using a randomized experiment, we assess the impact of a training session where information on a more efficient cooking stove (ICS) is provided along with the opportunity to purchase it at market price. We find positive direct and spillover effects of being invited to the session on ICS ownership in 6 to 9 month following our treatment. We also find a positive impact on usage, measured both through self-reporting and with monitoring devices, along the extensive and intensive margin. Our training session appears to have no impact on the knowledge about ICS and any measure of welfare. Within our session, we furthermore randomly assign participants to receive information on a peer's actual purchase or previous ownership and find that women are more likely to adopt the product if the information they receive is on a peer who purchased the product and whose opinion is respected. This allows us to investigate the various mechanisms of social interaction potentially at play and provide evidence supporting imitation effects, rather than social learning or constraint interaction.

Keywords: Technology Adoption, Social Interaction, RCT, Cookstoves, Mali

JEL classification: D91, O33, O13, M31

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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 consequences on the health of households, particularly due to indoor air pollution (IEA, 2016). The Global Burden Disease study estimates that almost four million people die prematurely worldwide 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). Moreover, the extensive use of wood as main energy fuel impacts the local environment, due to deforestation, soil degradation and erosion. In developing regions, such as Africa and South Asia, emissions from cooking stoves account for well over 50% of all anthropogenic sources of particulate matter (UNEP/WMOO, 2011; Bond et al., 2004). For these reasons, access to safe, affordable, reliable, and sustainable energy is one of the goals of the sustainable development agenda. In addition to that, access to inexpensive health-improving technologies can play a role in curbing emissions. One such technology is the improved cookstoves (ICS). They are more energy efficient than traditional cookstoves and can potentially bring about health and efficiency gains through fuel savings. Despite their potential benefits, the take-up and sustained usage of ICS remain low in many contexts (Miller and Mobarak, 2013; Mobarak et al., 2012; Hanna et al., 2016).

In this study, we provide evidence on the impact of a training session where information on ICS is provided along with the opportunity to purchase it at market price. Our study is carried in the context of urban Mali, where most people rely on solid fuels and traditional technologies to cook. In a field experiment run in a controlled setting we assess the impact of the invitation to our training session by comparing ICS take-up of the treated women in each cluster to a control group living in similar areas. We find that being invited to the training session increases the probability of owning ICS by 31 percentage points (160% increase with respect to the baseline value) in the longer run, that is after six to nine months after the treatment. Similarly,we find that our training session has a positive and significant impact on the usage of ICS in the long run. The results on usage rely on both self-reporting (subjective) and monitoring device (objective) measures over the period covering six to nine months post-treatment. The monitoring is done with Stove Usage Monitoring Systems (SUMS) installed on a subsample of ICS. They complement a nascent literature looking at monitored usage (Beltramo et al., 2018).

The success of our intervention in raising ICS take-up at a market price suggests different interpretations apart from the fact that they seem to correspond to local needs and preferences (Lewis and Pattanayak, 2012). First, it may have raised knowledge about the product and its potential benefits. However, this is unlikely as we find that our training sessions did not significantly improve knowledge on ICS. That technology and its potential benefits appeared already to be well known in our context. Second, our intervention may have lowered transaction costs. As we argue below, our design ensured that this was unlikely to be the case. Third, it is possible that people may have limited attention ('scarcity of attention') to the necessity of buying an ICS and that drawing their attention to such a need brings it to the 'top of mind' (Shah et al., 2012; Datta and Mullainathan, 2014). We argue that this factor may be at play in our context but we have no way of disantangling and measuring it.

Within our intervention we investigate an additional effect: that one's decision to adopt and use ICS is influenced by information received on one's social network member's decision to buy or not. One of the contribution of this paper is to study the influence of peers in the context of ICS adoption and usage. We provide empirical evidence from an "information on peer" intervention, whereby a random sample of individuals who attended the training session receive information on a peer living in the neighbourhood. We exploit variations in pairwise relationship[1]. They range from a peer being unknown, known by sight and whose opinion is respected. This additional treatment is implemented the following way: after our training session, where ICS is presented and compared to the traditional technology, a random half of the women within a session are individually invited to purchase the product (or leave a deposit for a purchase in five days' time). Following this, the other half of participants receives an information nudge about another participant's decision to purchase (or previous ownership).

Comparatively, interventions disseminating information about peer behavior have been mostly studied in the form of "social norms marketing" with the aim of teaching people that a certain behavior is more common than they had previously believed and thereby motivating those people to engage more in the behavior themselves. This has been done mainly by providing information about the behavior of a reference group. As a result, in most cases, an individual behavioral shift towards the peer norm is observed. Such designs have been used to study social comparison in the field of public good contribution, energy use and financial decisions by providing information on some reference groups (Frey and Meier, 2004; Chen et al., 2010; Costa and Kahn, 2013; Cai et al., 2015). [2] On that matter our study contributes to a small literature (see Bursztyn et al., 2014) where the information given relates to a particular agent (in our case a neighbour), part of one individual's social

[1]See also Bursztyn et al. (2014) where the purchase decision of a new financial asset by a friend or a family member who is also a customer of the same financial brokerage is randomly communicated in order to identify peer effects and their channels.

[2]In a related literature the information nudge reveals the average or median behavior of a reference group. Among other examples: small charitable donations, towel re-use in hotels, taking petrified wood from a national park and stated intentions to vote (Frey and Meier, 2004; Goldstein et al., 2008; Gerber and Rogers, 2009). Contradicting results are obtained by Beshears et al. (2015) concerning saving decisions.

network or not. After mapping social networks within sampled women of each cluster, we find that individuals who received information about a peer whose opinion is respected and who purchased the ICS had a higher probability to adopt the technology compared to women who did not receive information about a peer.

We also identify and measure positive long run spillover effects of our intervention on ICS adoption for the group of women invited but who did not attend our training session. This is done by comparing these non-compliers to women living in control areas, where no intervention took place. Given the setup of our intervention, which we detail below, these effects are likely the results of social interactions between compliers and non-compliers. Three main mechanisms have been described as drivers of social interaction in technology adoption: social learning, imitation and constraint interaction (Manski, 2000). In our context we can reject constraint interaction. Our results on the welfare impact of ICS adoption and the prevalent level of knowledge of ICS allows us to exclude the social learning mechanism. It is unlikely that non-compliers learned significantly from others about the functioning and the benefits of the technology. First, ICS is a fairly easy to use product, whose functioning and use is similar to the widely diffused traditional cooking stoves. Moreover, the level of knowledge about ICS existence and main attributes is already high among the population at the baseline. We measure the effects of attending the training session on the level of knowledge about the product and find no significant effect. This supports the absence of informational/knowledge gap regarding this product. This tends to rule out that any relevant information which may improve one's knowledge about ICS, and therefore influence its take-up, could be learned from others. Second, we do not find any significant welfare impact of owning ICS (see following paragraph). Hence, benefits from the use of the technology may be hardly perceivable by individual users. It is thus unlikely that social interaction effects are driven by the learning from others about the benefits of the technology. This leaves us with imitation as potential mechanism. We argue below that it may be at play in the longer run, given that we observe such effect in the short run following our information on peer intervention. Overall, our results support the view that the process of technology adoption can be driven by several factors beyond market mechanisms (Foster and Rosenzweig, 2010). Notably, social interaction effects have been found to play a relevant role in the decision to adopt a technology, in several fields such as agriculture, health prevention and financial decisions (Foster and Rosenzweig, 1995; Conley and Udry, 2010; Kremer and Miguel, 2007; Oster and Thornton, 2012; Godlonton and Thornton, 2012; Duflo and Saez, 2003; Beshears et al., 2015; Bursztyn et al., 2014).

Our work also contributes to the debate on the impacts of ICS on welfare-related outcomes (Hanna et al., 2016; Bensch and Peters, 2015; Smith et al., 2011). In line with the review from Bonan et al. (2017), which shows that the impacts from adoption are inconclusive, we do not find a significant beneficial effect of ICS adoption for women on fuel expenditure, time for income generating activities and income. Although the level of usage appears relatively high, we believe that in our context, where meals are prepared for relatively large families with multiple stoves, the substitution of one traditional stove with a more efficient one is not enough to generate significant such welfare benefits and to climb the "energy ladder"[3]. We also contribute to the nascent literature focusing on the barriers to ICS take-up. Among them, liquidity constraints, intra-household preferences, information inefficienceies and marketing strategies seem to play significant roles (Hanna et al., 2016; Miller and Mobarak, 2014; Mobarak et al., 2012; Bensch et al., 2015; Levine et al., 2018; Meredith et al., 2013). We complement existing investigations on the role of opinion leaders and peer effects in the adoption of ICS (Miller and Mobarak, 2014; Beltramo et al., 2015a; Adrianzén, 2014) by exploring the importance of imitation effects.

The rest of the article is organized as follows. Section 2 presents the study design and discusses the experimental design, sample and data collection. In section 3 summary statistics and contextual evidence are first presented, followed by the description of identification strategies and results. Section 4 discusses our identification strategy and some results. Section 5 looks at the possible mechanisms at play. Section 7 concludes.

2 Study design

2.1 Context and background

Over 95% of Malian urban population uses solid fuels (wood, biomass or charcoal) for cooking and only 6% of Malians have access to clean sources of energy (kerosene, gas or electricity); less than 0.5% of the population uses improved biomass cookstoves. It has been estimated that 9,750 deaths per year are caused by indoor air pollution in Mali[4]. Typical traditional wood and charcoal cookstoves are shown in panels *a*, *b* and *c* of Figure 1.

—[Insert Figure 1 here]

In collaboration with the French NGO "Groupe Energies Renouvelables, Environnement et Solidarités" (GERES), we identified an improved charcoal cookstove, locally known as

[3]The idea of an energy ladder implies the movement of households towards 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 through energy stacking, i.e. both modern and traditional fuels and cookstoves, not being mutually exclusive, are used at the same time (Ruiz-Mercado et al., 2011; Masera et al., 2000).

[4]These figures are from the Global Alliance for Clean Cookstoves website cleancookstoves.org/country-profiles/26-mali.html, consulted on January 2017. Mali has a population of around 18 million.

"Fourneau Seiwa". For several years before the launch of this study, GERES supported and supervised the value chain of the product and certified its advantages with laboratory tests[5]. The ICS, shown in panel *d* of Figure 1, is produced by local artisans using recycled materials, is portable, and has a metal structure and a combustion chamber made of baked clay, which allows to retain the heat and save charcoal. The market price of 3,500 CFA (USD 6) is higher than traditional charcoal cookstoves (2,500-3,000 CFA; USD 4.20 - 5). However, it can be recovered in around three months of full usage due to saving in charcoal according to GERES own estimations. The product is available at local markets, but its take-up is relatively low. No other models of ICS with characteristics similar to "Fourneau Seiwa" were available on the market in Bamako at the time of the study.

We provide some background information on cooking activities to better understand the context of the intervention. Cooking is carried out exclusively by women and is one of the activities which is usually organized at the level of the extended household in order to exploit economies of scale. Malian extended households (locally named as *gwa*) can include several nuclear units, living in the same compound[6]. Meals are prepared "centrally" for all members and women who participate in a cooking rotation, in which every day (or week) a different woman has to prepare for the whole *gwa* in turn. In our sample, the woman in the cooking rotation is in charge of preparing between two to three meals per day. On average, about two women are involved in a cooking rotation (62% rotations have only one woman, 20% two, 9% three and 9% four or more).

2.2 Experimental design, sampling and data collection

From October 2014 to January 2015, we conducted a baseline survey of 1080 women from 36 neighbourhood clusters in the city of Bamako. We adopted a clustered, multi-stage, probability sampling in order to have a sample representative of the population of Bamako. We constructed a random selection of 36 cluster areas, from starting points which were randomly identified on the map. From each starting point, 25 contiguous houses were selected following a pseudo-random process *à la* Afrobarometer. From each starting point, another

[5]Experimental tests for the assessment of the ICS performance 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. Similar ICSs have been investigated in other studies in Senegal by Bensch and Peters (2013a) and Bensch and Peters (2015).

[6]Note that multiple *gwa* can be subsumed by an even larger extended household structure, named *du*, but cooking is done at the level of the *gwa*.

5 houses were selected 10 minutes-walk apart, in a random direction, following the same procedure of house selection. The former 25 houses were assigned to the treatment group, whereas the latter 5 to the control group. In each *gwa*, the female head in the cooking rotation was selected as the primary respondent to our survey. On the one hand, the sampling strategy ensures that, within each cluster, treatment and control women live in relatively similar settings. On the other hand, the distance is selected so as to avoid spillover effects from treated to non-treated areas. 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[7].

Nine hundred women in the treatment arm received an invitation to a training session to be held in a nearby school on a Saturday in ten-days time[8]. The hand-delivered invitation flier contained a preview of the topic to be discussed, namely energy efficiency and ICS, the contact details of our field supervisor, the date, time and address of the session. Women were also told that participants would be reimbursed 1,000 CFA for transport costs, i.e. the usual taxi rate for a return trip within the city[9]. One day before the session, all invited women received a reminder call. Sessions were specifically organized to gather women from the same cluster. They were held either in the morning or in the afternoon, lasted about 3 hours and were conducted by a professional product promoter[10]. General information on health, the importance of hygiene while cooking, health consequences of indoor air pollution, efficiency gains and economic advantages (fuel saving, reduced health care need, etc) from using ICSs, how to use and maintain them correctly and their market price were provided. Moreover, the promoter set up a cooking show where the same traditional dish was prepared using both a traditional and an ICS. Before starting, charcoal was publicly weighed, so that fuel saving could be actually verified. After sharing the meal, women were invited one by one, in random order, to another room, where an enumerator proposed the purchase of an ICS at the current market price of 3,500 CFA. Women could decide whether to buy one ICS immediately, to buy it in five days (the next Thursday) by leaving a deposit of 500 CFA, or not to buy. The second option was introduced in order to relieve cash constraints at the time of purchase. Women willing to buy, but without enough cash at the time of the session, had the opportunity to purchase during the home visit by our staff five days later. In case of non-purchase five days after, the deposit was lost.

[7] Population density in Bamako is about 670 inhabitants/km^2.

[8] Saturday was found the day which would maximize the presence of women, based on a dedicated question asked during the pilot phase.

[9] We do not have data on the actual usage of the money provided. However, we do not have anecdotal evidence that people did not use the money for transportation.

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

In 32 out of 36 training sessions[11], a random half of women attending the session were provided information about another random woman in the same session. The content of the information included the peer's identity, her purchase decision and whether she owned already an ICS[12]. This means that women could receive either a positive information, i.e. the peer purchased or previously owned, or a negative one, i.e. the peer did neither purchase nor previously owned ICS. The randomization protocol was incorporated in a software which we designed for data collection and treatment administration through tablets. Figure 2 represents the way different sub-samples were obtained within a representative cluster. 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 proposed the purchase of ICS at the same price conditions. In June 2015, an endline survey was conducted on all women sampled at the baseline.

—[Insert Figure 2 here]

At the time of the invitation to the training session, all women were administered a 40-minutes baseline questionnaire in local language. This baseline survey included questions on demographic composition of the household, socio-economic status, health, education, income, working conditions, time allocation, saving, sources of energy for different purposes, household expenditure on energy, available appliances and cooking stoves (type and fuel used), knowledge about improved cookstoves and participation in informal groups. During the training sessions, we also collected data on the social links among attendants. Each woman was asked whether she knew, at least by sight, each of the other attendants. She was also asked to name a maximum of six women attending the session whose opinion she respected. This allowed us to map the social links for those who attended our training session along three possible intensities of knowledge (or acquaintance): unknown, known by sight and respect opinion. At the endline, in June 2015 (from 6 to 9 months after the baseline), the questionnaire administered was similar to the one administered at the baseline.

A random sample of ICSs sold to participants, both during the experimental session and at the home visit taking place after five days, were equipped with stove usage monitoring systems (SUMS) which would record temperatures over time, and hence allow us to measure usage[13]. This makes such monitoring feasible and reliable on a large number of households

[11] In four sessions our field team faced technical problems with the software for data collection and treatment administration. Women participating to these sessions were exposed to the protocol for "control" individuals, so that none of them received any information nudge about peers. Thus, these sessions are not included in the analysis of social interaction effects during the experimental session, but are part of the study sample for the analysis outside the experimental setting.

[12] We made sure that women leaving the training venue after their purchase decision could not be seen by the other women still sitting at the session space, in order to prevent them from influencing other women and preserve the experimental setting.

[13] Households willing to buy ICS were informed that the product *could* be endowed with the SUMS which

while mitigating the risk of Hawthorne effect, which could arise if measurements were made through frequent households visits. The SUMS we used, iButtons^{TM}, are small sensors, the size of a coin, which can be easily attached to the stove, and which have been used in the literature on cookstoves efficiency (Ruiz-Mercado et al., 2011; Beyene et al., 2015). Figure 3 shows the timeline of our study.

—[Insert Figure 3 here]

3 Data and summary statistics

The whole study sample includes 1077 individuals, 898 of which were invited to the training session, while 179 were not[14]. About 46% of women invited to the session actually attended, with an average of 11 women per session. We define participants to the training session as compliers. We were able to successfully track 989 individuals at the endline (839 in the treated and 150 in the control group, for an overall 8.1% attrition rate).

3.1 Baseline characteristics

Panel A of Table 1 shows baseline characteristics by invitation status, compliance (conditional on invitation) and differences across sub-samples. Respondents are about 33 years old and 88% of them live in married couples within extended households (*gwa*). In our baseline sample the average size of a *gwa* is about 13 members. More than 40% of respondents have no schooling, 15% attended primary school, 11% secondary school and 30% beyond secondary school[15]. Over 43% of women have some income generating activity (mostly in the informal sector), dedicating on average 5 to 6 hours per week to it, and earning a personal income ranging from 16,000 to 20,000 CFA (USD27-34) per month[16]. We compute a wealth index using Principal Component Analysis (PCA) as suggested by Filmer and Pritchett (2001), by aggregating the information on all assets in a single synthetic index[17]. About 30% of

would record temperature for performance and quality tests. We have no anecdotal evidence that these SUMS influenced the purchase decision.

[14] We expected a sample of 1080 and 180 women for the two groups, however we discarded three observations as respondents did not complete the questionnaire or refused to answer to a majority of questions.

[15] 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.

[16] The averages presented for working time and income are for the full sample, not conditional on having work. The monthly income of the head of household ranges between 63,000 and 55,000 CFA for the treated and control groups, respectively (the difference is not statistically significant). The purchase of ICS at the market price of 3,500 CFA would represent 17.5% and 5.6% of women and head monthly incomes respectively.

[17] The wealth index 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

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.

—[Insert Table 1 here]

We find that for more than 80% of households the main fuel for cooking is charcoal, for 19% is wood, while less than 1% use gas as main fuel. All women interviewed, except three, have experience cooking with charcoal and could thus easily use an ICS if given the opportunity. At the baseline, 97% of women declared to own at least one traditional cookstove (on average more than three), 19.7% own ICS (among those, the average is 1.4)[18] and 50% own at least one small gas stove, typically used for quick heating, like water for baths or to heat up leftovers[19]. Overall, we find about four stoves per *gwa*. 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 *gwa*. 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 *gwa* 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 *gwa*.

The ICS or "Fourneau Seiwa" used in our intervention, is known by the vast majority of women surveyed (91 to 94%). More than 75% of women correctly attribute characteristics related to efficiency and fuel-saving to ICS. The main source of knowledge about the product comes from members of women's social network (family, friends or neighbours) who own one. This was mentioned by 61% of women, followed by market (56%) and promotional campaigns in the media (34%)[20]. We asked women to list some positive and negative characteristics of the ICS. The majority of respondents mentioned features related to efficiency and savings in fuel (77%), while other focused on quality and durability (37%) and health (16%). The most prominent negative aspects were the lack of durability (54%) and the high price (16%)[21].

categorical variables expressing house facilities such as type of roof, floor, toilet and water 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.

[18] Given the large variety of traditional models available in the market and the 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.

[19] 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.

[20] None of these variables are significantly different for invited and non-invited.

[21] During the study period, 13% of stoves acquired by women not owning one before have been found

The main reasons for not owning an ICS are related to the difficulty to finding them (39%) and their high price (31%). However, the average estimated price of ICS reported by women was 4,700 CFA, an amount above the actual market price of 3,500 CFA.

The lack of significant differences between invited and non-invited women along most of the observable baseline characteristics in Panel A of Table 1 leads us to believe that the randomization exercise was successful[22]. Participation to the session is the outcome of a self-selection process. Compliers were, on average, significantly older, living in larger *gwa*, less educated and less wealthy than those who did not attend. As far as ICS are concerned, compliers were not more likely to own or know about the existence of ICS than non-compliers. However, the former are more likely to know more about the efficiency features of ICS than the latter.

3.2 Outcomes

Panel B of Table 1 reports the short and long term outcomes of the training session as. One can notice that the difference in ICS ownership between invited and non-invited women was not statistically different at the baseline (20.3% and 17.3%, respectively). About 17% and 14% of women invited to the session eventually purchased ICS on Saturday and Thursday, respectively. This leads to a 31% overall purchase rate at the end of the intervention. At the endline, we find that the share of households owning ICS increases by 26 percentage points (significant at 1% level) with respect to the control group. Similarly, the number of ICS in the *gwa* moves from about 0.3 at the baseline, the difference between invited and non-invited not being significant, to 0.6 in the treated group at the endline, implying a 100% increase.

A set of variables relating to ICS knowledge is based on questions only asked at the endline. We observe that neither the knowledge about where one can buy ICS, nor knowledge of their technical efficiency vary significantly between the sample of invited and non-invited women[23]. Finally, we ask the respondents if they know other people owning ICS. This is the

partially or completely broken at the endline.

[22] The Table only report a sub-set of baseline characteristics which are employed as controls in the regression analysis which follows. However, we performed difference of means tests over 117 baseline characteristics, with a rejection rate of 12, 2.5 and 0% of the variables for the invitation treatment corresponding to the significance levels of 10, 5 and 1%, respectively. In the non-attrited sample the rates are 11, 7 and 0%. Results are available upon request.

[23] In order to construct the first variable, women were asked to list places where ICS could be purchased within the city. Locations were checked and the variable takes the value of 1 if ICS were actually sold in the reported locations and 0 otherwise. For the second variable 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 takes the value of 1 if the estimated saving are in a reasonably correct range (20% to 40%) and 0 otherwise. An index for ICS knowledge is constructed summing the following dummy variables: knowledge of ICS existence, knowledge of its main features related to efficiency (the previous questions were also asked at the

case for more people in the group of invited than in the control group. While no difference in the number of family members of friends owning ICS is found between the two samples, we find a significant difference in the number of neighbours owning ICS.

As far as monitoring data on usage are concerned, in 17 out of 36 clusters, SUMS were randomly attached to 100 ICS, out of the 282 sold during the intervention. We were able to successfully track 75 of them[24]. 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%). Analysis of attrition and of sample representativeness are presented in Appendix C. Overall, we show that this sample of 75 is representative of women who purchased ICS at our session. We configured the SUMS so that they would take a measurement every 47 minutes, allowing us to have an homogeneous coverage over different times of the day, and allowing their memory, able to hold up to 2048 measurements, to record temperatures for 66 days. When this period was close to end for many of the SUMS released, that is around mid January 2015, a monitoring pass was ran, so that 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: Figure C.1 in the appendix C summarizes their timing. Different algorithms have been proposed in the literature to convert temperature measurements from SUMS into usage statistics: our approach draws inspiration 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 both the extensive and intensive margin for ICS usage. Details on the procedure are provided in appendix C. Table 2, Panel A, reports the descriptive statistics of usage variables from SUMS. We find that about 73% (55 out of 75) of women use ICS at least once. ICS is used, on average, in 35% of the days of monitoring. If ever used, an ICS is used, on an average day, for 267 minutes (more than four hours), during more than 2 cooking events which last about one hour and a half each.

Panel B of Table 2 reports the descriptive statistics based on self-reported measures of usage. We focus on the non-attrited sample of women who owned ICS at the endline with non-missing self-reporting information (N=367)[25]. About three fourth of women owning ICS at the endline reported to use it daily (49% for every meal, 27% at least once a day)[26],

baseline), knowledge of where one can buy ICS and correct estimate of expected saving. The score ranges between 0 and 4 and is calculated only at the endline on the non-attrited sample.

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

[25] We have non-missing information on self-reported usage for 91% of women owning ICS at the endline. The analysis of missing data is done in appendix C.

[26] In the analysis which follows, a dummy variable indicating at least daily usage is employed.

about 10% use it from one to four times a week, 9.5% declared to use it rarely and 5% never. We construct a self-reported ICS usage score by assigning the values 1, 0.5, 0.25, 0.1, 0 for the reported frequencies "always" (or daily), 3-4 times/week, 1-2 times/week, rarely, never, respectively. In Appendix C.2 we show that self-reported measures are good predictors of actual usage, as monitored through SUMs. From such regressions, we generate out-of-sample predictions of effective usage, along both the extensive and intensive margin, for the entire population owning ICS at the endline and reporting its usage. These variables are employed to corroborate the evidence of the impact of the training session on ICS usage. Descriptive statistics of both self-reported and predicted ICS usage at the endline for different experimental samples are reported at the bottom of table 1.

—[Insert Table 2 here]

4 Direct and indirect effects of the training session

We investigate the extent to which our training session directly and indirectly influences women's take-up decision and ICS usage. For the direct effects, we look at the Intention to Treat Effect (ITT) of the invitation to the training session and the Local Average Treatment Effect (LATE) of participating the session on the whole study sample. In order to measure the indirect or spillover effects in clustered randomized trials, the literature suggests to compare the outcomes of non-treated individuals in treated clusters, with non-treated individuals in non-treated clusters (see, among others, Baird et al., 2017). This is not possible in our context, as we do not dispose of data on (randomly) non-invited women in treated areas. Instead, we consider various samples: 1) women who did not participate in the training session (non-compliers) and women in control areas and 2) women who participated in the training session (compliers) but did not buy an ICS and women in control areas. We are aware of the fact that the former group may be formed as an outcome of a self-selection process. We run the following reduced-form on our various samples:

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

where β_1 provides the ITT of our intervention on ICS ownership and usage. Tables in this section show the results for ICS ownership at the endline and usage, respectively, for different sub-samples, reported on the column headings[27].

[27]The results shown use clustered standard errors at the level of the 36 sampling points. However, they are not affected when we consider invited and non-invited areas separately, therefore using 72 clusters. A formal LR test of multi-level model vs simple linear regression never rejects the null that that the between-subject variation is zero. All results presented below are also robust to the use of wild-bootstrapped clustered

4.1 ICS Ownership

Columns 1 and 5 of Table 3 refer to the whole non-attrited sample of the study and shows that being invited to the training session increases the likelihood of owning ICS by 31 percentage points (this represents a 160% increase with respect to the baseline value) and the number of ICS owned by 0.5 units (156% increase). Columns 2 and 6 show the Local Average Treatment Effect (LATE) of participating to the session. This is obtained through IV, where participation is instrumented by invitation to the session [28]. We find that participating to the training session increases the likelihood of owning ICS by 67 percentage points and the number of ICS owned by about 1 unit on the population of compliers. These represent a 290% and 320% increase with respect to the baseline values, respectively. In columns 3 and 7 we repeat the exercise on the sample of women who have been invited but who did not participate and the non-invited who form the control group. We find that non-participants are about 9% more likely to own ICS and own 0.3 more ICS than control women at the endline. This represents suggestive evidence of spillover effects which we discuss below in more detail. We think these results provide suggestive evidence of underestimated spillover effects from our treatment. Non-compliers self-selected not to attend but do not differ from control women under most of baseline characteristics, as reported in column 1 of Table E.1 [29]. In particular, we find no evidence that the group of non-participants could potentially have a higher demand of ICS, hence would be negatively selected. They do not own more ICS or know more about them than participants at the baseline. Moreover, when asked about which item a woman would prioritize if she had the possibility to buy, among an established list of kitchen tools (fridge, gaz stove, pots and ICS), no significant difference in the share of women preferring ICS arises at the baseline between participants and non-participants. Similarly, when asked to rank health problems in a list 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 a first priority problem in both groups, the difference between them being not significant. Columns 4 and 8 of Table 3 show that women who participated to the training sessions but who did not buy an ICS (neither on the spot nor with a 5-day delay) are still more likely to own ICS and own more ICS at the endline than women in control areas. This represents some evidence that the indirect effect of the training session beyond the time window of our intervention.

—[Insert Table 3 here]

standard errors (Cameron et al., 2008). Results are not reported, but are available upon request.

[28] In the first step, not reported, invitation strongly predicts participation to the session, with Cragg-Donald Wald F-statistics which exceeds 95 throughout specifications.

[29] Non-compliers live in significantly smaller gwa than control women.

4.2 ICS Usage

Table 4 reports the effects of the invitation to the training session on self-reported usage (Panel A) and predicted actual usage (Panel B). The results found on ownership are confirmed for most of usage outcomes. We find that invited women are 25 percentage points more likely to use ICS every day than control ones (column 1, Panel A). This represents a 148% increase with respect to the control group. The effect doubles for women who participated to the training session (column 2, Panel A). Non-compliers and non-buying compliers are 8 and 17 percentage points more likely to use ICS every day than control women, respectively (columns 3 and 4, Panel A), which represent 61 and 130% increase with respect to the control group. Results show the same pattern when we use the frequency usage score in columns 5 to 8 of Panel A. As far as predicted actual usage is concerned, we find that the share of days of ICS usage increased by 12 percentage points (200% increase) as the effect of being invited to the training session (column 1, Panel B) and by 25 percentage points for participating, with respect to the control group (column 2, Panel B). Non-compliers and non-buying compliers use ICS 2 and 8 percentage points more frequently than control women, respectively. However, only the latter is statistically significant (columns 3 and 4, Panel B). Positive and strongly significant effects are also found along the intensive margin. Invited and complier women use ICS for 32 and 69 minutes per day more than control ones, respectively (columns 5 and 6, Panel B). These represent 246 and 530% increase with respect to the control group. Non-compliers and non-buying compliers use ICS for 9 and 21 minutes per day more than the control group, respectively (columns 7 and 8, Panel B).

—[Insert Table 4 here]

We have to be cautious as the results presented in the current and previous sub-section have some limitations. First, differential attrition rates for the group of invited and non-invited women. We deal with that by checking the sensitivity of our results to different assumptions on the distribution of treatment effects among attritors, following Karlan and Valdivia (2011) and by estimating Lee bounds (Lee, 2009). Results are presented and discussed in Appendix B. We find that, while the overall effect of the training session on the whole sample are robust to different missing data scenarios, results on both the sample of "non-compliers and controls" and "non-buying compliers and controls" do not remain significant as the assumptions on attriters become more severe for outcomes related to ICS ownership. Results on usage appear more robust to severe assumptions on attrition than the ones on ownership. Second, the group of non-participants received an invitation to the training session, while the control group did not. This may have generated some salience effect on purchase. However, our questionnaire, administered to control and treated women equally, largely inquired about households cooking habits and included detailed questions on ICS. Cookstoves were mentioned as the main focus in the introduction of our survey and about 30% of survey questions were related to topics including cooking, kitchen, stoves and fuel. Given that, we believe that the impact of receiving an invitation to the training session via our flier at the end of the survey was negligible compared to the effect of the questionnaire, if there was any[30].

5 Possible mechanisms

In this section we discuss a number of potential reasons why our invitation and our training session have had a significant impact on ICS ownership and usage. The four sub-sections which follow explore more direct effects coming from our training session while the fifth one investigate indirect effects over the longer run.

5.1 Transaction costs

One can argue that our training sessions may have lowered the transaction costs for ICS purchase and thus help take-up. During our sessions women could buy an ICS at the market price with no need to go to the market. However, the large share, if not all, of the women we interviewed visit their local market, where ICS are available, on an almost daily basis. Furthermore, the distance between each woman's home and the market and home and the place where we had our training session are not significantly different across our various clusters. Home delivery was not offered for women who purchased an ICS on a Saturday. For women who left a deposit on a Saturday, they were expecting a visit from our enumerators at their home on the following Thursday. However, home delivery was not mentioned or offered to women leaving a deposit during the training sessions. Any 'home delivery effect' is thus unlikely to have played a role in their decision. Furthermore, given that each ICS weighs about five kilograms, most sellers in local markets to whom we discussed offer home delivery if requested by a client within reasonable distance. Participation to the session was subsidized, in the form of a reimbursement for transportation fees of 1,000 CFA. One could also see this as show-up fee for the time dedicated to the session (which lasted about three hours). No other form of incentive was introduced during the data collection process. As we highlight above, we do not have data on the actual usage of the money provided nor do

[30] The flier contained the following text "You are kindly invited to participate at a training session on improved cookstoves and their benefits. This is organized by a team of researchers in economics 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. For any question, please contact *details of the manager*".

we have anecdotal evidence that people did not use the money for transportation. For these reasons we think that transaction costs are unlikely to have played a significant role in the decision to purchase an ICS within our intervention.

5.2 Lack of information and salience

Arguably, an important reason for offering training sessions is to decrease any information gap on the existence of the product and the characteristics and benefits of usage. In the context of ICS, Levine et al. (2018) find that a free trial period significantly increases ICS take-up, while in Beltramo et al. (2015b) marketing messages conveying the benefits of ICS had no effect on willingness to pay for it. More in general, information dissemination interventions do not unambiguously lead to higher technology adoption (Meredith et al., 2013; Ashraf et al., 2013). Table 5 shows the ITT effects of the invitation to the training session on four variables related to the knowledge about ICS and on a score obtained from the sum of them.

—[Insert Table 5 here]

Regressions include the whole sample of women both invited and non-invited women (control group). For the first two columns both baseline and endline measures are available. We thus estimate the change over time for them; outcomes in columns 3 to 5 are based on measures taken at the endline only. We do not find any significant effect on the knowledge of ICS in column 1. The variable 'Knows ICS' is simply a dummy indicating whether an individual knows they exist. Similarly we find no significant impact on the dummy indicating whether an individual agrees or not with the fact that ICS is a more efficient technology compared to the traditional cookstoves (column 2) and whether a women gave the correct estimate of potential fuel savings by using an ICS compared to a traditional cookstove (column 4). Women do not seem to benefit from being invited to the training session on improving their knowledge on where ICS can be bought (column 3). The lack of significant effect persists when we consider the aggregate index of knowledge (column 5). Further results, not shown, indicate that there is no significant heterogeneous effects with respect to education levels (primary school, secondary school and beyond secondary school) from the invitation on the five knowledge variables we use. Overall our invitation does not seem to significantly raise knowledge on ICS. We attribute these results to the fact that ICS appear at the baseline to be widely known in the context where we sampled the individuals for our study and thus there was most likely very little information gap in the first place.

We further test if there is any heterogeneous effects of the invitation with respect to the baseline knowledge variables described above on ICS purchase during our intervention and ICS ownership at the endline. This exercise is conducted by estimating specification 1 in column 1 of Table 3 along the baseline values of the dummy variables "know ICS" and "Agree with ICS is efficient, allows to save fuel" (these are the only variables, among the five dependent variables in 5, for which we have baseline observation). Results are reported in Panel A of Table 6. The Table reports the coefficients of the interaction between invitation to the session and the variables reported. Each coefficient is obtained from separate regressions, except for the three dummy variables related to schooling which are estimated simultaneously. We find no evidence that the training session leads to higher ICS take-up or ownership at the endline for women who were less knowledgeable about ICS at the baseline. We do find heterogeneous effects along the baseline knowledge of ICS efficiency. In particular, knowledgeable women are more likely to purchase ICS during our intervention (Saturday and Thursday), while, on average, less knowledgeable women tend to own more ICS at the endline. This unexpected results is likely to be due to non-compliers. Some of them end up owning ICS at the endline (see section 4.1) and non-compliers are significantly less likely to know about ICS efficiency features than compliers (see bottom line of Panel A of Table 1). On the whole these findings does not allow us to clearly contradict our claim that the session appear not bridge an informational gap.

—[Insert Table 6 here]

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'. Evidence shows that intervention leveraging on salience are likely to impact individuals with limited attention, commitment problems and mental constraints (Hanna et al., 2014; Datta and Mullainathan, 2014). In the absence of proxies of such measures in our survey, we test the heterogeneous effect of the invitation, along the level of education of the respondent as shown in Panel A. We find some indirect evidence of that. The take-up during our intervention is higher for less educated women, in particular for those with no schooling and primary degree as compared to women who have the highest level of schooling. Similarly, women with primary level of schooling are significantly more likely to own ICS at the endline.

5.3 Liquidity constraints and intra-household bargaining

Within our treatment women could decide whether to buy one ICS immediately or to buy it in five days (the next Thursday) by leaving a deposit of 500 CFA during the training session. This option was introduced in order to potentially relieve cash constraints. Women willing to buy, but without enough cash at the time of the session, had the opportunity to purchase during our staff visit at their home five days later. This delay was thought to potentially help in raising money in the following five days or allow for discussing the purchase with their spouse. It aimed at alleviating liquidity constraints and helping with potential intra-household bargaining constraint. Liquidity constraints have been documented to be relevant barriers to ICS adoption (Bensch et al., 2015; Mobarak et al., 2012; Levine et al., 2018). Intra-household bargaining constraint also appear to represent relevant barriers when preferences towards the new technology differ between the male and the woman in the household (Miller and Mobarak, 2013; Meredith et al., 2013).

In Panel B and C of Table 6 we look at the heterogeneous effects of the invitation on ICS purchase after the intervention (both Saturday and Thursday) and ownership at the endline along a set of variables used as proxies for liquidity constraints and intra-household bargaining power. The table reports the coefficients of the interaction between invitation to the session and the variables reported. Each coefficient is obtained from separate regressions. In Panel B, we find no effect on average along the following dimensions: wealth, individual income and use of a saving device. Similarly there is no effect if we use quartiles of the income distribution (results not shown). In Panel C, we find no differential effect of our intervention neither on ICS take-up, nor on ownership at the endline, along the following dimensions: living in couple, being the household head, getting married before the age of 16, age difference with the partner, difference in years of schooling and individual income as a share of the sum of the pooled household income (husband and wife). It appears from these results that liquidity constraint and intra-household bargaining played on the whole no significant role in the decision to take-up.

5.4 Peer effects

Our project also focused on investigating the impact of how one's decision to purchase or not a new technology can be influenced by her peers' decision. Within a Saturday training session and towards its end, half of the attending women were randomly assigned to a treatment group (the "peer info treatment group") and the other half to the control group (the "peer info control group"). Women assigned to the peer info control group were invited individually to decide on whether of not to purchase an ICS (or to leave a deposit) without the others knowledge. Then each woman randomly assigned to the peer info treatment group was also individually invited to make a similar decision but was given information about the ownership (measured at baseline) and purchase decision of ICS at the session of another randomly selected woman from the peer info control group. The experiment involved

353 women in 32 sessions[31]. These purchase decision were made in a separate room or an area on our sites which allowed for privacy. Table D.1, in the appendix, shows that the two experimental groups are similar under most of the observable characteristics considered throughout the analysis and suggests that the randomization exercise was successful.

We evaluate the effect of receiving this information nudge, by estimating the following equation on the sample of participants:

$$Y_i = \beta_0 + \beta_1 RI_i \times PO = 0_j + \beta_2 RI_i \times PO = 1_j + \gamma X_i + \lambda Z_i + \epsilon_i \quad (2)$$

where Y_i is the outcome of woman i, it takes value one if she buys on the Saturday and zero otherwise. RI_i is a dummy which is equal to one if the woman received the information nudge and zero otherwise. PO_j represents the content of the information provided and is expressed as a dummy. It takes value one if the peer j owned before or bought ICS at the session, or both. β_1 captures the impact of receiving information on the peer when such information is negative (no take-up or ownership), while β_2 captures the relative effect of positive information on the peer's take-up decision (the exact information provided states if an ICS was owned previously, was purchased at the session or both). X_i is a vector of individual baseline characteristics including age, marital status, size of *gwa*, dummies for education levels, participation to informal groups, having an income generating activity, any saving, an index for wealth, knowing about and already owning an ICS, normalized distance from the drop-off point, the number of people known by sight and whose opinion is respected present at the session. Z_i is a vector of session controls we discuss below. These results are shown in the first two columns of table 7.

We then assess the extent to which the intensity of the linkage with a matched peer influences individual decision to take-up, by extending our model and including two additional terms: $RI_i * PO = 0$ and $RI_i * PO = 1$ respectively interacted with $Respectspeer'sopinion_i j$. This dummy takes value one if i respects j's opinion (zero otherwise) or put differently if the treated woman i respect the randomly selected peer j' opinion. These results are shown in the last two columns of table 7. The analysis is conducted on the sample of women who participated to the training session using OLS with robust standard errors[32].

In Table 7 we find that on the whole the simple fact of receiving information on ICS ownership does not affect ICS take-up during the experimental session. This is due to the fact that the sum of the two coefficients in column 1 is not statistically different from zero.

[31] 14 out of 367 attendants did not participate to the final phase and were not included in the analysis of the treatment. More details on such cases and attrition are discussed in Appendix B and are taken into consideration in the regression analysis.

[32] Being the treatment assigned at the individual level within session, no clustered standard errors are applied. The results are similar when using probit or logit.

On average, women receiving information about a peer who purchased or already owned ICS are 6 percentage points more likely to purchase ICS, however the coefficient is not statistically significant (column 1).

When we look at the different level of peer's social connectedness, we find that women are significantly more likely to take up ICS when the peer they receive information about owns or adopts ICS and her opinion is respected (column 3). This suggests that positive information may be more salient than negative information, contrary to what Miller and Mobarak (2014) find in their study.

—[Insert Table 7 here]

One can argue that there can be a dimension of network endogeneity due to assortativity along unobservables. For instance, women with modern attitude can be simultaneously more socially connected and more likely to adopt ICS. This can be erroneously attributed to peer effects. However, table D.1 shows that the average number of women whose opinion is respected in a session is balanced across peer info treatment and control group. With that respect, both groups appear to have similar peer connections within a session. Furthermore, this variable, together with the number of women known by sight, are included as controls in the regressions. Table D.1 also shows that both peer info treatment and control group appear similar in terms of their modern attitude: both show non-significantly different means of suitable proxies: ICS ownership at the baseline and knowing about ICS. It could also be the case that clusters/sessions may have specific features which affect both participation and take-up decision, such that, for example, in a session where most participants are interested in ICS, treated participants would be more likely to receive a positive peer information. We deal with this problem by adding session controls, namely the number of participants, the share of ICS owners, the share of those knowing about the existence of ICS and a measure of connectivity in a session [33]. We add those session controls in the models shown in columns 2 and 4. The point estimate of the interaction effect of interest in column 4 slightly decreases and remains significant at 10% significance level[34].

Given our experimental design the results in table D.1 focus on the decision to purchase on Saturday only. This way we can assess the short term impact of our informational nudge within a controlled setting. If we define our dependent variable as the decision to purchase either on Saturday or Thursday the effect we highlight above become insignificant. We can think of two reasons explaining this: 1) the information given may have a very short term

[33] Connectivity is measured as the density of the social network of knowledge (where a link is considered present when reported by at least one of the two parties).

[34] We also tested by adding cluster fixed effects (by adding a seires of 31 dummies): while the point estimate remains at the similar level of 0.22, the standard error increases, leading to a p-value of 0.19. Results are available upon request.

impact and does not prolong over five days until the following Thursday and 2) during the five days following out treatment other factors outside our controlled setting may play a significant role among which we can think of intra-household bargaining. Moreover, as we explain above, within each session each woman was asked whether she knew, at least by sight, each of the other attendants. Each was also asked to name a maximum of six women attending the session whose opinion she respected. So we can categorize the social links between each i and j along three possible intensities of knowledge (or acquaintance): unknown, known by sight and respect opinion. Table D.1 only reports the impact of respecting one peer's opinion. Additional estimations show that whether i knows j only by sight does not significantly impact on the decision to purchase on Saturday. Our results show that peer effect only becomes significant when the intensity of the link between them is at the highest intensity. Furthermore, whether i knows j only by sight does provide an ambiguous metric for the intensity of knowledge: one can be known by sight for positive or negative reasons. Our analysis focuses on the highest intensity as its interpretation is compelling.

5.5 Social interactions

Our training sessions were an opportunity for discussing about ICS and, as we can see from our previous results, they generated some interest in the product leading to higher take-up. Table 8 shows that women in our session did not only get information on ICS but also on who owns one. The share of women knowing someone owning an ICS increases by 18 percentage points as an effect of the session (Column 1). This variable is only measured at the endline and the mean of the control group is 0.37. In a follow-up multiple choice question we inquire whether these people were members of the family, friends or neighbours. We use this information to build the outcome variables "Know people owning ICS: family or friend" and "Know people owning ICS: neighbours" used in models 3-4 and 5-6 respectively. The result, for the whole sample, seems to be driven by the number of people owning ICS among neighbours (column 5), rather than among friends and relatives (column 3). We also explore the extent to which this effects is present for invited women who did not attend the sessions and the control group. While we find positive but non-significant effects on average (Column 2), we do find that non-compliers are around 15 percentage points more likely to know neighbours owning ICS than control women, the difference being significant at 95% confidence level.

According to (Manski, 2000), social interaction effects can occur through three main channels: expectation, preference and constraint interaction. The first channel is defined as expectation interaction or social learning (henceforth social learning). It occurs when people learn from the actions chosen or outcomes experienced by others. People may thus learn more about benefits and value of a product (Conley and Udry, 2010; Kremer and Miguel, 2007; Devoto et al., 2012); or about how to use it (Munshi and Myaux, 2006; Oster and Thornton, 2012; Cai et al., 2015). The second channel is broadly defined as social utility, imitation effect, conformism, herd behavior or social contagion (henceforth imitation effect). It comes into play when people's preferences are influenced by other individuals' decisions[35]. The third channel, constraint interaction, derives from positive and negative externalities arising from individual decisions which, together with peers' decision, jointly affect the set of feasible actions for all. A small number of recent studies have tried to identify imitation effects, disentangling them from social learning. Bursztyn et al. (2014) directly investigate the first two channels by providing social connected pairs (friends or family members) either information about peer's intention to purchase a new financial asset and his actual capability to own it (which was randomized) or peer's intention to purchase only. They find that both social learning and imitation effects are economically significant. Bernard and Torero (2015) find evidence of interaction effects in the decision to connect to electricity in rural Ethiopia and, by excluding the social learning channel, conclude that preference interaction offers the most reasonable explanation. Our research setting does not allow us to clearly disentangle which channel is responsible for the social interaction effects in ICS diffusion. However, the peculiarities of the product, our context and additional evidence presented below lead us to speculate that the imitation effect is the prevailing explanation for the effects we observe.

The particular type of product employed in the study has some key characteristics that make it different from other technologies which have been investigated in relation to social interaction effects. First, ICS is already an established technology which is known by a large majority of women in the population (93.6% know about it at the baseline). Its design and usage are similar to the traditional charcoal stove which is widely used and as such do not imply significant behavioral changes, adjustment in one's cooking technique or important informational gaps to be filled on how to use the technology. Of all the 1078 women involved in our study, only three had no previous experience in cooking with a tradional charcoal stove. In this regard, ICS is different from, for example, index insurance, menstrual cups and contraceptives where social learning has been shown to be a major driver of adoption (Cai et al., 2015; Oster and Thornton, 2012; Munshi and Myaux, 2006). Second, ICS is a relatively cheap and risk-free technology, which implies relatively little investment or risks. This is widely different from the case of adoption of new seeds or agricultural practices, which entail important risks and changes on fundamental sources of livelihood.

[35]Related to financial decisions this mechanism has been empirically investigated by Bursztyn et al. (2014) and Beshears et al. (2015), while in agricultural technology adoption by Bandiera and Rasul (2006).

In Table 5 we do not find any significant effect of the trianing session on the knowledge of ICS existence, its main advantage (efficient use of combustible) and knowledge on where ICS can be bought. Although we cannot completely rule out that the treatment effect is not mediated by information, we could not find any survey measure of knowledge and information which reacted to the treatment.

We further consider the lack of perceived benefits from the use of the technology. In many studies, social interaction works through social learning about the benefits of technologies such as deworming pills, anti-malaria bednets or agricultural innovations or piped water connection (Kremer and Miguel, 2007; Dupas, 2014; Conley and Udry, 2010; Maertens, 2017).

We test the impact of our intervention on a set of variables which are expected to be influenced by the introduction of ICS: fuel expenditure, income generating activities, time allocation, monthly income. The first dimension is related to the fuel efficiency features of ICS, while the intuition behind the remaining ones is that a more efficient cookstove would affect the amount time spent for meal preparation.

As a first step, we estimate the reduced form, i.e. the impact of being invited to the session on the differences in outcomes between the endline and the baseline values. Results are reported in Table 9. The exercise includes the entire study population. The invitation to training session does not seem to have any significant impact on overall monthly fuel expenditure[36], propensity to have an income generating activity, time spent for income generating activities or monthly income[37].

Several reasons can explain these findings. First, despite the positive results of our intervention in increasing ICS ownership and usage, documented in section **??**, ICSs keep being a relatively minority in the overall set of stoves within enlarged households. In particular, the number of ICS as a share of total number of ICS in the *gwa* grows from about 7% at the baseline (not statistically different between treatment and control groups) to about 12% at the endline in the treatment group (the difference being significant with p-value<0.01). One needs to keep in mind that meals are prepared for large families and often require the use of several cookstoves at the time. In fact, only 13% of women owning ICS declared to exclusively use ICS, while 71% of women declare to use other traditional stoves. As a further check of this interpretation, we estimate the impact of ICS ownership after the training

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

[37]For some variables the sample size is reduced due to missing data. We do not find any systematic pattern in missing observations with respect 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.

session, ICS usage and ownership at the endline on welfare outcomes. We use the invitation to the training session as instrumental variable. First stage results are reported in column 1 of Table E.2 (Cragg-Donald Wald tests always reject the hypothesis that the instrument is weak) The results, shown in Table E.2, confirm the null effects reported in Table 9.

Moreover, ICS ownership may not necessarily imply exclusive or continuous usage. Such 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 to use ICS for every meal, 23% daily, 15% 1 to 4 times per week[38]. Adding to that, we find above in Table **??** that for around 27% of ICS on which a SUMS was attached we do not have a single measurement indicating that those ICS have not been used. In order to have a sizeable impact, besides being regularly used and well maintained, ICS should completely replace traditional stoves. It is well documented that energy transition are often carried out through energy stacking, i.e. both modern and traditional fuels and cookstoves are used simultaneously (Ruiz-Mercado et al., 2011; Masera et al., 2000).

One can argue that non-significant coefficients may be due to lack of statistical power. However, power calculation suggests that our design (considering the invitation to the training as the treatment) is powered to detect 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)[39]. These are commonly considered as small effects. For example, in the case of fuel expenditure, our design would allow to detect a minimum of 15% reduction, which is about half the minimum level of efficiency gains measured by GERES through laboratory tests.

Overall, the evidence suggests that ICS ownership does not bring perceivable improvements in women's welfare and lifestyle. If that is the case we would assume that women are unlikely to share a positive experience or perceived benefits from the use of this technology with their neighbours. This in turns makes social learning improbable as the main mechanism driving the process of diffusion of ICS through social networks.

Imitation effects seem at work and several reasons may drive an individual to mimic peers' behavior. People may think others' behavior reflects private and valuable information they do not have, therefore leading them to imitate the others, regardless of the private information or preference (Banerjee, 1992; Bikhchandani et al., 1992). Alternatively, people may interpret others' decisions as part of a social norm, to which they should conform to (Munshi and Myaux, 2006). This may be due to taste for social status, fear of sanctions, social identity, or relative level of consumption preferences (Bernheim, 1994; Akerlof, 1980;

[38]In Bensch and Peters (2013b), the authors find similar self-reported usage in urban Senegal, for an ICS type comparable to the one in this study.

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

Benjamin et al., 2010; Abel, 1990; Luttmer, 2005; Fafchamps and Shilpi, 2008; Bursztyn et al., 2017)[40]. Fear of sanctions and social identity may appear unfit to our context, unfortunately our data do not allow us to pinpoint which one(s) of the remaining mechanism(s) is driving our results.

The third channel mentioned by Manski (2000) as responsible of social interaction effects relates to constraint interactions. Several contextual reasons suggest that this channel is unlikely to be relevant and that general equilibrium effects are negligible. First, our intervention involves a small sample of the overall city population (of about 1.9 million people) and is spread across a vast area (the city extends over an area of 245 km^2). Then, the large change in ICS ownership is mainly driven by our training intervention where ICS was directly sold. Given that we procured our stock of ICS directly at producers, it is unlikely that our intervention may have caused any spillover (or shortage) on the availability of ICS in the city or in its price.

6 Further issues

6.1 Robustness checks

One can argue that the decision to adopt and use ICS may be different for women already owning an ICS at the baseline. As a robustness check, we repeat the exercise above on the sub-sample of women who did not own ICS at the baseline. Results, reported in Table E.3 and E.4, are qualitatively similar. The ownership of ICS at the baseline did not seem to affect the decision to participate to the training session either, as shown in Table 1.

Another issue concerns the identity of the respondent. In about 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, a woman of another nuclear household of the same *gwa* or a daughter). This may lead to biased estimates if the new respondent has a different informational set compared to the original one. Although it is unlikely that the new respondent is unaware of the presence of ICS at *gwa* 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.

[40]See Cialdini and Goldstein (2004) for a review of social influence in psychology.

6.2 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 cost 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.

7 Conclusion

Our work provides experimental evidence on the effect of peers and social interaction on the adoption and usage of a more efficient technology for cooking in a context of energy poverty. Our study is innovative in that it provides evidence of both peers and social interaction effects on technology adoption and usage in an urban context of a developing economy, a still under-researched area. We find that individuals are influenced by information received on the purchase decision or ownership of another peer living within the same cluster. In the short-run, during the Saturday sessions, such influence manifests itself via ICS take-up if the peer matched has been listed among those whose opinion is respected. Several months following our training sessions, so in the longer run, we also find evidence of social interaction effects in the natural diffusion of this new technology several. By using additional results, on knowledge of ICS and various welfare variables, we are able to rule out social learning and conclude that this impact is most likely driven by imitation effects.

The study provides some policy implications. First, technology diffusion may spread naturally through social interactions. However, depending on the prevalent mechanism in place and on the characteristics of the technology, different policies can be implemented to speed up the process of adoption. In our case or the ones of other technologies which show similar characteristics: easy to use, relatively cheap and similar to the traditional or already used ones, the focus should not be on informational campaign, but more on direct market penetration, possibly lowering through transaction costs. We find a large effect of our session on take-up, but not because it fills an informational gap. Indeed, we observe a wide knowledge of the product and its attributes already at the baseline. Instead, the intervention offers the purchase of an ICS at market price and this seems to help in giving salience to the product in women's mind. Given that imitation seems to play a role in the diffusion and usage, marketing strategies which target trustworthy and respected individuals in communities and neighbourhoods are most likely to meet with success. In order to generate significant welfare impacts on population coping with energy poverty, interventions should consider carefully the local context. First, by designing ICS models to make them fit local tastes and second in considering cooking habits. The latter relates particularly to the practice of energy stacking, which requires greater efforts to reach successfull energy transition.

8 Figures and Tables

Figure 1: Different models of cookstoves used

(a) Traditional three stone stove (using wood)

(b) Traditional metal stove (using wood)

(c) Traditional metal stove (using charcoal)

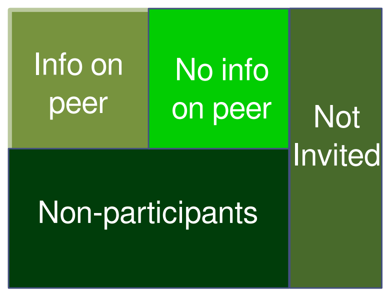
(d) Improved cookstove (using charcoal)

Figure 2: Experimental design

(a) A given cluster

(b) Random assignment of invitations to attend our training session on a Saturday

(c) Partial compliance in participation to our training session



(d) Random assignment of our treatment giving information on a peer's decision to purchase/own

Figure 3: Timeline of the study

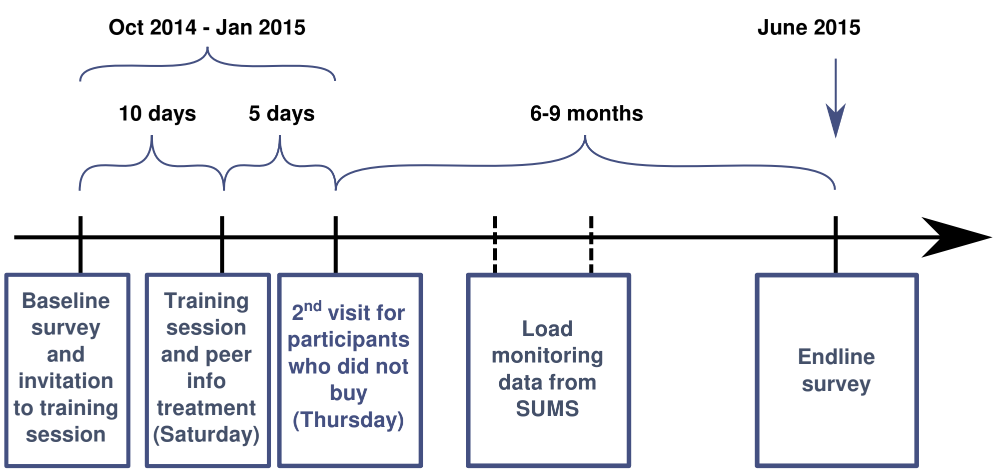


Table 1: Summary statistics and sample balance related to the invitation and participation to the training session

	Invited	Non invited	P-value of diff	Complier	Non complier	P-value of diff
N. of observations	898	179		415	483	
Participated to training session (compliers)	0.462					
Endline survey not administered	0.066	0.162	0.000	0.031	0.095	0.000
Panel A: Baseline characteristics						
Respondent age	33.2	32.2	0.298	34.3	32.3	0.011
Live in couple	0.873	0.894	0.431	0.860	0.884	0.280
Size of gwa	12.836	13.073	0.721	13.458	12.302	0.042
N. of women in cooking rotation	1.818	1.771	0.687	1.827	1.812	0.866
No schooling	0.438	0.408	0.453	0.496	0.387	0.001
Primary school	0.147	0.151	0.877	0.149	0.145	0.834
Secondary school	0.109	0.128	0.445	0.104	0.114	0.611
Secondary school or above	0.306	0.313	0.844	0.251	0.354	0.001
Have income generating activity	0.455	0.436	0.616	0.448	0.462	0.672
Weekly time working (hours)	6.373	5.056	0.108	5.853	6.822	0.157
Repondent monthly income (CFA)	19753	16538	0.258	17922	21323	0.154
Wealth index	0.014	-0.068	0.632	-0.255	0.245	0.001
Monthly fuel expenditure gwa (CFA)	13414	13574	0.851	13347	13471	0.853
Use saving device	0.323	0.274	0.192	0.316	0.329	0.653
Member of informal groups	0.546	0.536	0.803	0.542	0.549	0.829
ICS in the gwa	0.203	0.173	0.359	0.195	0.209	0.593
N. of ICS in the gwa	0.323	0.318	0.926	0.301	0.342	0.427
N. of stoves in the gwa	4.371	4.425	0.811	4.284	4.445	0.409
Know ICS	0.941	0.916	0.209	0.949	0.934	0.315
Agrees with "ICS allows save fuel"	0.786	0.760	0.426	0.831	0.747	0.002
Panel B: Outcomes						
Purchase ICS at the session (Sat)	0.175			0.378		
Purchase ICS at Thurs visit	0.141			0.306		
Purchase ICS after intervention (Sat+Thur)	0.314			0.680		
Owns ICS at the endline^a	0.447	0.187	0.000	0.662	0.249	0.000
N. ICS owned at the endline^a	0.611	0.307	0.000	0.818	0.421	0.000
Know where one can buy ICS^a	0.751	0.727	0.519	0.731	0.769	0.205
=1 if 20-40% saving from ICS usage^a	0.241	0.213	0.458	0.284	0.201	0.005
ICS knowledge score (0-4)^a	2.741	2.553	0.017	2.913	2.584	0.000
Know people owning ICS: all^a	0.535	0.373	0.000	0.682	0.400	0.000
Know people owning ICS: family or friends^a	0.293	0.307	0.725	0.366	0.227	0.000
Know people owning ICS: neigbours^a	0.448	0.147	0.000	0.612	0.297	0.000
Reported high frequency usage (every day)^a	0.309	0.127	0.000	0.468	0.162	0.000
Frequency usage score (0-5)^a	1.789	0.693	0.000	2.734	0.920	0.000
Share of days of usage (predicted)^a	0.155	0.057	0.000	0.234	0.081	0.000
Avg daily usage time (predicted)^a	39.624	13.039	0.000	59.229	21.332	0.000

Note: Values reported refer to the whole sample (36 clusters), ^{a} refers to variables measured only at the endline on the non-attrited sample (N=989).

Table 2: ICS usage summary statistics

	N	mean	sd	min	max
Panel A: Monitored ICS usage					
Extensive margin:					
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
Intensive margin:					
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 3: Direct and spillover effects of the training session on ICS ownership at the endline

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ICS ownership				N. of ICS owned			
	All		Non-compliers & control	Non-buying compliers & control	All		Non-compliers & control	Non-buying compliers & control
Invited	0.311***		0.0886*	0.208***	0.477***		0.286**	0.351***
	(0.0449)		(0.0447)	(0.0590)	(0.0961)		(0.112)	(0.106)
Participated		0.670***				1.026***		
		(0.0888)				(0.195)		
Constant	-0.0541	-0.00167	0.0544	0.0884	-0.219	-0.138	0.0205	-0.0829
	(0.0899)	(0.0882)	(0.0890)	(0.170)	(0.167)	(0.178)	(0.216)	(0.312)
Observations	989	989	587	277	989	989	587	277
R-squared	0.150	0.264	0.218	0.235	0.170	0.136	0.181	0.234

Note: Standard errors, in parenthesis, are clustered by 36 sampling points, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. All outcomes are measured at the endline. Individual controls include age, marital status, size of *gwa*, number of women in the cooking rotation, dummies for education levels, participation to informal groups, has an income generating activity, use saving device, an index for wealth, knows about ICS, owns an ICS at baseline, normalized distance from the drop-off point. Specifications in columns 2 and 6 are obtained via IV, while the remaining are OLS. Compliers are those who participated in the training session. The sample "All" is formed by all non-attriter women (both invited and non-invited to the training session). "Non-compliers & control" includes invited women who did not attend the training and non-invited ones. "Non-buying compliers & control" includes attending women who did not buy and non-invited ones.

Table 4: Direct and spillover effects of the training session on ICS usage at the endline

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	High frequency usage (every day)				Frequency usage score (0-5)			
Panel A: Self-reported usage	All		Non-compliers & control	Non-buying compliers & control	All		Non-compliers & control	Non-buying compliers & control
Invited	0.251***		0.0760**	0.175***	1.390***		0.377**	0.949***
	(0.0444)		(0.0372)	(0.0471)	(0.218)		(0.185)	(0.235)
Paricipated		0.534***				2.962***		
		(0.0792)				(0.388)		
Constant	-0.120	-0.0738	0.0809	-0.00593	-0.453	-0.196	0.389	0.404
	(0.0939)	(0.0927)	(0.0913)	(0.134)	(0.461)	(0.448)	(0.453)	(0.669)
Observations	953	953	563	268	953	953	563	268
R-squared	0.138	0.220	0.202	0.267	0.156	0.282	0.233	0.294
	Share of days of usage				Avg daily usage time			
Panel B: Predicted actual usage	All		Non-compliers & control	Non-buying compliers & control	All		Non-compliers & control	Non-buying compliers & control
Invited	0.116***		0.0249	0.0833***	32.47***		9.113**	21.40***
	(0.0187)		(0.0159)	(0.0250)	(5.029)		(4.297)	(6.255)
Paricipated		0.248***				69.17***		
		(0.0344)				(9.199)		
Constant	-0.0117	0.00984	0.0560	0.0929	-6.351	-0.353	8.866	21.50
	(0.0461)	(0.0438)	(0.0452)	(0.0746)	(12.25)	(11.71)	(12.13)	(19.94)
Observations	953	953	563	268	953	953	563	268
R-squared	0.190	0.296	0.244	0.316	0.214	0.289	0.247	0.310

See notes Table 3. The sample is restricted to individuals with non-missing self-reported usage.

Table 5: Effects of invitation on knowledge of ICS

	(1) Δ_t Knows ICS	(2) Δ_t Agrees with “ICS efficient”	(3) Knows where to buy ICS	(4) Correct estimate of fuel saving (20-40%)	(5) ICS knowledge score (0-4)
Invited	0.0504	0.0808	-0.0219	0.0568	0.164
	(0.0803)	(0.122)	(0.0912)	(0.108)	(0.186)
Constant	0.0818	-0.141	0.564***	0.215**	2.213***
	(0.0720)	(0.110)	(0.0945)	(0.0943)	(0.232)
Observations	989	989	989	989	989
R-squared	0.028	0.026	0.025	0.012	0.030
Controls	Yes	Yes	Yes	Yes	Yes
Mean Dependent Variable	0.937	0.793	0.727	0.213	2.553

Note: Standard errors, in parenthesis, are clustered by 36 sampling points, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The tables report the coefficient of the interaction between invitation to the session and the variables reported. The outcomes in columns 1 and 2 are calculated as difference between endline and baseline values. Outcomes in columns 3 to 8 are only measured at the endline. Individual controls as in table 3. The omitted education category is "High school or above". Mean dependent variable for columns 1-2 and 3-11 is the unconditional mean for the control group at the baseline and endline, respectively. The analysis is performed on the whole non-attrited sample.

Table 6: Heterogeneous effects on ICS take-up and ownership

	(1) Purchase ICS following intervention (Sat+Thur)	(2) Owns ICS at endline
Panel A: Information and saliance		
Know ICS	0.0103	-0.0858
	(0.0786)	(0.105)
Agree with 'ICS is efficient'	0.0838*	-0.131*
	(0.0440)	(0.0691)
Primary school	0.0331	0.167*
	(0.0547)	(0.0930)
Secondary school	-0.0647	-0.0608
	(0.0598)	(0.141)
Beyond Sec school	-0.0957**	-0.0815
	(0.0420)	(0.0988)
Panel B: Liquidity constraints		
Wealth index	-0.00222	0.0196
	(0.00877)	(0.0171)
Individual income, 10KCFA	-0.00230	0.0242
	(0.00715)	(0.0172)
Has saving	-0.0362	-0.0770
	(0.0404)	(0.0777)
Panel C: Intra-household bargaining		
Live in couple	-0.00580	-0.134
	(0.0593)	(0.0893)
Ind. is head of hh	0.00603	0.0106
	(0.0352)	(0.0641)
Got married before 16	0.0246	0.0223
	(0.0462)	(0.0930)
Ind. income as a % of pooled income	-0.0216	0.0841
	(0.108)	(0.220)
Age difference with partner	0.00203	0.000580
	(0.00482)	(0.00938)
Difference in years of schooling	0.00490	0.00881
	(0.00622)	(0.0109)

Note: Standard errors, in parenthesis, are clustered by 36 sampling points, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The tables report the coefficient of the interaction between invitation to the session and the variables reported. Each coefficient is obtained from separate regressions, except for the three related to schooling in Panel A which are estimated simultaneously. Individual controls as in table 3. The analysis is performed on the whole non-attrited sample.

Table 7: Effects of information received on peer's purchase on ICS take-up

	(1)	(2)	(3)	(4)
	Purchase ICS at the session			
RI * PO=0	-0.0205	0.0102	-0.00300	0.0282
	(0.0569)	(0.0566)	(0.0678)	(0.0680)
RI * PO=1	0.0661	-0.0199	-0.00744	-0.0874
	(0.0755)	(0.0753)	(0.0897)	(0.0898)
RI * PO=0 * Respects peer's opinion			-0.0556	-0.0578
			(0.104)	(0.106)
RI * PO=1 * Respects peer's opinion			0.277*	0.256*
			(0.163)	(0.155)
Constant	0.139	-0.871	0.137	-0.843
	(0.172)	(0.611)	(0.172)	(0.609)
Observations	353	353	353	353
R-squared	0.112	0.168	0.119	0.174
Ind. Controls	Yes	Yes	Yes	Yes
Session Controls	Yes	Yes	No	Yes

Note: Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. RI: "Received Information on peer's purchase"; PO: "Peer owned or bought ICS at the session". Individual controls include age, marital status, size of *gwa*, n. of women in the cooking rotation, dummies for education levels, participation to informal groups, has an income generating activity, use saving device, an index for wealth, knows about ICS, owns an ICS at baseline, normalized distance from the drop-off point, number of women known by sight and number of women whose opinion is respected in the session. Session controls include number of participants, average share of ICS ownership and knowledge about ICS, network centrality measure. The sample is formed by women who participated to the training session and who were successfully involved in the final experimental phase (N=353).

Table 8: Direct and spillover effects of the training session on knowledge of other people with ICS

	(1)	(2)	(3)	(4)	(5)	(6)
	Knows people owning ICS		Knows people owning ICS: family or friends		Knows people owning ICS: neigbours	
	All	Non-compliers & control	All	Non-compliers & control	All	Non-compliers & control
Invited	0.178**	0.0717	0.0255	0.00572	0.285***	0.146**
	(0.0695)	(0.0767)	(0.0547)	(0.0557)	(0.0605)	(0.0642)
Constant	0.155	0.227	0.0809	0.0945	0.0269	0.187
	(0.123)	(0.156)	(0.106)	(0.112)	(0.112)	(0.134)
Observations	989	587	989	587	989	587
R-squared	0.037	0.029	0.025	0.057	0.066	0.036

Note: See notes Table 3. All outcomes are measured at the endline.

Table 9: Welfare impacts, ITT estimates

	(1) Δ_tMonthly fuel expenditure	(2) Δ_tHas income generating activity	(3) Δ_tWeekly time working	(4) Δ_tIndividual monthly income
Invited at the training session	-3,268	0.00366	-2.733	-7,760
	(2,839)	(0.130)	(2.448)	(8,314)
Constant	6,003*	0.285**	-2.455	7,327
	(3,221)	(0.116)	(3.067)	(7,924)
Observations	971	989	987	823
R-squared	0.032	0.396	0.159	0.206
Controls	Yes	Yes	Yes	Yes
Mean Dependent Variable	13,659	0.453	5.409	15,732

Note: Standard errors, in parenthesis, are clustered by 36 sampling points, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Individual controls as in table 3.

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Appendix

A Sampling design and survey protocols

A.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. 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 subsample. 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, we are confident enough in our selection process to assume that 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. 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 Cartesian coordinates. First, a ruler with numbers on each dimension side is overlaid over 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.

A.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 outside of one cell and of the entire grid. In particular, this method also ensures that households which are part of the control sample fall within the same cell (as described in the next paragraph).

Once the initial direction is chosen we select 25 contiguous, inhabited compounds on either side of the walking direction. Each are assigned alpha-numeric IDs. Half of the households are selected from the right hand side and the other half from the left hand side. On each side, 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 25 households are selected for our treatment sample, we proceed to select those for the control sample. From the initial point 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 five new households which are part of the control sample. Once again, an alpha-numeric numbering system is used for these five 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.[41] As an added benefit, treated and control points in each clusters are visited in the same week, hence controlling for any time-specific phenomena which might affect specific parts of the city.

[41] 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.

A.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 with the woman/en responsible for the cooking rotation (the women 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 handed out. The sessions are held in a venue in the neighbourhood and women are told that they will receive 1,000 CFA if they show up for covering their transportation fees. For the control sample, no invitation is given to the interviewees.

If the targeted individual is not at home, the enumerator inquire about an approximate time when she will be home and return then for the interview. The enumerator can also request the phone number of that individual and ask to speak to her for an appointment. After two unsuccessful attempts to contact the selected individual within the household, a replacement procedure kicks in. A household is then replaced by the first available compound right at the end of the others according to the selection procedure outlined above.

A.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 *gwa* who is knowledgeable about the cooking rotation. Following this, our team of enumerators administer the follow-up questionnaire.

B 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 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[42]). This seems to be the outcome of small sample size and relatively high attrition in few control clusters[43]. 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 and a few cases of death. According to column 1 of table B.1, attriters and non-attriters appear as balanced samples along almost all observable characteristics.

In four out of thirty six training sessions, the peer information treatment was not implemented due to technical issues. These sessions were concentrated on a few successive dates and in a particular geographic area (commune 5). Such loss of data do not represent a threat to the internal validity of the experiment related to the peer information treatment because these sessions are not included in the sample for the relevant estimations. However, it is possible that such exclusion has an impact on the external validity. Column 2 of table B.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 correlate with the outcome variable reported in table 7 (results not shown but available upon request).

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 B.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.

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

[43]It can be shown that excluding 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).

To take into account the extent to which differential attrition has an impact on the internal validity of results in Tables 3 and 4, we run sensitivity analysis to different data missing scenarios. Following Karlan and Valdivia (2011), we create two scenarios where control group attriters are imputed the non-attriter control group mean plus 0.25 or 0.5 standard deviations of the found control distribution; 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 found treatment distribution. We also implement Lee bounds (Lee, 2009), where bounds are estimated by trimming a share of the sample, either from above or from below. We report ITT estimates for model estimated in Tables 3 and 4 for the different sub-samples of interest in Table B.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-compliers 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 compliers and control (columns 3 and 6) are robust to the imputation exercise, both with 0.25 and 0.5 SD imputations, but the Lee lower bound is not statistically significant[44].

[44]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. One should notice that point estimates in specifications without controls are always lower than the ones shown in Tables 3 and 4 and are always included in in the Lee bounds ranges.

Table B.1: Attrition analysis

	(1) Pr(Attriter) [whole sample]	(2) Pr(Compiler 32 sessions) [Compilers 36 sessions]	(3) Pr(Complier reaches final phase) [Compliers 32 sessions]
Invited at the training session	-0.0787*		
	(0.0414)		
Respondent age	-1.65e-05	0.00298**	0.00192**
	(0.000800)	(0.00124)	(0.000759)
Live in couple	-0.00242	0.0144	0.0413
	(0.0227)	(0.0462)	(0.0374)
Size of gwa	-0.00291*	-0.000246	-0.00190
	(0.00148)	(0.00266)	(0.00178)
N. of women in cooking rotation	0.00665	0.0200*	0.00497
	(0.00772)	(0.0118)	(0.00947)
Primary school	-0.0157	0.0939**	0.0123
	(0.0245)	(0.0403)	(0.0291)
Secondary school	0.00273	0.0149	0.0199
	(0.0251)	(0.0607)	(0.0355)
Secondary school or above	-0.0229	0.0973**	0.0167
	(0.0239)	(0.0401)	(0.0258)
Have income generating activity	-0.0167	-0.00644	-0.0163
	(0.0198)	(0.0389)	(0.0289)
Wealth index, all sample	0.00219	-0.00543	-0.00500
	(0.00500)	(0.00801)	(0.00604)
Use saving device	-0.0214	0.0568	-0.00880
	(0.0183)	(0.0423)	(0.0292)
Member of informal groups	-0.0157	-0.00304	0.0336
	(0.0228)	(0.0363)	(0.0301)
Know ICS	0.00592	-0.0808	0.154*
	(0.0308)	(0.0557)	(0.0907)
Improved coal stove in the gwa	0.0228	-0.0991**	0.00738
	(0.0253)	(0.0497)	(0.0268)
Distance from drop-off point	0.0162	0.0449	0.00118
	(0.0121)	(0.0349)	(0.0133)
Constant	0.181***	0.742***	0.710***
	(0.0590)	(0.0908)	(0.113)
Observations	1,077	415	367
R-squared	0.032	0.059	0.074
Mean Dependent Variable	.081	.884	.962

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

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

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

C ICS usage

C.1 Sampling and attrition

SUMs were installed on a random sub-sample (about 36%) of ICS that were sold at the training sessions on Saturday and also during our Thursday visits. In order to check the representativeness of the actually monitored sample, we look at the determinants (along the observable baseline characteristics used throughout the analysis) of the probability of purchasing a ICS on which is installed an SUMS out of all ICS bought on either Saturdays or Thursdays. This is done in column 1 of Table C.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)[45] 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), unable 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 to high temperature was used to secure SUMS to the stove. In that, we followed the guidelines of our SUMs reseller (Berkley Air Monitoring Group) and the best practices from other studies. However, differently from many of those studies, the particular model of ICS we consider is portable and suitable for both indoor and outdoor cooking. As such, it is often moved from one place to another. This makes SUMS particularly vulnerable to blows and scratching, which may cause their damage or loss. Column 2 of table C.1 reports the determinants of owning an ICS with a SUMS from which data were collected from 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 C.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.

[45]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 SUM in a given wave.

Table C.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 ported) [ICS owned at endline]
Invited at the training session			0.0956
			(0.0858)
Respondent age	5.10e-05	0.00339	-0.000815
	(0.00236)	(0.00379)	(0.00137)
Live in couple	-0.0625	0.0491	-0.0715**
	(0.0868)	(0.121)	(0.0272)
Size of gwa	-0.00756	-0.0193***	0.000876
	(0.00491)	(0.00535)	(0.00209)
N. of women in cooking rotation	0.0520	0.0689	0.00108
	(0.0323)	(0.0418)	(0.00948)
Primary school	-0.0339	-0.155	0.0231
	(0.111)	(0.141)	(0.0381)
Secondary school	-0.196*	0.0952	-0.0372
	(0.0973)	(0.168)	(0.0469)
Secondary school or above	-0.0198	-0.0583	-0.0427
	(0.101)	(0.103)	(0.0386)
Have income generating activity	-0.0596	0.0668	0.0187
	(0.0600)	(0.113)	(0.0315)
Wealth index, all sample	-0.0115	-0.0277	0.0111
	(0.0229)	(0.0314)	(0.00976)
Use saving device	0.0764	-0.00317	0.0311
	(0.0731)	(0.0948)	(0.0358)
Member of informal groups	-0.0124	0.0759	0.0375
	(0.0668)	(0.126)	(0.0349)
Know ICS	-0.0466	-0.164	0.0544
	(0.151)	(0.214)	(0.0727)
Improved coal stove in the gwa	0.0458	0.126	0.0377
	(0.0855)	(0.0966)	(0.0289)
Constant	0.500**	0.806***	0.806***
	(0.217)	(0.222)	(0.141)
Observations	275	100	403
R-squared	0.041	0.132	0.047
Mean Dependent Variable	0.364	0.750	0.911

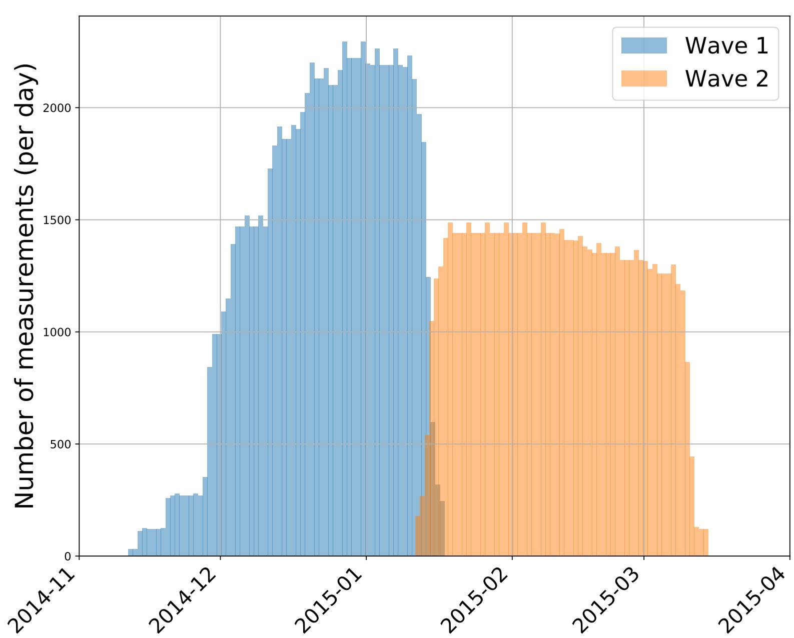
Standard errors, in parentheses, are clustered by 36 sampling points, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Eech column re the sample used in square brackets.

C.2 Measurements of usage

Figure C.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 C.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.

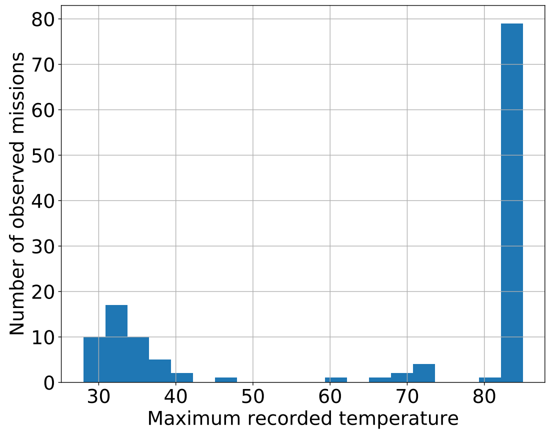
Figure C.1: Timing of our two waves of temperature measurements



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.

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 from the overall number of days where measurements were made. This definition measures cookstove usage at the *extensive* margin. We also derive an *intensive* measure by looking 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 2 reports the descriptive statistics of monitored usage over the monitoring period.

Figure C.2: Peak and average temperatures



Note: maximum temperature reached during each mission.

C.3 Monitored vs self-reported usage

Table C.2 shows the results of a set of regressions where the dependent variables are the share of days of usage and the average time of usage. We use the available measures of self-reported usage, namely the six dummies obtained from the questionnaire as regressors and the self-reported usage score, together with the usual controls used throughout the paper. We find that reported usage significantly predicts monitored usage throughout the models. We use the estimated coefficients of models 1 and 3, the ones with higher explanatory power, to make out-of-sample linear predictions of effective usage, for the whole population of women owning ICS at the baseline. We correct the predicted values as follows: negative shares and time are transformed into zero and we set the variables to be equal to zero when ICS is not owned at the endline. Descriptive statistics of the two variables are presented in table 1.

Table C.2: Monitored vs self-reported usage

	(1)	(2)	(3)	(4)
	Share of days of usage		Avg daily usage time (mins)	
Frequency of ICS use:				
always	0.337***		67.46**	
	(0.0769)		(26.39)	
daily	0.486***		113.2***	
	(0.107)		(29.81)	
3-4 times/week	0.491***		118.7**	
	(0.133)		(48.55)	
1-2 times/week	0.174*		37.76	
	(0.0909)		(31.29)	
rarely	0.00968		-10.08	
	(0.0590)		(31.66)	
Frequency usage score (0-5)		0.348***		78.96***
		(0.0766)		(22.94)
Constant	0.0573	0.114	14.80	24.14
	(0.196)	(0.140)	(43.61)	(40.69)
Observations	75	75	75	75
R-squared	0.439	0.312	0.315	0.228
Mean dependent var	0.354	0.355	90.20	90.21
Controls	Yes	Yes	Yes	Yes

Note: Standard errors, in parenthesis, are clustered by 36 sampling points, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Individual controls as in table 3.

D Descriptive statistics for the peer information treatment

In the 32 training sessions where the informational treatment was implemented, 164 women (47%) received the peer information treatment, while 189 (53%) did not[46]. Table D.1, Panel A, shows characteristics for women who participated in the training session for both the treated and the control sample. The lack of significant differences along most of the observable baseline characteristics leads us to believe that the randomization exercise was successful [47] The outcome of the peer information treatment is reported in Panel B: about 38% and 36% of women in the peer information and in the control group, respectively, purchase ICS at the end of the Saturday session. The difference is not statistically significant.

[46]The main reason of the unbalance in the number of women in the two groups is due to the fact that in sessions where the number of participants is odd, the non-treated group is larger by one unit.

[47]The tables only report a sub-set of baseline characteristics which are employed as controls in the regression analysis. However, we performed difference of means tests over 117 baseline characteristics, with a rejection rate of 6.8, 5.9 and 2.5% corresponding to the significance levels of 10, 5 and 1%, respectively.Results are available upon request.

Table D.1: Summary statistics and sample balance related to the treatment giving info on a peer's ICS ownership or decision to purchase

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

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

E Further results and robustness checks

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

	(1)	(2)
	Non-complers & control	Invited Non-buying compliers & control
Respondent age	0.000844	0.00311
	(0.00172)	(0.00271)
Live in couple	-0.0714	-0.202**
	(0.0633)	(0.0941)
Size of gwa	-0.00642*	-0.00194
	(0.00352)	(0.00621)
N. of women in cooking rotation	0.0214	0.0314
	(0.0133)	(0.0298)
Primary school	0.00725	-0.100
	(0.0654)	(0.0788)
Secondary school	-0.00158	-0.0477
	(0.0773)	(0.103)
High-school or above	0.0331	0.0114
	(0.0454)	(0.0758)
Have income generating activity	0.00411	0.0565
	(0.0491)	(0.0773)
Wealth index, all sample	0.0117	-0.0470***
	(0.0127)	(0.0171)
Personal savings	0.0756*	0.0555
	(0.0425)	(0.0775)
Member of informal groups	-0.0411	-0.0969
	(0.0431)	(0.0635)
Know ICS	0.0931	0.344**
	(0.0995)	(0.145)
Agrees with "ICS allows save fuel"	-0.0855	-0.0401
	(0.0658)	(0.0991)
ICS in the gwa	0.0238	0.125
	(0.0434)	(0.0821)
First purchase priority is ICS	0.0712	0.141*
	(0.0483)	(0.0725)
Health priority: consequences of IAP	0.0539	0.0266
	(0.0483)	(0.0855)
Constant	0.745***	0.148
	(0.124)	(0.203)
Observations	587	277
R-squared	0.035	0.107

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

Table E.2: Welfare impacts, LATE estimates

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

Standard errors, in parenthesis, are clustered by 36 sampling points, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Individual controls as in table 3. 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.

Table E.3: Direct and spillover effects of the training session on ICS ownership, sample of women not owning ICS at the baseline

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ICS ownership				N. of ICS owned			
	All		Non-compliers & control	Non-buying compliers & control	All		Non-compliers & control	Non-buying compliers & control
Invited	0.317***		0.0714*	0.205***	0.488***		0.267**	0.361***
	(0.0460)		(0.0393)	(0.0659)	(0.0868)		(0.110)	(0.103)
Paricipated		0.672***				1.033***		
		(0.0906)				(0.185)		
Constant	-0.109	-0.0202	0.0604	0.0556	-0.387*	-0.251	-0.0628	-0.0777
	(0.0891)	(0.0854)	(0.0879)	(0.177)	(0.205)	(0.204)	(0.290)	(0.278)
Observations	797	797	473	227	797	797	473	227
R-squared	0.078	0.251	0.053	0.143	0.096	0.059	0.100	0.200

See notes Table 3

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	High frequency usage (every day)				Frequency usage score (0-5)			
Panel A: Self-reported usage	All		Non-compliers & control	Non-buying compliers & control	All		Non-compliers & control	Non-buying compliers & control
Invited	0.240***		0.0529	0.145***	1.394***		0.288	0.878***
	(0.0474)		(0.0350)	(0.0453)	(0.237)		(0.178)	(0.227)
Paricipated		0.503***				2.927***		
		(0.0724)				(0.377)		
Constant	-0.141	-0.0747	0.0879	0.0644	-0.690	-0.307	0.373	0.508
	(0.0993)	(0.0897)	(0.0842)	(0.128)	(0.497)	(0.427)	(0.429)	(0.642)
Observations	769	769	455	218	769	769	455	218
R-squared	0.072	0.192	0.066	0.148	0.087	0.264	0.069	0.189
	Share of days of usage				Avg daily usage time			
Panel B: Predicted actual usage	All		Non-compliers & control	Non-buying compliers & control	All		Non-compliers & control	Non-buying compliers & control
Invited	0.119***		0.0196	0.0725**	32.22***		6.689**	17.16**
	(0.0189)		(0.0127)	(0.0271)	(4.945)		(3.222)	(6.669)
Paricipated		0.249***				67.64***		
		(0.0346)				(8.823)		
Constant	-0.0336	-0.000947	0.0581	0.105*	-10.11	-1.258	11.72	28.42*
	(0.0447)	(0.0430)	(0.0436)	(0.0611)	(11.74)	(11.52)	(10.94)	(16.31)
Observations	769	769	455	218	769	769	455	218
R-squared	0.103	0.250	0.094	0.206	0.122	0.245	0.085	0.192

See notes Table 3. The sample is restricted to individuals with non-missing self-reported usage.

Table E.5: Effects of information received on peer's ownership and purchase on ICS purchase at the session, sample of women not owning ICS at the baseline

	(1)	(2)	(3)	(4)
	Purchase ICS at the session			
RI * PO=0	-0.0222	0.0237	0.00478	0.0473
	(0.0621)	(0.0604)	(0.0733)	(0.0719)
RI * PO=1	0.122	0.00825	0.0285	-0.0682
	(0.0859)	(0.0857)	(0.103)	(0.103)
RI * PO=0 * Respect peer's opinion			-0.0897	-0.0799
			(0.113)	(0.116)
RI * PO=1 * Respect peer's opinion			0.315*	0.264
			(0.176)	(0.167)
Constant	0.105	-0.930	0.0975	-0.918
	(0.182)	(0.706)	(0.183)	(0.700)
Observations	290	290	290	290
R-squared	0.121	0.197	0.130	0.203
Ind. Controls	Yes	Yes	Yes	Yes
Session Controls	Yes	Yes	No	Yes

See notes Table 7