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Summary
We investigate the role of social interaction in technology adoption by conducting a field experiment in neighborhoods of Bamako. We invited women to attend a training/marketing session, where information on a more efficient cooking stove was provided and the chance to purchase the product at market price was offered. We randomly provided an information nudge on a peer's willingness to buy an improved cookstove. We find that women purchase and use the product more when they receive information on a peer who purchased (or previously owned) the product, particularly if she is viewed as respected. In general, we find positive direct and spillover effects of attending the session. We also investigate whether social interaction plays a role in technology diffusion. We find that women who participated in the session, but did not buy during the intervention, are more likely to adopt the product when more women living around them own it. We investigate the mechanisms and provide evidence supporting imitation effects, rather than social learning or constraint interaction.

Keywords: Technology Adoption, Social Interaction, Cookstoves, Mali

JEL Classification: D03, M31, O13, O33

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Abstract

We investigate the role of social interaction in technology adoption by conducting a field experiment in neighborhoods of Bamako. We invited women to attend a training/marketing session, where information on a more efficient cooking stove was provided and the chance to purchase the product at market price was offered. We randomly provided an information nudge on a peer's willingness to buy an improved cookstove. We find that women purchase and use the product more when they receive information on a peer who purchased (or previously owned) the product, particularly if she is viewed as respected. In general, we find positive direct and spillover effects of attending the session. We also investigate whether social interaction plays a role in technology diffusion. We find that women who participated in the session, but did not buy during the intervention, are more likely to adopt the product when more women living around them own it. We investigate the mechanisms and provide evidence supporting imitation effects, rather than social learning or constraint interaction.

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1. Introduction

The process of technology adoption and diffusion has been shown to be driven by several factors beyond market mechanisms (Foster and Rosenzweig, 2010). Social interaction effects, comprising both peer and network effects, have been found to play a relevant role for 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)[1].

In this paper we provide evidence of social interaction effects on the decision to adopt improved cookstoves (ICS), a technology potentially resulting in health and efficiency gains through fuel savings, in the context of urban Mali, where most people rely on solid fuels and traditional technologies to cook. Globally, about 2.74 billion people (40% of the population) still rely on traditional fuels and inefficient technologies to cook with severe consequences on the health of households, due to indoor air pollution (IEA, 2016). The Global Burden Disease study estimates that, globally, almost four million people die prematurely every year from indoor air pollution due to the use of traditional cooking fuels and stoves (Lim and et al., 2012; Martin et al., 2011). Moreover, the extensive use of wood as main energy fuel impacts the local environment, due to deforestation, soil degradation and erosion. Inefficient biomass combustion is a major determinant of black carbon, a contributor to global climate change. Emissions from cooking stoves continue to be a major component of global anthropogenic particulate matter particularly in developing regions, such as Africa and South Asia, where emissions from cooking stoves are well over 50% of all anthropogenic sources (UNEP/WMOO, 2011; Bond et al., 2004). For these reasons, access to safe, affordable, reliable, sustainable and modern energy is one of the goals of the sustainable development agenda. In spite of this, the debate on the impacts and drivers of ICS adoption is still very open in the literature (see Bonan et al., 2017 for a review).

In a field experiment run in a controlled setting, we find that the individual decision to take-up and use ICS is influenced by information on neighbors' willingness to buy them, particularly when the neighbor is respected. We document the presence of social interaction effects in the natural diffusion of ICS within the neighborhoods after the experiment. We provide suggestive evidence that imitation effects play a crucial role, by showing the secondary relevance of alternative mechanisms and by discussing the particular context and product features.

Estimating the impact of social interactions on technology adoption is made difficult by the problem of correlated unobservables, also known as the "reflection problem" (Manski, 1993). Several unobservable factors, different from social interactions, may lead to the observation of similar behavior among peers. For example, homophily and assortative matching imply non-random formation of social ties according to homogeneity of preferences (Manski, 1993; Shalizi and Thomas, 2011). Neighbors may behave similarly because they share a common environment, they may have self-selected, or because they are subject to related unobservable shocks. Economists have focused on a variety of econometric techniques to disentangle social interaction effects from correlated unobservables in non-experimental settings (see, among others, Foster and Rosenzweig, 1995; Conley

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

and Udry, 2010; Munshi, 2003; Bandiera and Rasul, 2006; Brock and Durlauf, 2001). More recently, the use of field experiments has allowed scholars to identify casual effects by exploiting the exogenous variation in the intensity of exposure to treated peers (Duflo and Saez, 2003; Godlonton and Thornton, 2012; Miguel and Kremer, 2004; Kremer and Miguel, 2007; Oster and Thornton, 2012; Dupas, 2014) or the random assignment of peers (Sacerdote, 2001; Kremer and Levy, 2008; Kling et al., 2007).

Our empirical approach to identify social interaction effects is based on two strategies pursued in experimental and non-experimental setting. First, we invite 25 women in each of 36 randomly selected geographical clusters to attend a training/marketing event where ICS is presented and tested against traditional technologies. After the session women are individually invited to purchase the product (or leave a deposit for a purchase in five-days time). In 32 clusters, a random half of participants receives an information nudge about another participant's decision to purchase (or previous ownership)[2]. We find that receiving information on a peer who purchased or previously owned the ICS significantly increases the individual propensity to buy, by about 14 percentage points (22% increase with respect to the control group). After mapping the social network of and among all women in the sampled clusters, we find that people not only adopt the ICS more often when they receive information on a respected peer who purchased (or previously owned), but also use the ICS more in the following months, compared to women who do not receive peer information.

We measure the impact of invitation to our training session by comparing ICS take-up of the 25 treated women in each cluster to a control group formed by 5 women living at 10-minutes walking distance in a random direction from the sampling point. We find that being invited to the training session increases the probability of owning ICS after six to nine months by 31 percentage points (160% increase with respect to the baseline value). We identify and measure positive spillover effects of our intervention on ICS adoption for the group of non-participants by comparing them with women living in control areas, where no intervention took place[3].

We then investigate the extent to which ICS diffusion is the outcome of social interaction among women in the cluster. This is done by exploiting our sampling strategy based on pseudo-random selection of households *à la* Afrobarometer. This strategy truncates existing social networks and allows us to obtain (random) variation in the density of women exposed to our invitation, along the geographical distance dimension. Given the success of our training session in increasing ICS take-up, we exploit the varying number of invited women living within different radii from each individual as an instrument for the number of women who purchased and owned ICS after the session. This allows us to test the impact of their ownership on other women's ICS purchase at different points in time (namely, five days and between six to nine months after the training session). We find that women who participated to the training session, but did not buy on the spot, are more likely to buy with a five-day delay if more women owning ICS live relatively close to them. We also find that women participating, but not buying ICS during our intervention are more likely to eventually own ICS six to nine months later if more women owning ICS live close to them.

[2]Similar 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; Ayres et al., 2009; Costa and Kahn, 2013; Bursztyn et al., 2014; Cai et al., 2015). However, in the existing literature, the information nudge typically reveals the average or median behavior of a reference group. In our study, the information is on a particular neighbor, part of the individual's social network.

[3]We do not dispose of a control group of women living in treated areas, to be compared with women in control areas. We provide evidence of positive selection into session participation and argue that our estimates are lower bounds of the spillover effects.

Three main mechanisms have been described as drivers of social interaction: social learning, imitation and constraint interaction (Manski, 2000). We cannot clearly disentangle which channel is at play in the natural environment; still, we provide evidence which tends to exclude the social learning mechanism, in the forms of learning from others about the functioning and the benefits of the technology. Unlike many technologies, ICS is a fairly easy to use product, whose functioning and use is similar to the widely diffused traditional cooking stoves. Moreover, it turns out that the level of knowledge about ICS existence and main attributes is already relatively high among the population. Starting from these considerations, first, we test the effects of the training session on the level of knowledge about the product and find no significant effect, which support 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 a significant impact of owning ICS on reported fuel expenditure, time spent in income generating activities and income. Hence, benefits from the use of the technology might be hardly perceivable by individual users. It is then unlikely that the social interaction effect may be mainly driven by the learning from others about the benefits of the technology.

Our article contributes to the literature in several ways. First, we provide empirical evidence of a "personal" peer information intervention, whereby individuals receive information on a well-identified peer living in the neighbourhood, and we exploit the variation in pairwise relationship[4]. Interventions disseminating information about peer behavior have been previously 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 in the behavior more themselves. This has been usually done by informing about the behavior of a reference group and, in most cases, an individual behavioral shift towards the peer norm is found[5]. Several reasons may drive an individual to mimic peer's 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 the taste for social status, fear of sanctions, social identity, or relative level of consumption preferences (Bernheim, 1994; Akerlof, 1980; Benjamin et al., 2010, 2016; Abel, 1990; Easterlin, 2001; Luttmer, 2005; Fafchamps and Shilpi, 2008; Bursztyn et al., 2017)[6]. Moreover, we find peer effects on behavioral change in the short-run (ICS take-up), but also in the mid-run (usage).

Second, we contribute to the literature estimating the causal effects of social networks on technology adoption in developing countries. While most of studies are conducted in rural areas where social networks are relatively easy to geographically define, we do it in a urban context, where no

[4]In Bursztyn et al. (2014) 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.

[5]This is the case, for example, in the domains of entrée selections in a restaurant, contributions of movie ratings to an online community, small charitable donations, music downloads, towel re-use in hotels, taking petrified wood from a national park and stated intentions to vote (Cai et al., 2009; Chen et al., 2010; Frey and Meier, 2004; Salganik et al., 2006; Goldstein et al., 2008; Cialdini et al., 2007; Gerber and Rogers, 2009). In constrast, opposite results are obtained by Beshears et al. (2015) concerning saving decisions.

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

clear boundaries are available[7]. This is relevant in the light of the increasing urbanization process taking place in developing countries. Moreover, contrary to many papers in this literature, we explicitly explore the mechanism of technology diffusion and we extend the experimental results to the natural context. Hence, our results contribute to the body of empirical literature testing technology diffusion as modeled by Granovetter (1978) and Acemoglu et al. (2011).

Third, from a policy perspective, we shed light on an important driver for the diffusion of ICS and we provide evidence of its impact and usage. The literature analyzing the drivers of and barriers to ICS take-up and continuous usage is still nascent. Among them, liquidity constraints, intra-household preferences and marketing strategies seem to play significant roles (Hanna et al., 2016; Miller and Mobarak, 2014; Mobarak et al., 2012; David et al., 2016). This paper speaks to this literature and complements other works investigating the role of opinion leaders and peer effects in the adoption of ICS (Miller and Mobarak, 2014; Beltramo et al., 2015; Adrianzén, 2014), by exploring the role of imitation effects. Moreover, the success of our intervention in raising ICS take-up at a market price suggests the importance of transaction costs and of the need for products which respond to local needs and preferences (Lewis and Pattanayak, 2012). We also contribute to the debate on the impacts of ICS on welfare-related outcomes (Hanna et al., 2016; Bensch and Peters, 2015a; Smith et al., 2011). We do not find a significant beneficial effect of ICS adoption for women, in the spheres of fuel expenditure, time for income generating activities and income. Although the level of usage appears relatively high[8], we believe that in our context, where meals are prepared for large families with multiple stoves, the substitution of one traditional stove with a more efficient one is not enough to generate significant changes to climb the "energy ladder"[9].

The rest of the article is organized as follows. Section 2 presents the study design and is articulated in the description of context, experimental design, sample and data collection. In section 3 summary statics and contextual evidence are first presented, followed by the description of identification strategies and results. Section 4 discusses the results and the mechanisms. Section 5 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 fuels (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[10]. Typical traditional wood and charcoal cookstoves are shown in panels a, b and c of Figure 1.

[7]The role of social interaction on job market outcomes within urban neighborhoods is studied in Schmutte (2015) and Bayer et al. (2008).

[8]We rely on both objective measures of usage for a relatively long time span through Stove Usage Monitoring Systems (SUMS) installed on a subsample of ICS and self-reported measures.

[9]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) 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).

[10]The source of the data is the Global Alliance for Clean Cookstoves website http://cleancookstoves.org/country-profiles/26-mali.html, consulted on January 2017.

In collaboration with the French NGO "Groupe Energies Renouvelables, Environnement et Solidarités" (GERES), we identified an improved charcoal cookstove, locally known as "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[11]. The ICS, shown in panel d of Figure 1, is produced by local artisans using recycled materials, is portable, has a metal structure and a combustion chamber made of baked clay, which allows to retain the heat and save charcoal. The market price (about 4,000 CFA, USD 6.50[12]) is higher than traditional charcoal cookstoves (2,500-3,000 CFA, USD 4.20 - 5), however it can be recovered in three months of full usage, due to saving in charcoal. 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 one of the activities which is usually carried out at the level of extended households[13] in order to exploit economies of scale[14]. Meals are prepared "centrally" for all members and women who participate to 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 and three meals per day. On average, about two women are involved in the cooking rotation, with a minimum of one and a maximum of fifteen.

2.2. Experimental design, sampling and data collection

From October 2014 to January 2015, we conducted a baseline survey on 1080 women from 36 neighborhood 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 houses were selected following a pseudo-random process *à la* Afrobarometer. From each starting point, another 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*, he woman leader of the cooking rotation was selected as 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, we believe that we can avoid major 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

[11] 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 (2015b).

[12] ICS market prices usually vary, depending on several factors such as the bargaining ability of seller and purchaser, the quantity bought, the location of the market. While the price for purchase in bulk to producers ranges between 3,000 and 3,500 CFA, the market price may range from 3,500 to 4,500 CFA.

[13] Malian extended households (locally named as *gwa*) include several nuclear units, living in the same compound.

[14] 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*.

among treated individuals is about 90 meters[15]. In some locations the distance between treated and control is lower, due to the presence of important infrastructural separation, i.e. a large road. Figure 2 shows the spacial distribution of treated (in blue) and control (in green) women around the drop-off point. Panel a shows the absolute distance (in meters) among women, while in panel b we standardize it by the cluster-specific maximum distance from the drop-off point to account for different population densities. Appendix A provides the details of the sampling procedure.

Nine hundred women in the treatment arm received an invitation to a training session to be held in a nearby school on a Saturday (non-working day) in ten-days time[16]. The invitation flyer contained a preview of the topic to be discussed, namely energy efficiency and ICS, the personal contact 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[17]. One day before the session, all invited women received a reminder call. Sessions were specifically organized to gather women from the same cluster, were held either in the morning or in the afternoon, lasted about 3 hours and were conducted by a professional product promoter[18]. 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 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 to another room, where an enumerator proposed the purchase of an ICS at the price of 3,500 CFA[19]. Women could decide whether to buy one ICS immediately, to buy it in five days (the next Thursday), 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, could find the money by the time of the home visit by our staff in five days. In case of non-purchase five days after, the deposit was lost. In 32 out of 36 training sessions[20], a random half of women attending the session were provided the information about another random woman in the same session. The content of the information included peer's identity, her purchase decision and whether she owned already an ICS[21]. The randomization protocol was incorporated in a software which we designed for data collection and treatment administration through tablets. Figure 3 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 (both those who left a deposit and those

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

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

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

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

[19]This corresponded to the procurement cost we incurred with our supplier.

[20]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.

[21]We made sure that women leaving the training venue after the purchase choice 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.

who did not want to buy at all) 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.

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 modern cookstoves, risk preferences, 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. Finally, she was asked to name a maximum of six women whose opinion she respected. At the endline, occuring on June 2015 (from 6 to 9 months after the baseline), the questionnaire administered was similar to the one of 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[22]. This not only makes such monitoring feasible and reliable on a large number of households: it also allows us to compare reported usage against observed usage while mitigating the risk of Hawthorne effect, which could arise if measurements were made through frequent households visits. The SUMS we used, iButtons™, 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 4 shows the timeline of our study.

3. Data and Results

3.1. Summary statistics and contextual evidence

The whole study sample includes 1077 individuals, 898 of which were invited to the training session, while 179 were not[23]. About 46% of women invited to the session actually attended, with an average of 11 women per session. 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). We find significant differential attrition rates along the invitation to treatment dimension: we could not track 16% of women not invited to the training session and 6.6% of those invited. In the 32 sessions where the experiment involving the peer information treatment was implemented, 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, attrition and partial compliance issues are analyzed and discussed more in depth in Appendix B and are taken into consideration in the regression analysis.

Table 1 shows baseline characteristics by invitation status and differences across sub-samples. Respondents are about 33 years old and 88% of them live as married couples within extended households (*gwa*). In our baseline sample the average size of *gwa* is about 13 members. More

[22] Households willing to buy ICS were informed that the product *could* be endowed with the SUMS which would record temperature for performance and quality tests. This ensured that the actual presence of SUMS should not influence the take-up decision.

[23] 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.

than 40% of respondents have no schooling, 15% attended primary school, 11% secondary and 30% high-school or more. 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[24]. 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[25]. About 30% of women have some form of personal savings (bank account, rotating credit and saving associations - "roscas" -, or other informal forms). More than half of women in the sample are members of informal groups, like roscas, discussion groups, or neighborhood groups. We also measure the risk preferences using a game based on Harbaugh et al. (2002) where subjects could choose between playing a simple gamble and receiving a specific amount of money with certainty[26]. We define "risk-averse" those who always preferred the certain amount. About two thirds of the sample turns out to be risk-averse.

As far as fuels and cookstoves are concerned, we find that in more than 80% of households the main fuel for cooking is charcoal, in 19% is wood, while in less than 1% gas is used as main fuel. At the baseline, 97% of women declared to own at least one traditional cookstove within the *gwa* (on average more than three), 19.7% own ICS (among those, the average is 1.4)[27] and 50% own at least one small gas stove, typically used for quick heating, like water for baths or to heat up leftovers[28]. 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 rightly attribute characteristics related to efficiency and fuel-saving to ICS. The main source of knowledge concerning the product is represented by people connected to the respondents (family members, friends or neighbors) who have one, mentioned by 61% of women, followed by market (56%) and promotional campaigns in the media (34%). 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

[24]The averages presented for working time and income are unconditional. 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 4,000 CFA would represent 20% and 6.4% of women and head monthly incomes respectively.

[25]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 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, presence of fridge, camera, TV, sofa, table and chairs, bike, motorbike, car, sewing machine, wood or iron bed, air conditioning, fan.

[26]Two sets of three questions were presented, with small and big stakes respectively, each of which offered the choice between A, receiving an amount of money with certainty, and B, participating in a lottery where they could either gain 1,000 (10,000) CFA, about USD1.7 (USD17), with probability 0.25, or gain nothing with probability 0.75. Hence, the expected absolute value of the gamble was always the same, and the amount of money received with certainty varied across choices (lower, equal to, and higher than the expected value of the gamble, namely 200, 250 and 300 CFA and the same amount multiplied by 10). The point at which a subject switches from the risky to the safe alternative allows us to determine the respondent's degree of risk aversion. Games were not incentivized.

[27]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.

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

aspects were the short durability (54%) and the high price (16%)[29]. 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 far above the actual market price.

Table 2 shows observable characteristics by treatment groups across women who participated in the training session and took part to the peer information experiment. Descriptive statistics for the two groups are similar to those presented in table 1. In the 32 training sessions where the informational treatment was implemented, 164 women (47%) received the information treatment, while 189 (53%) did not[30].

The lack of significant differences along most of the observable baseline characteristics in both tables 1 and 2 leads us to believe that the randomization exercises, both of the invitation treatment and of the information nudge, were successful[31].

The outcome variables for the analysis are related to the take-up, ownership of and knowledge about ICS at different stages of our study and for different sub-samples.

The first set of variables concerns the outcomes of the peer information treatment, refers to the sample of attendants and is reported in Table 2. The first variable relates to the willingness to buy the ICS just after the training session, on Saturday, is equal to one for 65% of participants and includes both actual purchase on the spot (about 37%) and the decision to leave the deposit (28% for both groups). The second is a binary variable for ICS purchase with 5-day delay (32%), while the third variable takes the value of one if the attendant purchased ICS within the experimental window, i.e. either on Saturday or Thursday (67 and 71% for control and treatment group, respectively). A set of variables related to ICS usage, both objectively measured and self-reported, is also introduced and is further detailed below. Overall, we find that women who received the peer information treatment are not more likely to purchase and use ICS than those who did not get the information.

The second set of variables concerns the outcomes of the training/marketing session, refers to the overall study sample (both invited and non-invited) or other specific sub-samples and is reported in Table 1. In particular ICS purchase and ownership at different stage of the study are shown. One can notice that while the difference in ICS ownership between invited and non-invited women was not statistically different at the baseline (20.3% and 17.3%, respectively), we find that the share of households owning ICS increases by 26 percentage points (significant at 1% level) at the endline in the treated group. However, one should notice that the the level of ICS ownership in the treated group at the endline (44.7%) is relatively similar to the one registered immediately after our intervention, defined as both Saturday and Thursday proposals (45.5%)[32].

[29] About 13% of stoves acquired during the study period by women not owning it before have been found partially or completely broken at the endline.

[30] 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.

[31] The tables 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% 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%, while for the sample exposed to the peer information experiment the rejection rates are 6.8, 5.9 and 2.5%, respectively. Results are available upon request.

[32] The reader should notice that the sample for which information on ownership at the endline is available is subject to attrition. The share of ICS owners after the intervention in the non-attrited sample is 46.3%. It should be also noted that the information on the share of women living in control areas and owning ICS after the session (Saturday) and the overall intervention (Thursday) are not directly collected. The same level observed at the baseline conducted ten to fifteen days before has been imposed.

A set of variables relating to ICS knowledge was asked only at the endline and is shown at the bottom of Table 1. 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[33]. Finally, we explicitly ask if the respondent knows other people owning ICS. This is 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 neighbors owning ICS.

As far as monitoring data on usage are concerned, in 17 out of 36 clusters, SUMS were randomly attached to 98 ICS, out of 236 sold in sessions where the peer information treatment was implemented. We were able to successfully track 73 of them. 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 the sample 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 the end for some of the SUMS released, that is around mid January, a monitoring pass was ran, so that data were collected from the devices, and a new recording was started. In principle, we hence have two waves of temperature data for each SUM: Figure 5 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 3, panel A, reports the descriptive statistics of usage variables from SUMS, as well as self-reported measures collected at the endline. We find that about 73% (53 out of 73) of women use ICS at least once. ICS is used, on average, in 35% of the days of monitoring. Upon usage, ICS is used, on an average day, for a total 267 minutes (more than four hours), during more than 2 cooking events which last about one hour and a half each. As far as self-reported measures are concerned (panel B), we focus on the non-attrited sample of women who participated to the peer information experiment: 65% of them (N=223) own ICS at the endline and for 96% of this group (N=214) we have non-missing self-reported usage[34]. About three fourth of women owning ICS at the endline reported to use it daily (47% all the time, 28% at least once a day)[35], about 10% use it from one to four times a week, 7% declared to use it rarely and 7% never. We also construct a continuous variable of self-reported ICS usage (named "share of time of usage") where we assign

[33]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 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.

[34]The analysis of missing data is done in appendix C.

[35]In the analysis which follows a binary for at least daily usage is employed.

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 we show that self-reported measures are good predictors of usage, as monitored through SUMs.

3.2. Measuring social networks

A social network is defined by individual members (nodes) and the links among them. Several techniques have been proposed to sample nodes within networks and to measure the type of relationship among them, each with advantages and disadvantages (see Maertens and Barrett, 2013 for a discussion). However, none of the previous work has tackled the measurement of social networks in urban developing contexts, where geographic boundaries are not as well defined as in rural villages. Our urban context could not allow to obtain complete census of networks[36], due to the impossibility of establishing boundaries and resource constraints. Hence, we use a "network within sample" approach: first, we randomly select 25 households living close to each other in each cluster, then we map the social links among respondents, along the dimensions of knowledge by sight and whose opinion one respects. Despite the several shortcomings of this approach (see Maertens and Barrett, 2013 and Chandrasekhar and Lewis, 2011 for a discussion), it represents a good option to test the role of social interaction on individual decision-making in our experimental setting[37]. Notwithstanding the fact that our sampled network structure may be a partial representation of the true social networks within a geographical cluster, by stratifying our information treatment at the session level, we end up with a balance between treated and non-treated individuals along the social network dimension. It is worth mentioning that the correlation (within clusters) between pairwise distances and connectedness is strongly significant ($p < 0.001$), but only explains about 20% of the variance. That is, while women tend to know their neighbors, looking at the social network allows us to capture dynamics of different nature than the mere geographic proximity. See Figure 6 for the distribution of pairwise distances conditional on the social network.

Table 2 shows that no significant difference in the number of women to whom participants are linked to, along different dimensions, arises between the two sub-samples. Also, the geographical distance of women from the drop-off point, i.e. from the point where the sampling strategy started, seems to be evenly distributed between treatment and control group. These two pieces of evidence point to the fact that any bias arising from our network within sample approach is unlikely to influence the results of the peer information experiment.

3.3. Peer effects on ICS take-up and usage

After the marketing session, women were randomly assigned to a treatment and a control group. Treated women received information over the previous ownership and purchase decision of ICS at the session of another randomly selected peer, whereas control women did not. We evaluate the effect of receiving the information nudge, by estimating the following equation on the sample of participants:

$$Y_i = \beta_0 + \beta_1 Info_i + \beta_2 Info_i \times ICS_j + \gamma X_i + \epsilon_i \tag{1}$$

where Y_i is the outcome of woman i, $Info_i$ is an indicator which is equal to one if the woman received the information nudge and zero otherwise. β_1 provides the overall impact of receiving the information, regardless of the content, i.e. the identity of the peer and whether or not she owned

[36]Census of networks are constructed for example in Broeck and Dercon (2011); Banerjee et al. (2013, 2014)

[37]The study of determinants of link existence and social network structure goes beyond the scope of our analysis.

or bought the ICS. ICS_j represents the content of the information provided and is expressed as a dummy equal to one if and only if the peer j, randomly selected from the list of other participants, owned before or bought ICS at the session. β_2 captures the effect of conveying information on the peer's take-up decision (and/or previous ownership of ICS). 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, risk aversion, knowing about and already owning an ICS. We assess the extent to which different types of social linkages with the peer influence individual decision to take-up, by estimating the following regression:

$$Y_i = \beta_0 + \beta_1 ICS_j + \beta_2 Peer\_Rel_{ij} + \beta_3 ICS_j \times Peer\_Rel_{ij} + \gamma X_i + \epsilon_i \tag{2}$$

where $Peer\_Rel_{ij}$ indicates different levels of social linkage between the treated woman i and the randomly selected peer, j, about whom we provide the information. We construct two variables which are equal to one if they know each other by sight and if the treated woman named her peer's opinion as respectable, and zero otherwise, respectively.

In Table 4 we look at the probability for woman i of purchasing ICS or leaving the deposit just after the traning/marketing session (columms 1 and 2), purchasing with 5-day delay (columms 3 and 4), or purchasing within the experimental window, including both the post-session on Saturday and the home visit on Thursday (columns 5 and 6). We find that receiving the information does not affect ICS take-up. However women receiving the information about a peer who purchased or owned ICS are 14 percentage points more likely to take-up ICS or leave the deposit at the session. This represents an increase of about 22% with respect to the control group. The coefficient is significant at the 10% confidence level. We also find that women are significantly more likely to take up ICS, at all stages, when the peer they receive information about owns or adopts ICS and is respected. The negative coefficient (significant only in the case of purchase with 5-day delay) attached to the existence of a relationship based on respect indicates that when the respected peer does not buy or did not own, women tend to buy less. In this sense, this represents suggestive evidence of a symmetric effect. Being a simple acquaintance does not influence the propensity to take-up ICS.

We also look at the extent to which the information nudge has longer-term effect on ICS usage. We use the same specifications from previous analysis and measure the impact of receiving the information nudge and its heterogeneous effect along the pairwise relationships. This is done in Table 5, where monitored usage is assessed at the extensive (columns 1 and 2) and intensive margin (columns 3 and 4) on the sample of women with ICS endowed with SUM. Self-reported usage is presented as a dummy for daily usage (columns 5 and 6) and a continuous variable (columns 7 and 8). We find significant heterogeneous effects along the pairwise relationships. In particular, monitored usage at the extensive margin increases for women who received the information on a respected peer who purchased or owned ICS, while decreases if the respected peer did not purchase or own. We also find that usage decreases when the peer who purchased was not known at least by sight (column 2). While the asymmetric pattern is not confirmed in the other usage dimensions, the effect of receiving information on a respected woman who purchased or owned ICS is positive and significant for the intensive margin of monitored usage and for both self-reported measures.

3.4. Direct and spillover effects of the training session

We investigate the extent to which our training session directly and indirectly influences women take-up decision. For the direct effects, we look at the Intention to Treat Effect (ITT) of the invitation to the training/marketing session. 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., 2014). This is not possible in our context, as we do not dispose of data on (randomly) non-invited women in treated areas. Instead, we use women who did not participate to the training session and compare their take-up decision with that of women in control areas. In doing that, 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 different samples:

$$Y_i = \beta_0 + \beta_1 Invited_i + \gamma X_i + \epsilon_i \qquad (3)$$

where β_1 provides the ITT of our intervention on ICS ownership at the endline. Table 6 shows the results for different sub-samples, reported on the different column headings. Columns 1 refers 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). Column 2 shows 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. We find that participating to the training/marketing session increases the likelihood of owning ICS by 67 percentage points on the population of compliers. In column 3 we repeat the exercise on the sample of women who have been invited but who did not participate and non-invited ones. We find that non-participants are more likely than control women to own ICS at the endline. This represents suggestive evidence of the presence of spillover effects. However, the estimate is likely to be biased, as the group of non-participants is self-selected. We claim that the selection bias is negative, i.e. non-participants are less likely to purchase than women in control areas, who are not different from the full sample of both participants and non-participants (see Table 1 for a comparison of the two samples along observable characteristics). In other words, selection into participation is likely to be positively related to ownership. Besides evidence from column 3, we also find that women who participated to the intervention but who did not buy ICS (neither on the spot nor with a 5-day delay) are still more likely to own ICS at the endline than women in control areas (column 4). While we are unable to precisely quantify the importance of different channels (information treatment, within-cluster social interaction) in affecting this result, it still provides suggestive evidence of the effect of the experiment. In general, the evidence of positive and significant (at 5% confidence level) effect of the invitation on the group of non-participants can be interpreted as a lower bound of spillover effects[38].

Two main reasons can explain the relatively high success of the intervention. The first lies in the role of the training session in decreasing information constraints. The second is related to the role of the session in lowering the transaction costs for ICS purchase. Women could access ICS at the lowest price one could get in the market, with no need to go to the market, negotiate the price and bring home the product (about 5 Kg weight). Moreover, participation to the session was subsidized, in the form of a reimbursement for transportation fees (1,000 CFA)[39]. Our experimental design does

[38]One caveat of the analysis is represented by the possible consequences of the differential attrition we faced. We estimate Lee bounds (Lee, 2009) and find robust results for column 1, but not for columns 3 and 4. In particular, in the two latter cases the lower bound is not statistically different from zero. Combining the lower bound estimate of the spillover effect with the fact that we are underestimating them leads to consider the results with caution.

[39]One could also see this as show-up fee for the time dedicated to the session (about 3 hours). No other form of incentive has been introduced during the data collection process.

not allow us to clearly disentangle the two channels. However, we find no evidence that the training session leads to higher ICS take-up for women who were less knowledgeable or experienced about ICS at the baseline[40]. This result seems to suggest a primary role of transaction costs with respect to the information constraints explanation.

3.5. Diffusion of ICS through social interaction

Identifying the causal effects of social interaction in the diffusion of technology adoption is an empirical challenge (Moffitt, 2000; Bramoullé et al., 2009; Fafchamps, 2015). Typically, the linear-in-means model, where the outcome of each individual depends linearly on his own characteristics, on the mean outcome of his reference group and on its mean characteristics, has been used for such purpose. In the jargon of Manski (1993), one should distinguish exogenous (or contextual) effects (i.e., the influence of exogenous peer characteristics) and endogenous effects (i.e., the influence of peer outcomes) from correlated effects (i.e., the fact that individuals in the same reference group tend to behave similarly because they are similar). The first identification problem arises from the difficulty to distinguish social interaction effects (both endogenous and exogenous) from correlated effects. The second problem derives from agents' simultaneous behavior, implying that the the expected mean outcome of the reference group and its mean characteristics are perfectly collinear.

Experimental approaches offer a solution to these problems. One of them exploits the random variation in the density of treated peers at varying distances from control individuals as a proxy for indirect treatment intensity exposure. In order to overcome the reflection problem and borrowing from this literature, we base our identification of causal social interaction effects on the construction of an instrumental variable which would correlate with the reference group's behavior but would be uncorrelated with all determinants of the correlated effects. In what follows, we present how the instrumental variable is constructed and used to identify social interaction effects. First, our sampling strategy randomly selects the starting points and truncates existent social networks from a geographical point of view, by selecting households in a pseudo-random way, as described in section 2.2 and A.3. This implies that women, by design, regardless of their personal traits and social connections, are "surrounded" by a varying number of other women who have been invited, depending on their distance from the initial starting point. Such number is deemed exogenous by construction. Intuitively, within the same treated sampling point, we compare women at the boundary of the sampling areas with women at the core of it. The former are expected to have fewer neighbors who have been invited to the session compared to the latter. We look at the number of invited women in the sample at different (arbitrary) radii from each individual. In order to account for different levels of density of buildings and households, our measure of geographical distance is a function of the mean pairwise distance among all women in the same cluster and α, which is a fixed arbitrary parameter ranging from 0.1 to 4. Consequently, the length of the radius around each woman varies, across clusters, depending on the density of houses in the neighborhood and on α. Table 7 shows the mean characteristics of women who have a number of neighbors within the radius above and below the median number of neighbors in the cluster at a given level of α (panel A reports the values for $\alpha = 0.5$ while panel B for $\alpha = 1$), respectively. The exercise is repeated for the sub-samples of interest for the analysis which follows. One can notice that most characteristics

[40] The exercise is conducted by estimating heterogeneous effects of specification 3 along the baseline values of the dummy variables "know ICS", "ICS is efficient", "allows to save time and fuel" and "ICS owned at baseline". Results are available upon request.

are balanced across the samples[41]. This supports our claim that the number of invited women within a given radius can be deemed exogenous. Thus, we estimate the following reduced form:

$$Y_{isd} = \beta_0 + \phi(\#Invited_{isd}) + \gamma X_i + \psi P_s + \epsilon_{isd} \tag{4}$$

where Y_{isd} is the outcome of interest (ICS take-up or ownership) for individual i in cluster s, $\#Invited_{isd}$ is the number of invited women living within a radius $d = \alpha \cdot \bar{D}_s$ from woman i, with $\bar{D}_s$ being the mean pairwise distance among all women in cluster s[42]. ϕ measures the marginal effect of having an additional neighbor invited to the session within the radius d. X_i includes the same set of individual controls previously specified and the individual normalized distance from the sampling point. P_s is the median pair distance in cluster s.

The second important aspect for the validity of the instrument is the relationship between the invitation to the session and ICS ownership. Our invitation to the session significantly increases the level of ICS ownership. The level of ICS ownership grows from 20.3 to 34.1% after the training session and increases by an extra 11.4 percentage points after the second visit after five days, eventually reaching a share of 45%[43]. We check the existence of the first stage relationship by looking at the effect of $\#Invited_{isd}$ on $\#ICS\_owners_{isd}$, test the weakness of the instrument and eventually estimate the following structural equation through instrumental variables:

$$Y_{isd} = \beta_0 + \psi(\#ICS\_owners_{isd}) + \gamma X_i + \psi P_s + \eta_{isd} \tag{5}$$

where ψ represents the marginal effect of having an additional neighbor owning ICS living within distance d. Since the density measure may be spatially correlated, we run, as a robustness check, corrected standard errors following the spatial dependence correction by Conley (1999). Results, not shown, are always confirmed and standard errors are always lower than the standard errors clustered at the level of sampling point which are shown in the tables.

We look at different sub-samples in different phases of the study design. In particular, we first consider the sub-sample of women who were directly exposed to our training session but did not purchase on the spot. Secondly, we look at the sub-sample of non-participants. Given the non-random nature of the samples considered, if the instrument was correlated with the selection into participation and ICS purchase during the session, our results would be biased. However, in Appendix D we show that the instrument does not predict attendance to the training/marketing session nor willingness to buy or leave the deposit.

3.5.1. Short-term social interaction effects

We estimate the probability of purchasing ICS for the sample of women who are revisited and proposed to buy ICS five days after the training session. This was the case for women who did not buy ICS on the spot. Table 8 reports the results obtained for α equal to 0.5. As the number of neighbors who have been invited increases, the propensity to purchase ICS also increases (column 1). When we use the number of neighbors who actually purchased at the session on Saturday (column 2) and the number of neighbors owning ICS after Saturday, which also includes those owning ICS before (column 3), the marginal effects appear to increase in size, consistently reflecting a decrease in the value of the regressors. Columns 4 and 5 show the first step of the IV estimates

[41] Similar findings are obtained for other values of α, results available upon request.
[42] The mean distance among peers within geographical clusters is about 95 meters (median 87 and maximum 388).
[43] These numbers only refer to invited women and can be seen in Table 1.

conducted in columns 2 and 3, respectively. The number of invited neighbors within a certain distance is a strong predictor of the endogenous variables employed and passes standard tests for weak instruments. We run sensitivity analysis on α in Figure E.1. The figure shows that positive effects persist, across specifications, for values of α between 0.5 and 0.7, while it vanishes for smaller and larger distances[44]. In absolute terms, we find effects within a radius varying from 47 to 66 meters, being 95 meters the average pairwise distance across the 36 clusters. The results survive to the inclusion of the usual individual controls used in the previous exercises, none of which displays significant coefficients. Results hold when we control for two variables which, unsurprisingly, seem to be positively correlated with the outcome. The first is a dummy variable which is equal to one for participants who left the deposit at the session. The second is equal to one if the woman reported to have discussed about ICS purchase with other attendants after the training session.

We look at heterogeneities in own and other's network characteristics, by including individual network centrality, average neighbors' network centrality and the number (or share) of neighbors socially connected to the woman within the radius. However, no significant effects seem to arise (results are available upon request).

Our results seem to suggest social network dynamics implying a natural flow of information about the ownership of ICS by other neighbors living at relatively close geographical distance. This does not seem to hold for other products, such as index insurance, certainly a less evident and tangible product: in Cai et al. (2015) peers' decision to adopt index insurance matters in individual take-up decision only when it is explicitly revealed to individuals.

3.5.2. Long-term social interaction effects

In this section, we focus on the natural behavior of the sample of women invited to the session in the period comprised between the end of our intervention and the endline survey, which was ran after 6 to 9 months.

First of all, one should notice that the share of invited women owning an ICS after the second visit is 45% (46% if we look at the non-attrited sample), while it is 44% at the endline. This suggests a rather poor take-up rate in the period following our intervention. However, this aggregate data conceals some heterogeneity across different sub-groups. First, for the sample of attendants the share of owners decreases from 73.2% after our intervention (considering both the training session and the second visit) to 66.3% at the time of the endline. This, in turn, reflects two opposite effects: on the one hand, for the group of participants who purchased ICS during the intervention (275 observations) the share of ICS owners at the endline is 83%. This represents a 17 percentage points (46 cases) decrease with respect to the post-intervention situation[45]. On the other hand, for

[44] These are calculated at 10% confidence level. Effects are significant at 5% level for values of α between 0.5 and 0.6.

[45] There may be several reasons which may explain why such decrease occurred. First, it can be the case that women purchased ICS in order to give it to some other woman outside the *gwa*. Traditionally, at the time of marriage, Malian women leave their home and move to their husband's house (and *gwa*) and tend to receive gifts for the kitchen as part of the bride's dowry to be taken in the new house. Second, in some cases the respondent of the endline survey is different from the respondent at the baseline and session participant. In some of these cases, the original respondent could have moved to another location, taking the ICS with her. We tackle this particular issue by running a robustness check where all the analysis are repeated on the sample of observations where the respondent remains unchanged and results, available upon request, are not affected. Third, our intervention may give rise to re-sale on the secondary market. Given the relatively small profit margin, we tend to exclude this channel, also in the light of the evidence by Hoffmann et al. (2009), where it is shown that even free or heavily subsidized anti-malaria bednets tend to be retained by poor rural households. Fourth, within our intervention, women were entitled to

the group of participants who did not purchase ICS during the intervention (N=127) the share of ICS owners grows from 19.7% right after the intervention to 29.1% at the endline.

Second, for the sample of non-participants (N=437), the share of owners moves from 20.6% at the time of the baseline[46] to 24.9%[47]. The discussion which follows aims at analyzing the role of social interaction in influencing ICS ownership for these different sub-groups.

As far as the group of participants is concerned, we separately estimate equations 4 and 5 on the sub-sample of women who purchased ICS during the intervention and on those who did not. Results are shown in tables 9 and 10, while sensitivity analysis along α can be found in figures E.2 and E.3, respectively. We find no evidence that social interaction effects play a role in the decrease of ICS ownership over time, for the group of buying participants. Instead, we find positive effects of the number of invited women (as well as the number of women purchasing ICS during the intervention and those owning ICS after it) for the sample of non-buying participants. The effects are positive and significant (at 10% confidence level) for values of α ranging from 0.2 to 0.5.

Finally, we repeat the exercise for the sample of non-participants and we do not find any significant effect from the density of women owning ICS living close (at different geographical distances). This is shown in Table 11 for α equal to 0.5, while Figure E.4 shows the sensitivity analysis.

4. Mechanisms and discussion

Overall, the results from the experiment conducted during the training session tend to indicate that social interaction plays a role in the individual decision-making over technology adoption. In particular, people react to the information over another peer's behavior by conforming to her choice[48]. This appears to be particularly true if women are revealed information about a respected peer who purchased or previously owned ICS. Some studies show evidence that negative information is more salient than positive information (Miller and Mobarak, 2014; Sungjoon Nam, 2010). Conversely, we show evidence of the relevance of positive rather than negative information.

In sections 3.5.1 and 3.5.2 we provide non-experimental evidence on the diffusion of ICS ownership at different points in time as an effect of social interaction. A different mechanism seems to arise for women who participated our training session and for those who did not. For the former group, we find that having more neighbors living at a relatively close geographical distance and owning ICS significantly increases the individual likelihood to purchase ICS. For the latter group, we do not find evidence of the same mechanism. One explanation of the finding is that the common

buy only one ICS each. This may lead to collusion behavior among participants: those who were not interested in purchasing bought an extra ICS for those who were interested who already bought one, therefore not entitled to get another. Given the characteristics of our intervention, we believe that this may have more likely occurred for women who purchased with a five-day delay. Indeed, this seems to be the case for about two thirds of women who did not own anymore at the endline, despite having bought (30 out of 46 women purchased ICS with 5-day delay). Fifth, another possible mechanism is related to under-reporting on the actual ownership of ICS at the endline, driven by strategic behavior in the expectation of future benefits (see similar examples in Blattman et al., 2016 and Martinelli and Parker, 2009). In the absence of a direct way to test for this, we look at the number of women owning ICS at the baseline, purchasing ICS during our intervention and not owning it at endline, but we find only one case.

[46] This share is not statistically different from that of the sample of those who eventually participated to the training session, equal to 20.3% (P-value=0.91).

[47] All values presented are calculated for the non-attrited sample.

[48] We are unable to shed light on which piece of information revealed - peer's purchase at the session or previous ownership - is actually more relevant in inducing imitation.

experience of participating to the session makes the issue of ICS more salient in the minds of those who participated and reinforce the social interaction channel.

Social interaction effects can occur through three main channels: expectation, preference and constraint interaction (Manski, 2000). The first channel is defined as expectation interaction or social learning (henceforth social learning). It occurs when people draw lesson from the actions chosen or outcomes experienced by others. People may learn more about benefits and value of the product (Conley and Udry, 2010; Kremer and Miguel, 2007; Devoto et al., 2012); or learn 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[49]. 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. Few recent studies have tried to identify imitation effects, disentangling from social learning. First, 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. Second, 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, the context and additional evidence presented in this section lead us to speculate that the imitation mechanism we observe in the experimental setting is a more reasonable and prevailing explanation for the effects we observe, compared to the social learning channel.

The particular type of product employed in the study has some key characteristics that make it differ from other technologies which have been investigated in relation to social interaction effects. First, ICS is already an established technology which is known by the great majority of women in the population (93.6% know about it at the baseline). Its design and way of use are similar to the traditional charcoal stove and therefore do not imply significant behavioral changes or large informational gaps to be filled on how to use the technology. In this regard, ICS is different from, for example, index insurance, menstrual cups and contraceptives where social learning has been shown as the 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. This is widely different from the case of adoption of new seeds or agricultural practices, which entail important changes on fundamental sources of livelihood.

The first piece of evidence supporting our proposition lies in the lack of informational gaps in the knowledge of the product. In other words, we find that no relevant information which may improve one's knowledge about ICS, and therefore influence its take-up, could be learned by others. This seems to be the case simply because there does not seem to be anything to learn about. Table 12 shows the ITT of the invitation to the training session on four variables in the domain of the knowledge about ICS and on a score obtained from the sum of them. Regressions include the whole

[49] A first theoretical study of herd behavior is found in Banerjee (1992). In the domain of 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).

sample of women (both invited and non-invited women)[50]. We do not find any significant effect on the knowledge of (the existence of) ICS, or its main peculiarity, the efficiency trait, measured in two ways (column 2 and 4). People do not seem to benefit from the attendance to the session in improving the knowledge of where ICS can be bought (column 3). The lack of significant effect persists when we consider the aggregate index of knowledge (column 5). While no effect of the training session is found on the individual level of knowledge about ICS and its attribute, we find that the share of women knowing someone owning ICS increases as an effect of the training session (column 6)[51]. The result in column 6 seems to be driven by the number of people owning ICS among neighbors (column 8), rather than among friends and relatives (column 7).

The second piece of evidence relates to 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, agricultural innovations or piped water connection (Kremer and Miguel, 2007; Dupas, 2014; Conley and Udry, 2010; Maertens, 2017; Devoto et al., 2012). In our setting, ICS ownership does not seem to influence significantly women's welfare and lifestyle. Table 13 shows the impact of being invited to the session and ICS ownership on a set of variables which are expected to be influenced by the introduction of ICS: fuel expenditure, women time allocation and income generating activities. The exercise includes the entire study population. The invitation to the session is first used in a reduced form (ITT), then as an instrumental variable for ICS ownership. The use of the latter leads to the estimation of the LATE. The instrument seems satisfactory: it is uncorrelated with the outcomes by construction, as for the randomization, and is correlated with the endogenous regressor, ICS ownership after the intervention, as confirmed by the first step regression in column 1[52]. ICS ownership does not have any significant impact on overall monthly fuel expenditure[53], propensity to have an income generating activity, time spent for income generation or monthly income[54]. Our impact analysis has three caveats, though. First, ICS ownership may not necessarily imply 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 all the times they have to cook, 23% daily, 15% 1 to 4 times per week[55]. This reveals a certain degree of appreciation by people. Results from Table C.2 show that reported usage significantly predicts the effective ICS usage, as monitored on a representative sample of purchasers through SUMS. Hence, we are confident that self-reporting is a reliable measure of actual usage. Despite the relatively high levels of usage, 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. In order to have some impact, besides

[50] It should be noticed that for the first two outcomes both baseline and endline measures are available, therefore we estimate the change over time, while for outcomes in columns 3 to 5 the simple treated - control comparison at the endline is shown.

[51] Even in this case, the variable is only measured at the endline and the regression represents treatment-control comparison.

[52] The weak identification statistics reported in the bottom line confirms that our instrument is not weak, as the first stage F-stat is always way above the rule-of-thumb level of 10.

[53] We do not have data on effective and perceived fuel saving by different stove type.

[54] For some variables the sample size is reduced due to missing data, however we do not find any systematic pattern with respect to our treatment allocation. For the continuous outcome in columns 8 to 11 the results remain unchanged after using standard trimming or winsorization, to control for outliers.

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

being regularly used and well maintained, ICS should replace completely traditional stoves. It is well documented that energy transition likely occurs through energy stacking, i.e. both modern and traditional fuels and cookstoves are used at the same time, instead of switching (Ruiz-Mercado et al., 2011; Masera et al., 2000). Second, non-significant coefficients may be due to lack of statistical power. However, through ex-post power calculations, we find 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)[56]. These are commonly considered as small effects. Third, women perception about the benefits may differ from reality. We look at the effect of the session on the share of women over-rating the expected fuel saving from ICS usage, i.e. those convinced that fuel saving may exceed 40%. Although we find a positive effects of the invitation in the ITT specification (column 10), the result does not hold in the LATE specification (moreover the results would not pass a multiple hypothesis test). This indicates that our session (and ICS ownership) may have generated some over-expectations on the benefits of ICS usage, however we believe that such effect is far from being determinant in the social interaction dynamics[57]. Overall, the evidence seems to suggest that social learning cannot be interpreted as the main mechanism driving the process of diffusion of ICS through social networks.

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 tiny sample of the overall city population (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 on the availability of ICS in the city or in prices.

5. Conclusions

This paper combines experimental and non-experimental evidence on the effect of social interaction on the adoption and usage of more efficient technology for cooking in a context of diffused energy poverty. We find that individuals are influenced by the information on the purchase decision or ownership of another peer living in the same geographical area, particularly if she has been listed among those whose opinion is respected. In the short-term, being influenced by the peer implies higher ICS take-up. In the mid-term, women receiving the information nudge from a respected peer who purchased or previously owned ICS tend to use ICS more. We test the existence of social interaction effects in the diffusion of the new technology beyond our intervention, at different points in time, within geographical clusters. By exploiting our sampling strategy, we use the proximity of women invited to our training session as an instrument for the number of surrounding women adopting the product. We find that women are more likely to purchase the product when more women around them own it, and we are able to rule out that this effect is driven by social learning.

The study provides important policy lessons. First, technology diffusion may spread naturally through social networks: however, depending on the prevalent mechanism in place and on the characteristics of technologies, different policies should be implemented to speed up the process of

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

[57] Also in Miller and Mobarak (2014) fuel savings from the use of an efficient ICS with characteristics similar to ours are not perceived as significant.

adoption. In case of products similar to ICS, i.e. easy to use, relatively cheap and similar to the traditional one, the focus should not be on informational campaign, but more on direct market penetration, possibly lowering transaction costs. Indeed, we find a large effect of our session on take-up, but not because it fills an informational gap, given the wide knowledge of the product and its attributes already at the baseline. Instead, it probably provides easy to get ICS at the most competitive price available in the market. Once geographical penetration is obtained and a sufficient number of women takes-up, social interaction would contribute to the diffusion of the technology. People tend to imitate peers whom they respect and trust. This implies that in order to increase take-up and actual usage of technologies, marketing strategies should target trustworthy and respected individuals in communities and neighborhoods. Second, in order to generate actual welfare impacts on energy poor women, interventions should consider carefully the local context, both in designing ICS models to make them fit local tastes and in considering cooking habits. This latter theme is related particularly to the practice of energy stacking, which requires higher effort to reach the energy transition.

Our study is innovative in that it provides evidence of social network effects on technology adoption in urban development context, a still under-researched area. From a research perspective, our design and limited measure of social networks do not allow us to further investigate the imitation mechanism and disentangle different channels identified by the theoretical literature. These would be important empirical advances future research should consider.

6. Tables and Figures

Figure 1: Models of cookstoves

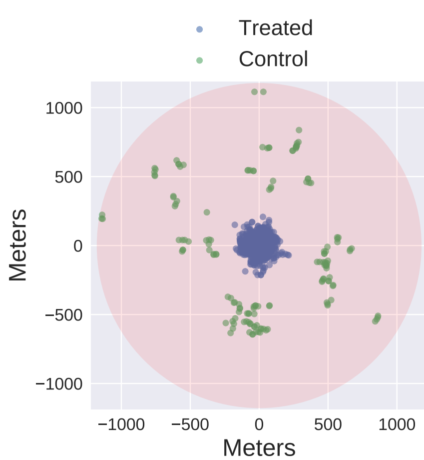
(a) Traditional three stone stove (wood)

(b) Traditional metal stove (wood)

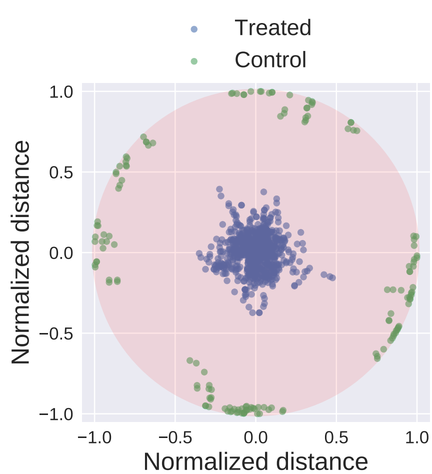
(c) Traditional metal stove (charcoal)

(d) Improved cookstove (charcoal)

Figure 2: Geographical distribution of women with respect to drop-off point

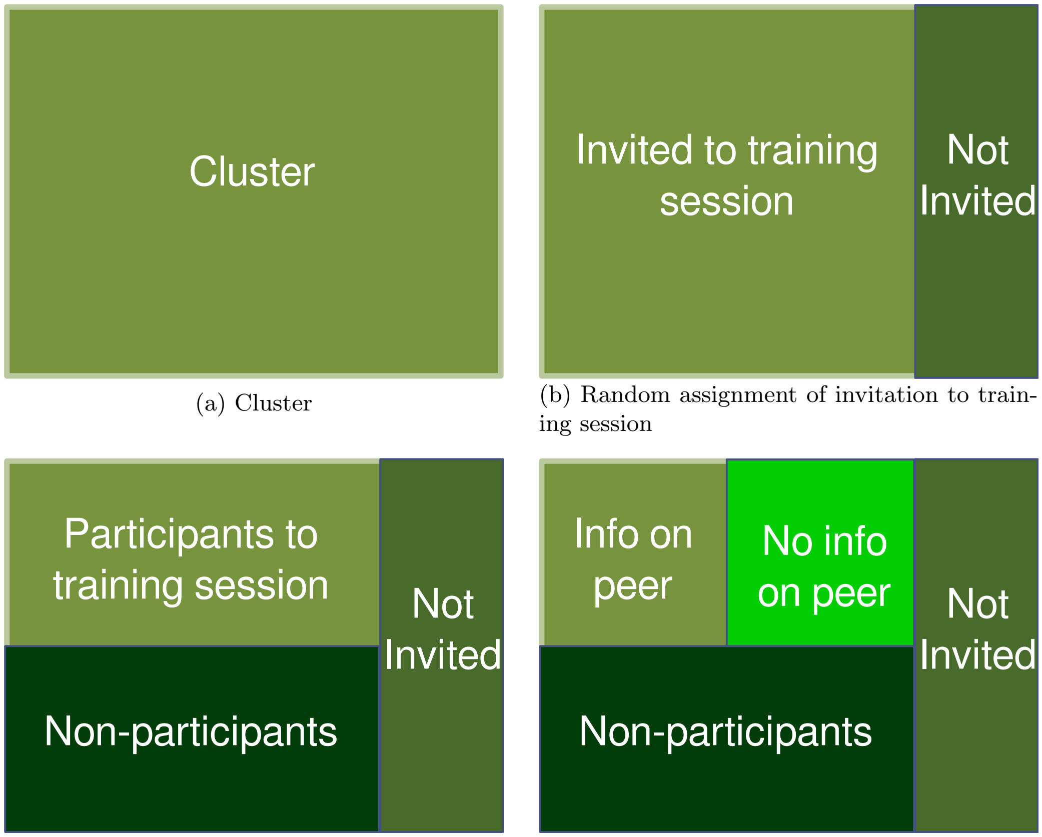


(a)



(b)

Figure 3: Experimental design



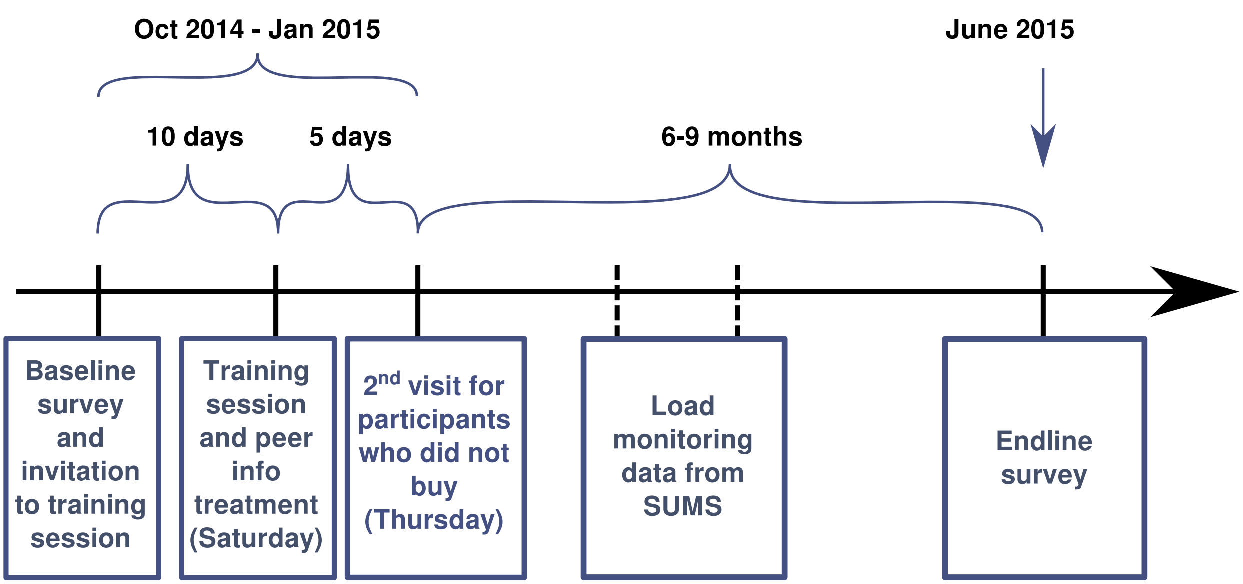
(a) Cluster

(b) Random assignment of invitation to training session

(c) Partial compliance in participation

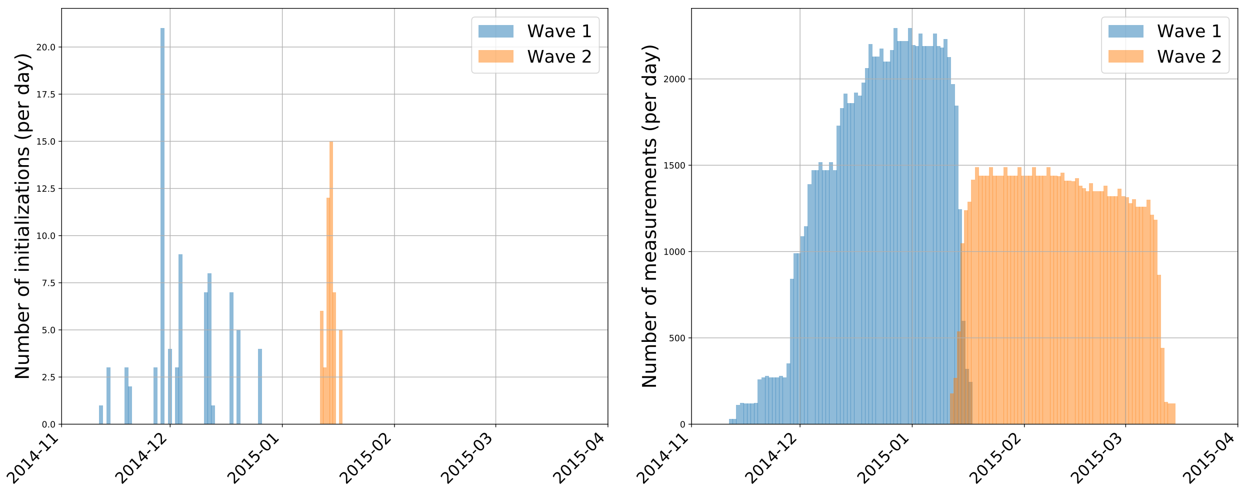
(d) Random assignment of peer's information

Figure 4: Timeline of the study



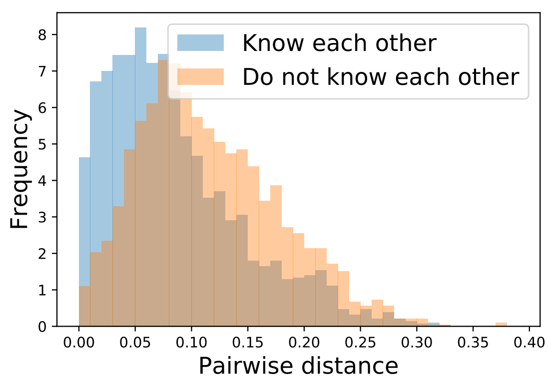
Note: The timeline refers to a typical woman who participated to the training session in a typical cluster

Figure 5: Timing of temperature measurements waves



Note: time profile of missions initializations (left) and of measurements density (right), distinguished by wave. A mission initialization denotes the beginning of up to 2048 measurements for a given SUM.

Figure 6: Distribution of pairwise distances between households



Note: Only distances and connections between women participating in a same experimental session are considered.

Table 1: Summary statistics and sample balance

	Invited	Non-invited	Diff	P-value
N. of observations	898	179		
Participated to the training session	0.461			
Participated in nudge experiment	0.393			
Endline survey not administered	0.066	0.162	-0.096	0.000
Baseline characteristics				
Respondent age	33.226	32.251	0.975	0.298
Live in couple	0.873	0.894	-0.021	0.431
Size of *gwa*	12.836	13.073	-0.236	0.721
No schooling	0.438	0.408	0.030	0.453
Primary school	0.147	0.151	-0.004	0.877
Secondary school	0.109	0.128	-0.019	0.445
High-school or above	0.306	0.313	-0.007	0.844
Have income generating activity	0.455	0.436	0.020	0.616
Weekly time working (hours)	6.373	5.056	1.317	0.108
Repondent monthly income (CFA)	19752	16538	3214	0.258
Wealth index	0.014	-0.068	0.082	0.632
Monthly fuel expenditure, *gwa* level (CFA)	13413	13574	-160	0.851
Personal savings	0.323	0.274	0.049	0.192
Risk averse, small stake	0.663	0.687	-0.025	0.514
Member of informal groups	0.546	0.536	0.009	0.803
ICS owned at the baseline	0.203	0.173	0.029	0.359
Know ICS	0.941	0.916	0.025	0.209
ICS is efficient, allows to save time and fuel	0.786	0.760	0.026	0.426
Outcomes				
Purchase ICS at the session (Sat)	0.175			
Purchase in five-day time (Thurs)	0.141			
Purchase ICS after intervention (Sat+Thurs)	0.314			
ICS owned after the session (Sat)	0.341	0.173	0.168	0.000
ICS owned in five-day time (Thurs)	0.455	0.173	0.282	0.000
ICS owned at the endline a	0.447	0.187	0.260	0.000
Know where one can buy ICS a	0.751	0.727	0.024	0.519
=1 if 20-40% expecteed saving from ICS usage a	0.241	0.213	0.027	0.458
ICS knowledge score, 0-4 a	2.741	2.553	0.188	0.017
Know people owning ICS a	0.535	0.373	0.162	0.000
Know people owning ICS: family or friends a	0.293	0.307	-0.013	0.725
Know people owning ICS: neigbours a	0.448	0.147	0.301	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.

Table 2: Summary statistics and sample balance, experimental session

	Info	No info	Diff	P-value
N	164	189		
Improved coal stove in the gwa	0.177	0.180	-0.003	0.922
Know ICS	0.951	0.958	-0.006	0.757
Efficient, allow save time and fuel	0.829	0.857	-0.028	0.463
Respondent age	35.354	34.709	0.645	0.604
Live in couple	0.872	0.868	0.004	0.889
Size of gwa	12.896	14.116	-1.220	0.187
No schooling	0.457	0.497	-0.040	0.445
Primary school	0.165	0.153	0.011	0.759
Secondary school	0.091	0.111	-0.020	0.533
High-school or above	0.287	0.238	0.048	0.296
Have income generating activity	0.470	0.434	0.036	0.493
Wealth index, all sample	-0.299	-0.297	-0.002	0.972
Personal savings	0.341	0.317	0.024	0.621
Risk averse, small stake	0.689	0.667	0.022	0.642
Member of informal groups	0.579	0.540	0.040	0.447
Normalized distance from drop-off point	0.824	0.791	0.034	0.445
# of women known by sight in the session	6.423	6.101	0.322	0.573
# of women whose respect opinion in the session	3.550	3.911	-0.360	0.439
Peer owned, bought or left deposit at session	0.695			
Relation with peer: known by sight	0.543			
Relation with peer: respect opinion	0.287			
Outcomes:				
Purchase ICS or leave deposit at session (Sat)	0.671	0.646	0.025	0.607
Purchase at the session	0.384	0.365	0.019	0.699
Leave the deposit at the session	0.287	0.280	0.006	0.880
Purchase ICS after 5 days (Thurs)	0.323	0.317	0.0006	0.891
Purchase ICS within experiment (Sat+Thurs)	0.707	0.672	0.035	0.466
Usage data from SUMS	0.244	0.175	0.069	0.107
Share of days of usage (SUMS)	0.352	0.354	-0.002	0.959
Avg daily time of usage (SUMS), in mins	94.154	88.116	6.038	0.782
Endline survey not administered	0.049	0.026	0.022	0.263
Reported high frequency usage (every day)	0.439	0.471	-0.032	0.539
Reported share of time of usage	0.471	0.496	-0.025	0.619

Note: The sample is based on 32 sessions where the experiment was implemented and 353 attendants who participated the final phase of the experiment. Of those, 340 were successfully tracked at the endline.

Table 3: ICS Usage: summary statistics

	N	mean	sd	min	max
Panel A: Monitored ICS Usage					
Extensive margin:					
Days of moniotoring	73	72.03	29.19	12.63	112.2
N. of days with at least one usage	73	23.25	20.72	0	62
Share of days of usage, over monitoring period	73	0.353	0.295	0	0.970
At least one usage event	73	0.726	0.449	0	1
Intensive margin:					
Time of usage, mins per day of usage above 50°C	53	267.5	114.9	111.1	698.2
N. of usage events per day of usage	53	2.791	0.830	1.048	5.016
Duration of usage event in day of usage, in mins	53	96.83	27.29	32.18	148.9
Panel B: Self-reported ICS Usage					
ICS owned at endline	340	0.656	0.476	0	1
Self-reported ICS usage at endline	223	0.960	0.197	0	1
Frequency of ICS use: always	214	0.467	0.500	0	1
Frequency of ICS use: daily	214	0.285	0.452	0	1
Frequency of ICS use: 3-4 times/week	214	0.0561	0.231	0	1
Frequency of ICS use: 1-2 times/week	214	0.0421	0.201	0	1
Frequency of ICS use: rarely	214	0.0794	0.271	0	1
Frequency of ICS use: never	214	0.0701	0.256	0	1
Share of time of usage	214	0.799	0.364	0	1

Table 4: Peer effects on ICS take-up

	Purchase ICS or left deposit (Sat)		Purchase ICS after 5 days (Thurs)		Purchase ICS within experiment (Sat+Thurs)	
	(1)	(2)	(3)	(4)	(5)	(6)
Received information on peer's purchase	-0.0758	-0.0940	0.0574	0.0249	0.0552	0.0569
	(0.0773)	(0.115)	(0.0757)	(0.112)	(0.0752)	(0.111)
Peer owned, bought or left deposit at session	0.142*	0.181	-0.0591	-0.00652	-0.0298	-0.0434
	(0.0814)	(0.124)	(0.0798)	(0.126)	(0.0804)	(0.123)
Relation with peer: known by sight		0.153		0.232		0.133
		(0.169)		(0.179)		(0.145)
Sight*Peer owned, bought or left deposit at session,		-0.265		-0.395*		-0.218
		(0.201)		(0.203)		(0.182)
Relation with peer: respect opinion		-0.202		-0.292*		-0.225
		(0.173)		(0.175)		(0.157)
Respect opinion*Peer owned, bought or left deposit at session		0.352*		0.558***		0.453**
		(0.211)		(0.211)		(0.194)
Constant	0.732***	0.722***	0.471***	0.459***	0.725***	0.720***
	(0.122)	(0.124)	(0.173)	(0.174)	(0.141)	(0.141)
Observations	353	353	353	353	353	353
R-squared	0.123	0.131	0.054	0.075	0.068	0.082
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dependent Variable	0.646	0.646	0.317	0.317	0.672	0.672

Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Mean dependent variable refers to the control group. Individual controls include 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, risk aversion, knowing about ICS and owning already an ICS.

Table 5: Peer effects on ICS usage

	Share of days of usage (SUMS)		Share of time of usage (SUMS)		High frequency usage (reported)		Share of time of usage (reported)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Received information on peer's purchase	0.106	0.332*	35.72	85.63	-0.105	-0.144	-0.0860	-0.101
	(0.152)	(0.178)	(49.59)	(71.09)	(0.0770)	(0.109)	(0.0745)	(0.105)
Peer owned, bought or left deposit	-0.106	-0.366*	-23.71	-87.90	0.101	0.143	0.0856	0.109
	(0.144)	(0.184)	(44.02)	(73.04)	(0.0840)	(0.124)	(0.0809)	(0.120)
Relation with peer: known by sight		0.0607		11.91		0.108		0.0474
		(0.172)		(50.80)		(0.163)		(0.157)
Sight*Peer owned, bought or left deposit						-0.267		-0.213
						(0.195)		(0.187)
Relation with peer: respect opinion		-0.628**		-131.3		-0.0762		-0.0469
		(0.258)		(97.75)		(0.173)		(0.169)
Respect opinion*Peer owned, bought or left deposit		0.663***		173.0*		0.396*		0.362*
		(0.237)		(97.23)		(0.219)		(0.210)
Constant	0.419**	0.470***	78.86*	93.61*	0.259	0.257	0.300*	0.293*
	(0.163)	(0.160)	(45.66)	(48.66)	(0.177)	(0.177)	(0.165)	(0.165)
Observations	73	73	73	73	331	331	331	331
R-squared	0.181	0.270	0.170	0.218	0.162	0.179	0.169	0.186
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dependent Variable	0.354	0.354	88.12	88.12	0.497	0.497	0.497	0.497

Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Individual controls as in table 4. The coefficients of "Sight*Peer owned, bought or left deposit" in columns 2 and 4 are omitted because of collinearity.

Table 6: The impact of invitation to the session and spillover effects

	ICS ownership at the endline			
	All		Non-participant & control	Non- buying participants & control
	(1)	(2)	(3)	(4)
Invited at the training session	0.310***		0.0854**	0.207***
	(0.0409)		(0.0414)	(0.0588)
Participated to the training session		0.671***		
		(0.0857)		
Constant	-0.0739	-0.0205	-0.0201	0.0769
	(0.0951)	(0.0857)	(0.103)	(0.152)
Observations	989	989	587	277
R-squared	0.150	0.264	0.231	0.235
Controls	Yes	Yes	Yes	Yes
Lee lower bound	0.197***		0.00295	0.00581
	(0.0427)		(0.0487)	(0.0638)
Lee upper bound	0.312***		0.0826***	0.145***
	(0.0409)		(0.0402)	(0.0573)
Mean Dependent Variable	0.194	0.194	0.194	0.181
Weak identification F stat		374.6		

Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Individual controls as in table 4. The mean dependent variable indicates the average share of women owning ICS at the baseline for the sample considered. All specifications are obtained via OLS, but column 2 via IV.

Table 7: Sample balance by density of neighbors in the radius

	All invited (n=839)			5-days delay visit (n=258)			Non-buying participants (n=127)			Non-participants (n=437)		
	Above median	Below median	P value	Above median	Below median	P value	Above median	Below median	P value	Above median	Below median	P value
Panel A: $\alpha = 0.5$												
Respondent age	33.320	33.282	0.963	33.175	33.947	0.596	31.667	34.156	0.189	32.636	32.284	0.749
Live in couple	0.869	0.873	0.858	0.849	0.886	0.380	0.841	0.844	0.970	0.889	0.878	0.730
Size of *gwa*	12.311	13.565	0.035	12.952	14.318	0.236	13.508	12.547	0.560	12.029	12.770	0.354
Primary school	0.146	0.150	0.868	0.143	0.091	0.195	0.111	0.094	0.749	0.140	0.152	0.722
Secondary school	0.106	0.113	0.756	0.119	0.121	0.958	0.111	0.125	0.810	0.106	0.117	0.714
High-school or above	0.331	0.295	0.262	0.270	0.220	0.351	0.333	0.234	0.219	0.377	0.352	0.594
Have income generating activity	0.467	0.459	0.813	0.444	0.470	0.685	0.460	0.531	0.428	0.498	0.457	0.392
Wealth index, all sample	0.025	0.018	0.961	-0.341	-0.745	0.132	-0.677	-0.895	0.580	0.275	0.252	0.909
Personal savings	0.323	0.339	0.640	0.333	0.326	0.898	0.333	0.313	0.804	0.353	0.343	0.841
Risk averse, small stake	0.657	0.673	0.624	0.675	0.674	0.995	0.683	0.688	0.952	0.652	0.661	0.849
Member of informal groups	0.546	0.558	0.729	0.532	0.545	0.826	0.508	0.516	0.932	0.560	0.561	0.992
ICS owned	0.183	0.214	0.253	0.175	0.197	0.646	0.175	0.219	0.535	0.193	0.213	0.609
Panel B: $\alpha = 1$												
Respondent age	33.106	33.506	0.622	33.715	33.405	0.832	32.063	33.766	0.370	32.049	32.877	0.450
Live in couple	0.884	0.857	0.248	0.854	0.884	0.475	0.810	0.875	0.315	0.907	0.858	0.117
Size of *gwa*	12.789	13.140	0.556	13.080	14.298	0.292	14.048	12.016	0.217	12.516	12.316	0.803
Primary school	0.127	0.170	0.085	0.153	0.074	0.049	0.127	0.078	0.368	0.129	0.165	0.286
Secondary school	0.113	0.106	0.719	0.095	0.149	0.186	0.095	0.141	0.432	0.133	0.090	0.148
High-school or above	0.338	0.285	0.098	0.270	0.215	0.305	0.317	0.250	0.403	0.378	0.349	0.534
Have income generating activity	0.468	0.457	0.759	0.467	0.446	0.738	0.524	0.469	0.539	0.511	0.439	0.130
Wealth index, all sample	0.003	0.041	0.799	-0.475	-0.631	0.562	-0.782	-0.792	0.978	0.274	0.250	0.905
Personal savings	0.299	0.366	0.038	0.292	0.372	0.174	0.222	0.422	0.016	0.342	0.354	0.801
Risk averse, small stake	0.644	0.688	0.173	0.650	0.702	0.368	0.635	0.734	0.231	0.631	0.684	0.246
Member of informal groups	0.519	0.587	0.046	0.496	0.587	0.147	0.444	0.578	0.134	0.551	0.571	0.680
ICS owned	0.197	0.201	0.864	0.190	0.182	0.870	0.190	0.203	0.859	0.209	0.198	0.780

The table reports the mean characteristics of women with the number of neighbors invited at the session in the radius being above and below the cluster-specific median number of neighbors. Radius is calculated as α*average pairwise distance in the cluster. Results are reported for $\alpha = 0.5$ (Panel A) and $\alpha = 1$ (Panel B).

Table 8: Social interaction effects on ICS purchase with 5 day-delay, $\alpha = 0.5$

	Purchase ICS with 5-day delay			# of purchasing neighbours at session	# of neighbours owning ICS after session
	OLS	IV	IV	First stage	
	(1)	(2)	(3)	(4)	(5)
# of invited neighbours, mean=5.13	0.0277**			0.220***	0.412***
	(0.0116)			(0.0567)	(0.0615)
# of purchasing neighbours at the session (Sat), mean=.94		0.126*			
		(0.0664)			
# of neighbours owning ICS after the session (Sat), mean=1.82			0.0673**		
			(0.0290)		
Leave the deposit at the session	0.493***	0.491***	0.471***		
	(0.0652)	(0.0618)	(0.0607)		
Discussed with other attendands about purchase before Thurs	0.289***	0.282***	0.310***		
	(0.0827)	(0.0837)	(0.0742)		
Constant	0.0439	0.157	0.0902	-0.898	-0.688
	(0.309)	(0.280)	(0.289)	(1.017)	(0.838)
Observations	258	258	258	258	258
R-squared	0.385	0.337	0.376	0.277	0.488
Controls	Yes	Yes	Yes	Yes	Yes
Mean Dependent Variable	0.484	0.484	0.484	0.891	1.694
Weak identification F stat				15.01	44.87

Standard errors clustered by sampling point in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Neighbors are considered as people living within a distance alpha*the average pairwise distance in the sampling point, with alpha=0.5 Weak identification test is based on Kleibergen-Paap rk Wald F statistic.

Table 9: Social interaction effects on ICS ownership, sample of buying participants, $\alpha = 0.5$

	ICS ownerswhip at endline			# of purchasing neighbours (Sat+Thurs)	# of neighbours owning ICS after the session (Sat+Thurs)
	OLS	IV	IV	First step	
	(1)	(2)	(3)	(4)	(5)
# of invited neighbours, mean=5.13	0.00438			0.468***	0.605***
	(0.0114)			(0.0577)	(0.0557)
# of purchasing neighbours (Sat+Thurs), mean=1.72		0.00936			
		(0.0230)			
# of neighbours owning ICS after the session (Sat+Thur), mean=2.46			0.00724		
			(0.0180)		
Constant	0.322*	0.336*	0.327*	-1.424	-0.668
	(0.184)	(0.182)	(0.177)	(1.063)	(0.802)
Observations	275	275	275	275	275
R-squared	0.138	0.141	0.138	0.432	0.584
Controls	Yes	Yes	Yes	Yes	Yes
Mean Dependent Variable	0.193	0.193	0.193	2.273	2.964
Weak identification F stat				65.72	117.7

See Table 8.

Table 10: Social interaction effects on ICS ownership, sample of non-buying participants, α=0.5

	ICS ownerswhip at endline			# of purchasing neighbours (Sat+Thurs)	# of neighbours owning ICS after the session (Sat+Thurs)
	OLS	IV	IV	First step	
	(1)	(2)	(3)	(4)	(5)
# of invited neighbours, mean=5.13	0.0243*			0.505***	0.627***
	(0.0130)			(0.0953)	(0.104)
# of purchasing neighbours (Sat+Thurs), mean=1.72		0.0481*			
		(0.0262)			
# of neighbours owning ICS after the session (Sat+Thur), mean=2.46			0.0388*		
			(0.0215)		
Constant	-0.468	-0.401	-0.423	-1.394	-1.153
	(0.360)	(0.313)	(0.324)	(1.599)	(1.517)
Observations	127	127	127	127	127
R-squared	0.389	0.376	0.379	0.475	0.581
Controls	Yes	Yes	Yes	Yes	Yes
Mean Dependent Variable	0.197	0.197	0.197	1.551	2.118
Weak identification F stat				28.14	36.27

See Table 8.

Table 11: Social interaction effects on ICS ownership, sample of non-participants, α=0.5

	ICS ownerswhip at endline			# of purchasing neighbours (Sat+Thurs)	# of neighbours owning ICS after the session (Sat+Thurs)
	OLS	IV	IV	First step	
	(1)	(2)	(3)	(4)	(5)
# of invited neighbours, mean=5.13	0.00283			0.355***	0.550***
	(0.00779)			(0.0488)	(0.0484)
# of purchasing neighbours (Sat+Thurs), mean=1.72		0.00795			
		(0.0213)			
# of neighbours owning ICS after the session (Sat+Thur), mean=2.46			0.00514		
			(0.0137)		
Constant	0.0477	0.0462	0.0467	0.184	0.177
	(0.162)	(0.157)	(0.156)	(0.731)	(0.709)
Observations	437	437	437	437	437
R-squared	0.250	0.249	0.250	0.322	0.524
Controls	Yes	Yes	Yes	Yes	Yes
Mean Dependent Variable	0.204	0.204	0.204	1.478	2.297
Weak identification F stat				53.05	129.1

See Table 8.

Table 12: Information and knowledge about ICS

	Δ_t Know ICS	Δ_t ICS is efficient	Know where to buy ICS	Correct estimate of fuel saving (20-40%)	ICS knowledge score (0-4)	Know people owning ICS		
						All	Relatives and friends	Neighbours
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Invited at the training session	0.0502	0.0770	-0.0113	0.0659	0.175	0.268***	0.0691	0.337***
	(0.0578)	(0.0824)	(0.0694)	(0.0655)	(0.143)	(0.0781)	(0.0735)	(0.0736)
Constant	0.0515	-0.197**	0.573***	0.162**	2.149***	0.244***	0.155*	0.0204
	(0.0714)	(0.100)	(0.0795)	(0.0755)	(0.164)	(0.0878)	(0.0800)	(0.0811)
Observations	989	989	989	989	989	989	989	989
R-squared	0.031	0.030	0.023	0.019	0.032	0.039	0.028	0.066
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean Dependent Variable	0.937	0.793	0.727	0.213	2.553	0.373	0.307	0.147

Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. 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. Mean dependent variable for columns 3 to 8 is the unconditional mean for the control group at the endline.

Table 13: Impact of ICS ownership on welfare

	ICS owned after the session	Δ_t Monthly fuel expenditure, *gwa*		Δ_t Has income generating activity		Δ_t Weekly time working (in hours)		Δ_t Individual monthly income		Expecteed over-saving from ICS usage (>40%)	
	1st stage	ITT	LATE	ITT	LATE	ITT	LATE	ITT	LATE	ITT	LATE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Invited at the training session	0.273***	-3,541		0.0192		-2.675		-7,350		0.168**	
	(0.0567)	(2,626)		(0.0811)		(1.864)		(6,317)		(0.0772)	
ICS owned after the session			-13,385		0.0703		-9.828		-28,660		0.615
			(12,406)		(0.464)		(10.76)		(31,628)		(0.421)
Constant	-0.0441	5,258*	4,577	0.277**	0.280**	-3.579	-4.012	4,373	3,517	0.499***	0.526***
	(0.0857)	(2,998)	(3,009)	(0.110)	(0.121)	(2.665)	(4.151)	(7,238)	(7,738)	(0.107)	(0.116)
Observations	989	971	971	989	989	987	987	823	823	989	989
R-squared	0.395	0.031	-0.026	0.396	0.396	0.162	0.061	0.207	0.088	0.060	-0.195
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lee lower bound	0.239										
	(0.0415)										
Lee upper bound	0.353										
	(0.0411)										
Mean Dependent Variable	0.167	13659	13659	0.453	0.453	5.409	5.409	15732	15732	0.467	0.467
Weak identification F stat			8.041		9.126		9.033		7.614		9.126

Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Weak identification test is based on Kleibergen-Paap rk Wald F statistic

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Appendix

A. Sampling design and survey protocols

A.1. Selection of 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 commune. We use Google maps to delimit each of the six communes and then overlay rectangles/clusters within each of them. It is worth noting that non-residential areas such as industrial zones, parks, rivers, ponds, sports areas etc. are excluded during this process. This exclusion takes place before the actual selection of clusters via simple random sampling. In the course of overlaying the grids, we try our best to ensure that the rectangles cover actual blocks of houses and are uniform in size.

Within each commune, each grid cell is then assigned a number, and a random number generator is used to select the actual clusters involved in the experiment. The number of clusters selected 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.

It is also worth mentioning that wealthy neighborhoods are excluded from the sampling, and whenever a randomly selected cluster is deemed too wealthy to be relevant for the study of energy poverty, a replacement cluster is selected within the same commune. Although such a scheme leads to a sample which is not fully representative of the entire population of Bamako, we can safely assume that selected clusters are representative of the population of interest for our study, i.e. non-wealthy families using cookstoves.

A.2. Sampling starting points

Once a cluster is selected, we then proceed to the selection of a starting point inside it. The selection of the starting point 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 commune 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.

The day before the survey, an advance team of supervisors uses first Google Earth and then a GPS device to determine the starting point on the field. The advance team takes pictures and notes landmark points for the subsequent deployment of the survey teams. When the 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, the advance team always goes to the field with a back-up starting point which we use in such circumstances.

A.3. Selection of households

The advance team proceeds with the selection of households which will be assigned to enumerators in the next day. The direction in which to start the selection is chosen by turning away from the closest line of the grid (border of the rectangular cluster) on the map, and looking right from that position. We choose this method to ensure that in all neighborhoods, the selected households fall within the starting point's cluster. Since the starting point is chosen at random and in some cases is at the edge of the cluster, randomly choosing a direction could in practice lead to the selection of households outside of the grid. In particular, this method also ensures that control households fall within the same neighborhood (as described in the next paragraph). It is worth stressing that this selection method is random since the starting point from which the closest side of the grid is based on is itself random.

Once the first direction is chosen, the second direction selected is the opposite one. Starting from the right hand side (first direction), contiguous, inhabited households on either side of the walking direction are selected and assigned alpha-numeric IDs. 30 households are selected within the neighborhood: 25 as initial sample and the remaining 5 as replacements. 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. If this process leads to the initial starting point without reaching the desired count, the team then moves to the end of the block where it initially turned right and proceeds instead to the next block.

Once the household selection in the treatment cluster is complete, to determine the control area the advance team goes back to the starting point, again facing away from the closest line in the grid, and walks straight for 10 minutes. Whenever obstacles prevented the straight line walk, the team alternated between turning right and left. The position at the end of the ten minutes walk is the starting point for the control neighborhood. The direction to the right is the first one while the one on the left is the second one. Five households are selected in either direction, for a total of 10 households. Once again, an alpha-numeric numbering system is used to select contiguous households, with a process analogous to the one described for the treatment cluster.

A.4. Baseline survey protocol

Few days after the identification work by the advance team, a team of enumerators reaches the group of selected households, which are each identified by its GPS coordinates. The enumerator entering a house, after introducing him/herself and shortly describing the aim of the project, asks to talk with the women responsible of the cooking rotation (the women who is most knowledgeable about the family's meal decisions). He/she asks for the consent and proceeds with the survey. For households in the treatment clusters, at the end of the survey he/she communicates that the women is invited to attend a training session on the use and advantages of ICS. The sessions are held in a venue of the neighborhood and women are told that will receive 1,000 CFA reimbursement upon personal participation showing the nominal coupon which is given. For the control clusters, no invitation is given to the interviewee.

If the targeted individual is not at home, the enumerator will 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 find the proper interlocutor within the household, a replacement procedure for the household kicks in. The field supervisor replaces the household with the first household available in the roster selected for that end by the advance team.

A.5. Endline protocol

The endline survey protocol follows three steps, performed in two consecutive days.

First, for each cluster, an advance team uses 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, the advanced team notifies them about the visit of enumerators in the next day. This process is completed for both control and treatment areas. The enumerators spend the day in the neighborhood to ensure that women who were temporarily absent are interviewed upon their return. When the targeted woman is absent for a long period, we use a replacement procedure by looking for the oldest woman within the same *gwa* who is knowledgeable about the cooking decisions.

In the second step, the advance team drops a team of enumerators at each starting point to administer the follow-up questionnaire. A member of the advance team who reconstituted the households the day before walks each enumerator to their respective assignments to ensure that the women who have been previously identified are the ones surveyed.

B. Missing data, attrition and partial compliance

The study is characterized by different degrees of data completeness which influence the 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.

As far as attrition is concerned, 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. We are not aware of any systematic process of refusal going on in control areas. The protocol for household and woman identification, using baseline information, has been followed throughout the endline survey administration. The most common reasons for attrition were related to the temporary or permanent displacement of women and a few cases of death. Moreover, attriters are more likely to live in larger *gwa*, while they do not show other unbalanced along other observable characteristics. This is shown in column one of table B.1. In order to test the extent to which differential attrition has an impact on the internal validity of our results, we implement Lee bounds (Lee, 2009) which are reported below the estimates of interest in tables 6 and 13.

About 46% of women invited to the session actually attended, with an average of 11 women per session. Participation is the outcome of a self-selection process. Column 2 of table B.1 shows that participants were on average older, living in larger *gwa*, less educated and less wealthy than those who did not attend. In order to identify the causal effect of the training/marketing session, we estimate the Intention to Treat (ITT) and Local Average Treatment Effect (LATE), in order to account for partial compliance.

In 32 out of 36 training sessions, the peer information treatment was not correctly implemented due to technical issues. The specific sessions were concentrated in a particular date and in a particular geographical 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 such cases are not included in the sample for such exercise. Though, such exclusion has an impact on the external validity. Column 3 of table B.1 shows that participants to marketing/traning sessions where the peer information experiment was rightly 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 variables reported in tables 4 and 5 (results not shown but available upon request).

Finally, 14 women (3.9%) who attended the training/marketing session were not involved in the 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 4 of table B.1 shows that these women were slightly older and more knowledgeable about ICS. As such, they are excluded from the analysis of the peer information treatment but are included in the rest of the analysis.

Table B.1: Attrition and partial compliance

	Attriter vs whole sample (1)	Attendedant vs Invited to the session (2)	Participant to peer info experiment vs Attendant (3)	Reached final phase vs Participant to peer info experiment (4)
Invited at the training session	-0.0968***			
	(0.0289)			
Respondent age	-3.45e-05	0.00375***	0.00275**	0.00179**
	(0.000760)	(0.00144)	(0.00123)	(0.000733)
Live in couple	-0.00430	-0.0969*	0.0201	0.0464
	(0.0263)	(0.0514)	(0.0466)	(0.0374)
Size of *gwa*	-0.00209**	0.00549***	0.00191	-0.00139
	(0.000887)	(0.00201)	(0.00202)	(0.00130)
Primary school	-0.0168	-0.0287	0.0876**	0.0102
	(0.0251)	(0.0507)	(0.0399)	(0.0274)
Secondary school	0.000281	-0.0781	0.0119	0.0228
	(0.0296)	(0.0561)	(0.0610)	(0.0364)
High-school or above	-0.0261	-0.123***	0.0911**	0.0170
	(0.0211)	(0.0429)	(0.0393)	(0.0255)
Have income generating activity	-0.0177	-0.0346	-0.00307	-0.0145
	(0.0187)	(0.0378)	(0.0387)	(0.0285)
Wealth index, all sample	0.00238	-0.0302***	-0.00415	-0.00456
	(0.00438)	(0.00861)	(0.00789)	(0.00581)
Personal savings	-0.0204	0.00912	0.0561	-0.0121
	(0.0199)	(0.0413)	(0.0419)	(0.0289)
Risk averse, small stake	-0.00375	-0.0246	0.0158	0.0232
	(0.0180)	(0.0356)	(0.0342)	(0.0229)
Member of informal groups	-0.0152	0.0157	-0.000638	0.0362
	(0.0197)	(0.0382)	(0.0359)	(0.0293)
Know ICS	0.00623	0.0923	-0.0722	0.156*
	(0.0372)	(0.0763)	(0.0519)	(0.0897)
Improved coal stove in the *gwa*	0.0246	0.0410	-0.102**	0.00612
	(0.0240)	(0.0435)	(0.0493)	(0.0261)
Constant	0.219***	0.328***	0.771***	0.696***
	(0.0587)	(0.102)	(0.0922)	(0.117)
Observations	1,077	898	415	367
R-squared	0.029	0.045	0.052	0.076
Mean Dependent Variable	0.081	0.462	0.884	0.961

Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The first row in the header refers to the outcome variable, while the second reports the sample of reference.

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 session (both Saturday and Thursday). 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 SUM installation. 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, seem to significantly predict SUM installation.

Out of 100 SUMs installed, we were able to successfully obtain data (from at least one wave[58]) for 75 of them (25% attrition rate). The main reasons for the attrition are breakage (15 cases), loss/inability to find the SUM (6 cases), absence of 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 the SUMs producer (Berkley Air Monitoring Group) and the best practices from other studies. However, differently from what happens in 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, which may cause their damage or loss. Column 2 of table C.1 reports the determinants of SUM data availability in the sample of installed ones. One may notice that the only significant predictor is the size of the extended household (negatively). The consequences of this fact on the estimate of effective ICS usage depend on the correlation between *gwa* size and usage. If larger *gwa* use ICS more often or more intensively, then our sample, by under-representing them, would lead to under-estimating effective usage. Conversely, if ICS are used less, for example because more women participate the cooking rotation, then we would over-estimate effective usage. The correlation between high self-reported usage and *gwa* size on the sample of assigned SUMs is negative, which makes the second explanation more credible.

In order to establish the relationship between ICS usage, as measured through SUMs, and self-reported usage, as from the endline questionnaire, we assess survey data completeness. Self-reported usage of ICS was asked to all women reporting to personally own and use ICS[59]. This leads to some missing data (2.6%) for the cases when ICS was present at the level of *gwa*, but not under the exclusive use and control of the respondent. Column 3 shows the determinants of reported data completeness on the sample of women who purchased ICS at the session whose ICS is still present at the endline. Once again, no systematic attrition process seems to arise along observable characteristics. Finally, column 4 shows the extent to which the available data from both the monitoring exercise and the self-reported variables are representative of the sample of ICS purchasers at the session who still own it at the endline. One can notice that the sample appears unbalanced with respect to the the size of *gwa* and the level of education of the respondent (women with no schooling, the omitted dummy, are over-represented). However, ICS usage and level of schooling appear relatively uncorrelated (considering both the samples for measured and self-reported usage).

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

[59]This was done to maximize the precision of the responses.

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

	SUMS installed vs ICS purchasers at session (1)	SUMS data collected vs SUMS installed (2)	Non-missing reported usage vs ICS owned at endline from session participants (3)
Received information on peer's purchase	0.0761	0.0955	-0.00272
	(0.0643)	(0.0889)	(0.0264)
Purchases after 5 days	0.0288	-0.116	-0.0132
	(0.0677)	(0.0925)	(0.0306)
Respondent age	-0.00319	0.00334	-0.000592
	(0.00257)	(0.00355)	(0.000999)
Live in couple	-0.170	0.105	-0.0352**
	(0.107)	(0.126)	(0.0171)
Size of *gwa*	-0.00218	-0.0105	-0.000441
	(0.00405)	(0.00655)	(0.00232)
Primary school	-0.0915	-0.214	-0.00126
	(0.0916)	(0.133)	(0.0353)
Secondary school	-0.326***	-0.128	-0.0905
	(0.101)	(0.253)	(0.0742)
High-school or above	-0.0700	-0.0749	-0.0227
	(0.0849)	(0.104)	(0.0315)
Have income generating activity	-0.0484	0.0658	0.00703
	(0.0757)	(0.105)	(0.0275)
Wealth index, all sample	0.000636	-0.0163	0.0121*
	(0.0170)	(0.0254)	(0.00624)
Personal savings	0.0647	0.0373	0.0353
	(0.0823)	(0.106)	(0.0342)
Risk averse, small stake	-0.0819	-0.0393	0.0155
	(0.0739)	(0.0991)	(0.0341)
Member of informal groups	-0.0583	0.00954	0.00160
	(0.0817)	(0.106)	(0.0311)
Improved coal stove in the *gwa*	0.104	0.0698	0.0269*
	(0.0926)	(0.115)	(0.0156)
Constant	0.797***	0.709***	1.006***
	(0.179)	(0.216)	(0.0509)
Observations	236	98	223
R-squared	0.075	0.132	0.064
Mean Dependent Variable	0.415	0.745	0.960

Standard errors clustered by sampling point in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

C.2. Measurement of usage

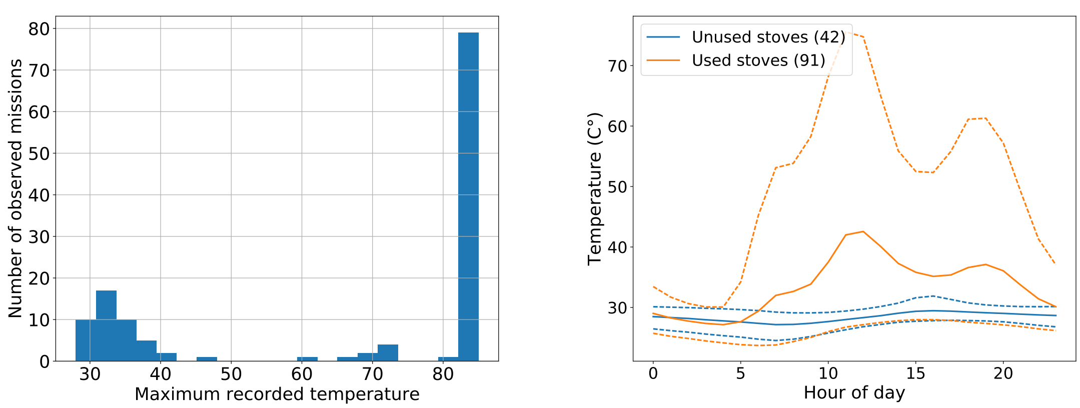
We first observe that the distribution of maximum temperatures measured in each mission is strongly bimodal (Figure C.1, left). We hence first classify as "clearly used" all SUMs reaching a temperature above 80 degrees. We then focus on SUMs not included in such sample and observe that temperature changes from one period to the next are, as expected, strongly concentrated around 0, with 0.5%th and 99.5%th percentiles of respectively -1°C. and 1.5°C. We then define a distinct usage as a temperature peak such that:

1. temperature is over 35°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.

While condition 1 might seem too relaxed, its relevance is minor, since condition 3 is instead very conservative, when compared to percentiles of temperature differentials.[60] Figure C.1 plots the intra-day distribution of measurements comparing SUMs featuring at least one instance of usage with other SUMs.

The algorithm just described records cookstove usage at the *extensive* margin: we also derive an *intensive* measure by looking at the raw share of measurements for which temperature is over 50°C: Figure C.2 (right) features the distribution of such measure for missions in which at least one use was recorded, highlighting the fact that the vast majority of used stoves was employed between 150 and 400 minutes per day.

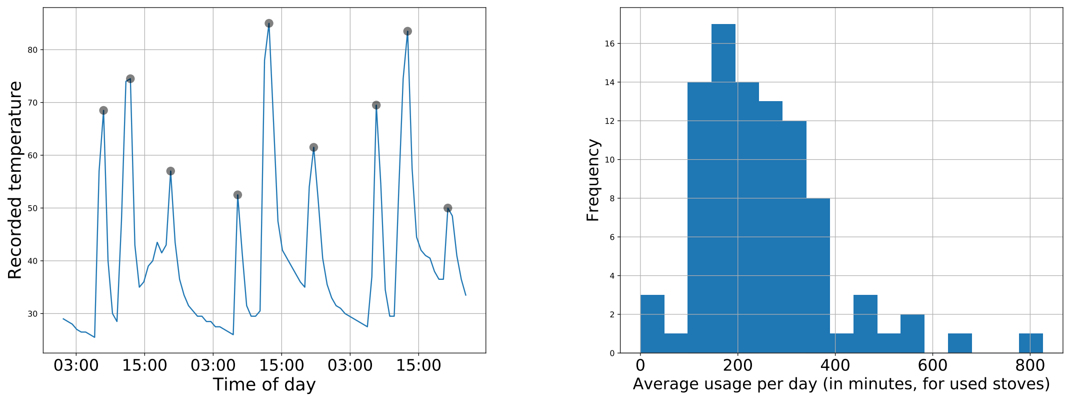
Figure C.1: Peak and average temperatures



Note: maximum temperature reached during each mission (left) and distribution of temperatures over time, classified depending on whether at least one usage was detected over the whole mission, together with 90% confidence intervals (right).

[60] On the other hand, the low threshold in condition 1 is justified by the clear evidence of cooking events reaching peak temperatures some time in the 47 minutes *between* two subsequent measurements. Such events would go uncaught if we raised the threshold.

Figure C.2: Example of the use detection algorithm



Note: example of the output of the use detection algorithm, ran over three days of usage of a single stove, where each circle denotes a detected use (left); average daily usage for used stoves (right).

C.3. Monitored vs self-reported usage

Table C.2 shows the results of a set of regressions where the dependent variables are different measures of monitored usage, namely the share of days of usage, the probability of experiencing at least one cooking event and the average time of usage. We use two measures of self-reported usage, namely the six dummies obtained from the questionnaire and the continuous variable "share of time of usage" as regressors, together with the usual controls used throughout the paper. We find that reported usage significantly predicts monitored usage in all specifications.

Table C.2: Monitored vs reported ICS usage

	Share of days of usage over monitoring period		Avg time of usage	
	(1)	(2)	(3)	(4)
Share of time of usage, reported	0.355***		76.79***	
	(0.0797)		(28.31)	
Frequency of ICS use: always		0.339***		62.03**
		(0.0913)		(30.04)
Frequency of ICS use: daily		0.481***		110.3***
		(0.107)		(37.53)
Frequency of ICS use: 3-4 times/week		0.495***		117.3**
		(0.116)		(45.11)
Frequency of ICS use: 1-2 times/week		0.168*		50.59
		(0.0952)		(34.68)
Frequency of ICS use: rarely		0.0140		-14.52
		(0.0807)		(39.09)
Constant	0.0968	0.0592	28.76	24.08
	(0.172)	(0.183)	(51.30)	(53.59)
Observations	73	73	73	73
R-squared	0.315	0.440	0.224	0.317
Controls	Yes	Yes	Yes	Yes
Mean Dependent Variable	0.353	0.353	91.42	91.42

Robust standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

D. Placebo tests

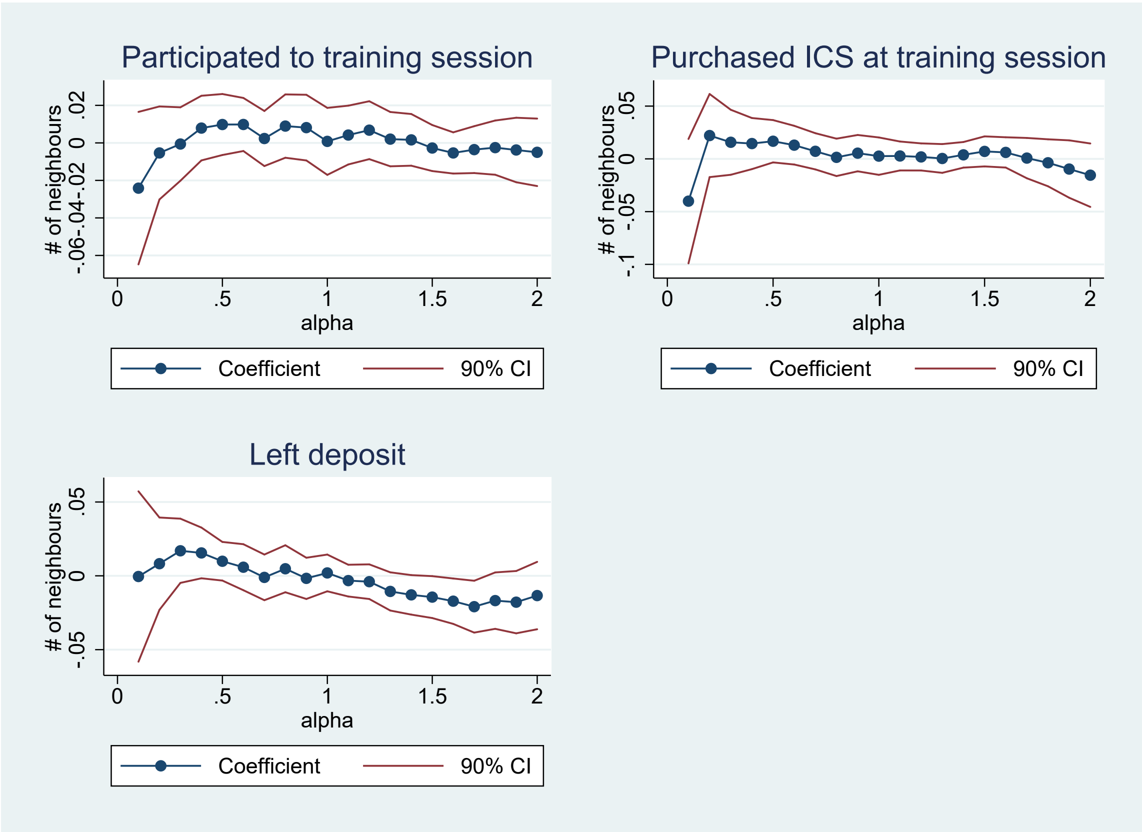
We run placebo regressions to test the goodness of our instrument. We estimate regression 4 on a set of outcomes which are pre-determined to the social interaction dynamic we are studying. In particular, we consider i. the individual participation to the session, ii. ICS take-up rate at the end of the training/marketing session and iii. the probability of leaving the deposit. The exercise is done on the different sub-samples of interest: all women invited (point i.) and participants (point ii. and iii.). Results for α=0.5 are shown in Table D.1, while sensitivity analysis is presented in Figure D.1. The instrument does not seem to have any significant influence on participation

Table D.1: Placebo regression on participation, ICS purchase at the training session and leaving deposit

	Attended the training session (1)	Purchase ICS at training session (2)	Left deposit (3)
# of invited neighbours, mean=5.13	0.00981	0.0167	0.00988
	(0.00988)	(0.0122)	(0.00796)
Constant	0.294	0.159	0.354
	(0.187)	(0.224)	(0.215)
Observations	898	415	415
R-squared	0.048	0.086	0.079
Controls	Yes	Yes	Yes
Mean Dependent Variable	0.462	0.378	0.282

Standard errors clustered by sampling point in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

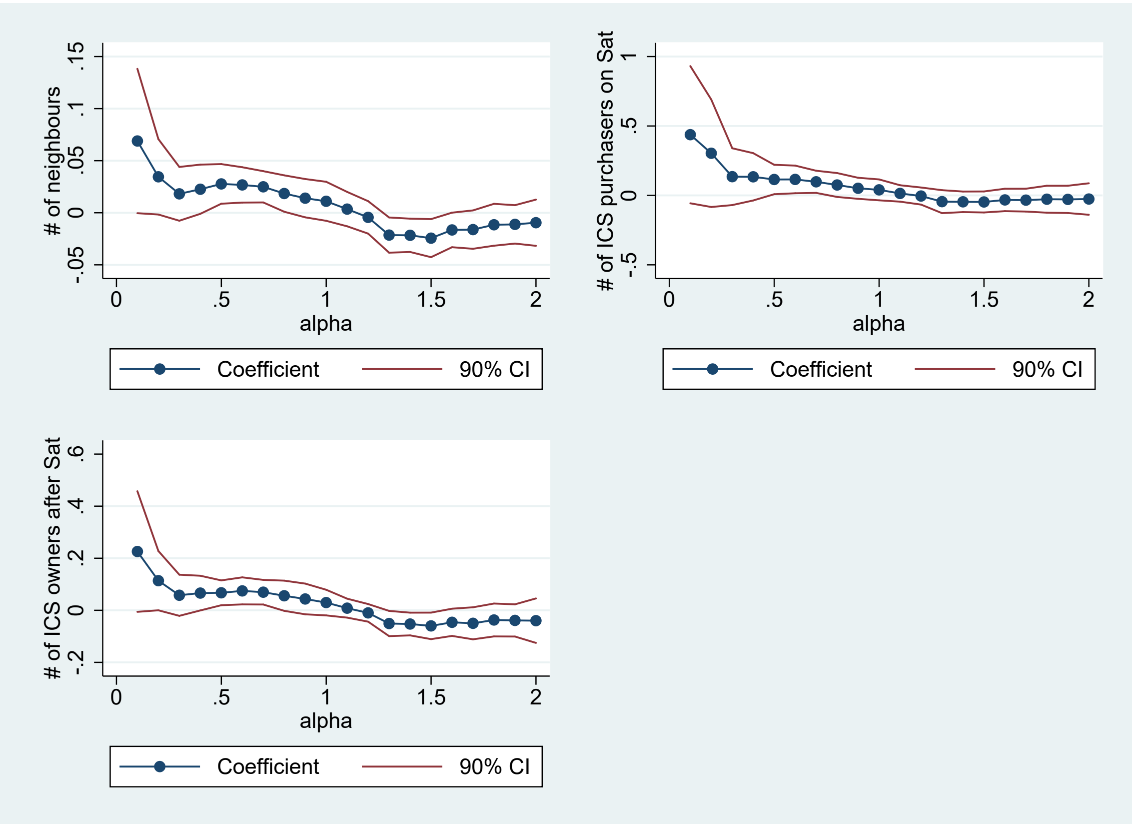
Figure D.1: Social interaction effects on session attendance, propensity to purchase ICS and leave the deposit during the session, sensitivity analysis on α



Note: Coefficients are derived from estimations as in Table 9.

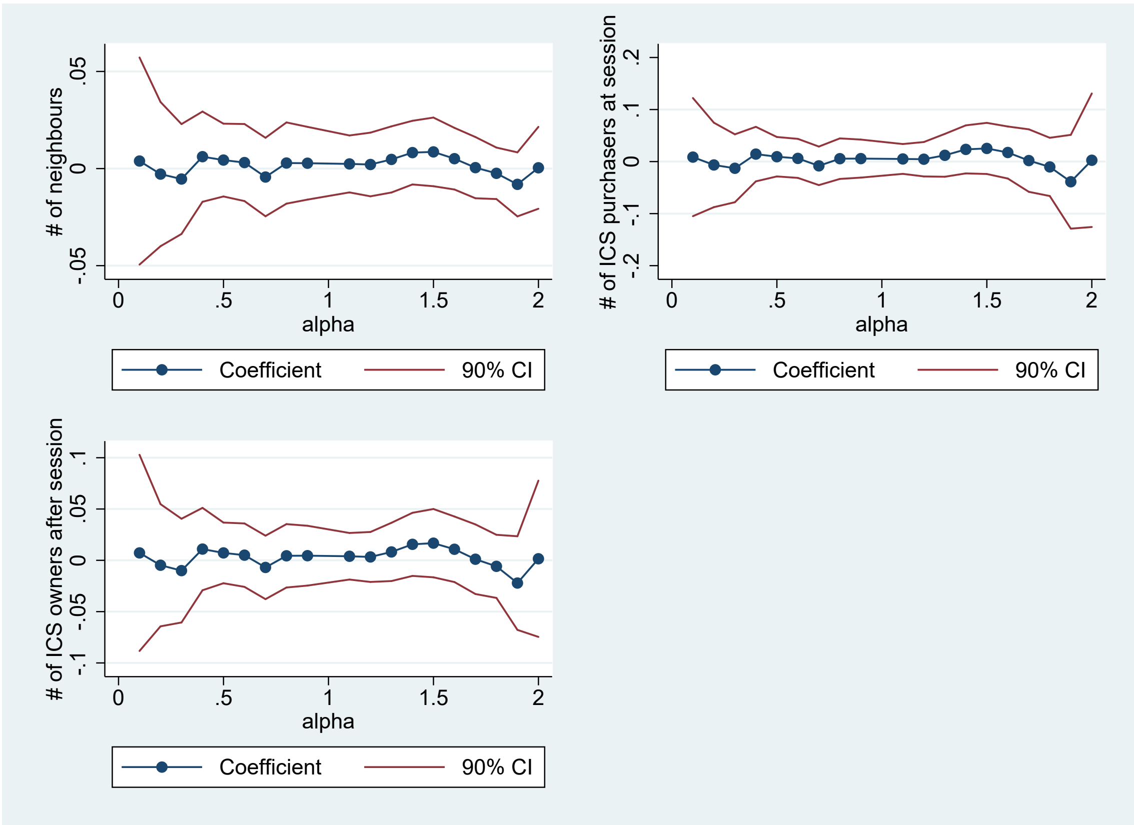
E. Sensitivity analysis on α

Figure E.1: Social interaction effects on purchase with five-day delay (Thursday), sample of non-buying participants at the session (Sat), sensitivity analysis on α



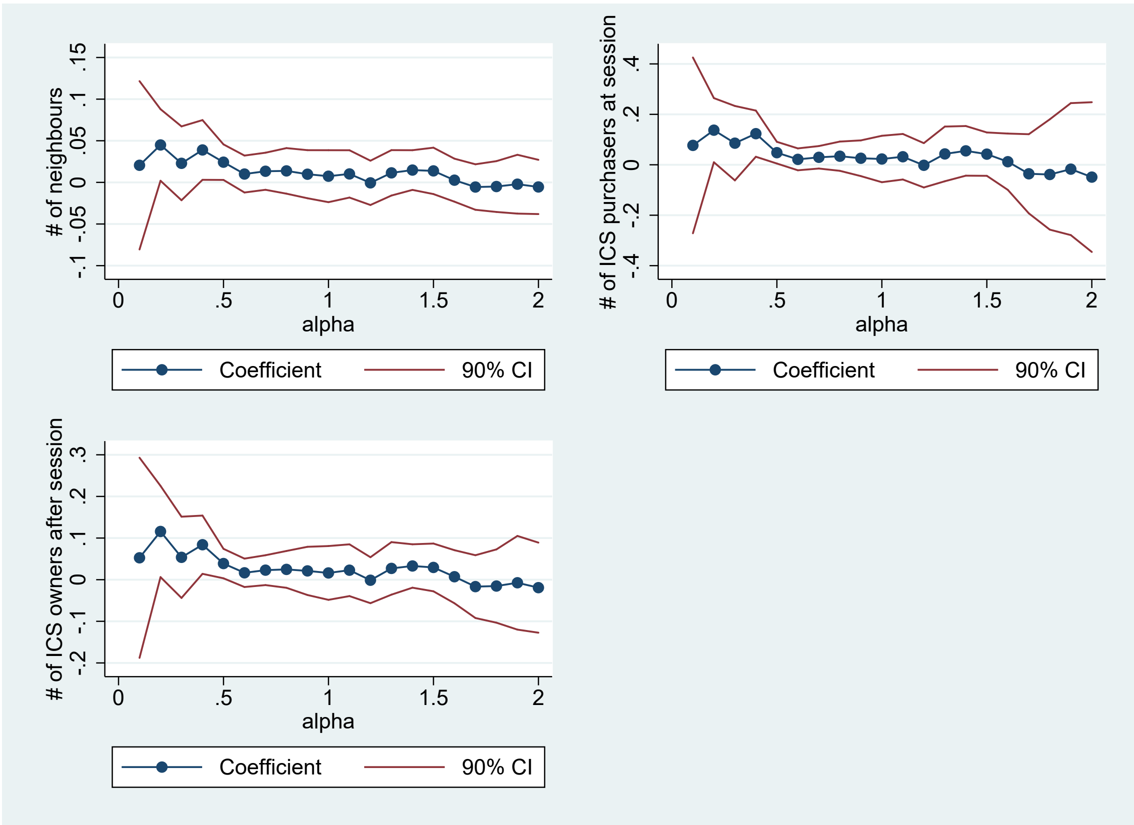
Note: Coefficients are derived from estimations as in Table 8. The sample is restricted to women who participated to the session and who did not buy on the spot.

Figure E.2: Social interaction effects on ICS ownership, sample of buying participants (Sat+Thurs), sensitivity analysis on α



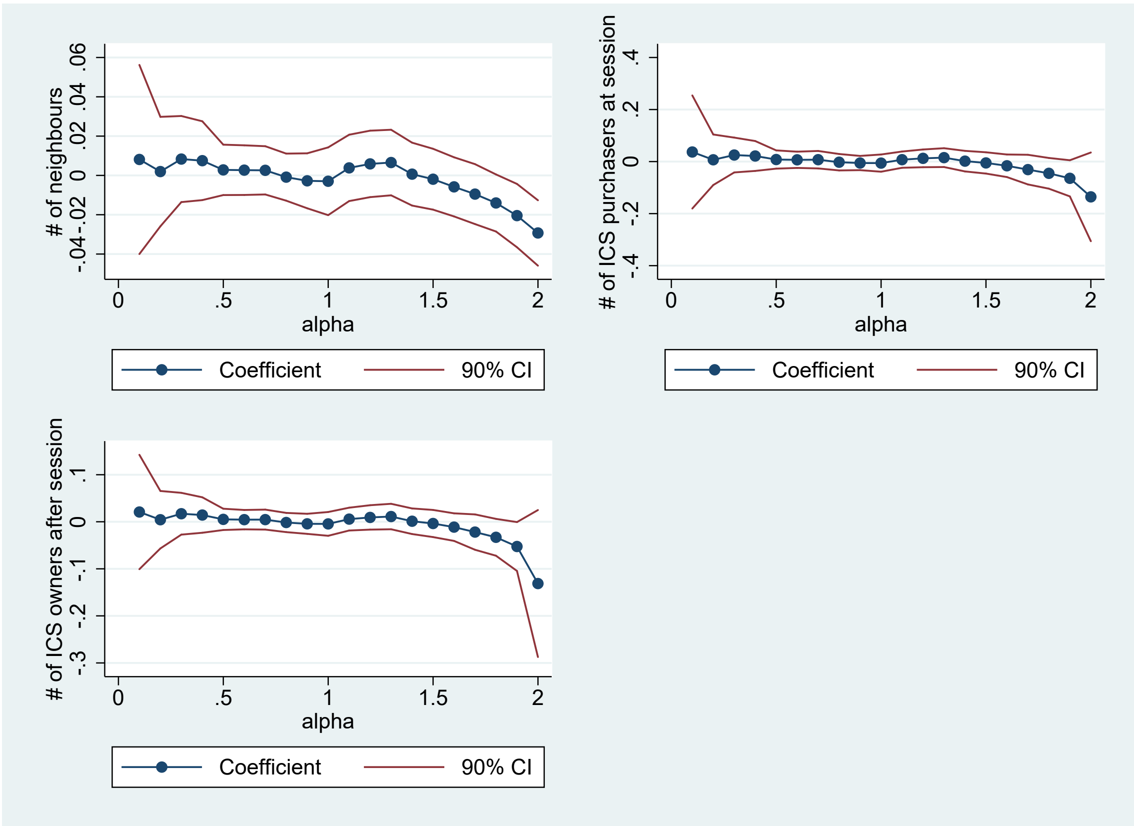
Note: Coefficients are derived from estimations as in Table 9.

Figure E.3: Social interaction effects on ICS ownership, sample of non-buying participants (Sat+Thurs), sensitivity analysis on α



Note: Coefficients are derived from estimations as in Table 10.

Figure E.4: Social interaction effects on ICS ownership at the endline, sample of non-participants, sensitivity analysis on α



Note: Coefficients are derived from estimations as in Table 11.

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