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One-Off Subsidies and Long-Run Adoption

Experimental Evidence on Improved Cooking Stoves in Senegal

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Abstract

Free technology distribution can be an effective development policy instrument if adoption is socially inefficient and hampered by affordability constraints. Yet, policy makers often oppose free distribution, arguing that reference dependence spoils the willingness to pay and thus market potentials in the long run. For improved cookstoves, this paper studies the willingness to pay six years after a randomized one-time free distribution. Using a real-purchase offer procedure, we find that households who received a free stove in the past do not reveal a lower willingness to pay to repurchase the stove. Furthermore, we provide exploratory evidence that learning and reference-dependence effects do not spill over from the treatment to the control group. The policy implication is that one-time free distribution does not disturb future market establishment and might even facilitate it.

Key Words: technology adoption, cookstoves, willingness to pay, real-purchase offer, energy access

JEL Codes: D03, D12, O12, O13, Q41

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1. Introduction

A growing body of literature shows that positive pricing of socially desirable technologies in poor settings leads to inefficiently low levels of adoption, in that considerable positive external effects are forgone. This is most notably due to a highly price-responsive demand. Cohen and Dupas (2010) and Tarozzi et al. (2014) observe very high price elasticities for insecticide-treated bednets, Kremer and Miguel (2007) for deworming drugs, Ashraf et al. (2010) for water disinfectants, and Mobarak et al. (2012) for improved biomass cookstoves. Based on this observation, Mobarak et al. (2012) make a case for subsidies or free distribution as obvious policies to overcome this type of under-adoption. Bensch and Peters (2015) in fact show that free distribution can be an effective instrument to trigger rapid short-term cookstove uptake among the poor.

In this paper, we test whether free technology distribution spoils the longer-term prospects of a self-sustaining market for this technology. Next to the fiscal burden of large-scale subsidy programs, a major argument against free distribution is that consumers may anchor their future willingness to pay (WTP) to prices previously paid for the product, a behavioural pattern also known as reference dependence (Köszegi and Rabin 2006). For experience goods, however, learning effects from a free trial might even increase future WTP (Levine et al. 2017). In her seminal paper, Dupas (2014) tests these two competing forces for the case of insecticide-treated bednets and finds important

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learning effects from own experimentation but no anchoring around previously subsidized prices. Dupas emphasizes, though, that this finding is very case-sensitive and transferability to other products and circumstances needs to be tested. The present paper builds on this research and extends Dupas' work to the case of improved biomass cookstoves (ICS). More specifically, we study the effect of free distribution on WTP for ICS in the long run.

The adoption of ICS is desirable from a public policy perspective because of their positive external effects on deforestation and climate change. Currently, more than three billion people worldwide are using firewood or charcoal for their daily cooking purposes, mostly in inefficient traditional stoves or open fires (World Bank 2017). Uptake of ICS is low because people are chronically short on cash and credit constrained (see Bensch et al. 2015; Beltramo et al. 2015). In addition, some private benefits of ICS such as time savings and health effects remain disregarded by households due to high discount rates or intra-household bargaining patterns (see Pattanayak and Pfaff 2009; Martin et al. 2011; Miller and Mobarak 2013).

The data used in this study was collected in 2015 among 371 households from 18 villages in rural Senegal. The identification strategy mainly relies on the exogenous variation stemming from a randomized controlled trial in 2009 for which we randomly allocated ICS at zero price among 253 households from that sample. The ICS has a lifetime of two to four years. Hence, when we conducted the follow-up in 2015, treatment group households had had the opportunity to test the ICS over a full lifecycle trial period. In this follow-up survey, we revisited both treatment and control households in order to offer the same type of ICS, now at positive prices. In addition to this experimental sample, we visited 118 households in six additional villages that had not been exposed to our RCT in 2009 or to any ICS promotion activity. We refer to this sub-sample as the non-experimental comparison group, which we then use in a supplementary analysis to explore the existence of spillovers within our experimental sample.

To estimate the WTP in all three groups, we use the Becker-DeGroot-Marschak (BDM) mechanism, an incentive-compatible real-purchase offer procedure (Becker et al. 1964; Plott and Zeiler 2005). This experimental design allows us to estimate the effects of one-off subsidies and a subsequent free lifecycle trial on ICS demand in the long run. Our study area resembles most rural areas in Africa to the extent that firewood is mostly collected, not purchased. Firewood scarcity is high, which is comparable to similarly arid countries in the region. In terms of ICS availability outside our experiment, ICS are not available in the villages, but can be obtained in towns located around 5 to 20 kilometres away. The type of ICS under analysis is adapted to local cooking habits and has been disseminated in other African countries as well, mostly going by the name Jambaar or Jiko (see, for example, Jetter et al. 2012).

Our paper complements Dupas' (2014) work in three ways. First, we assess the replicability of her findings for another base technology in a different setting. Second, unlike Dupas, we test the effect of a free full lifecycle trial period on demand, since the ICS is offered after the first product has exceeded its expected durability. This is an extension to Dupas, because she essentially assesses adoption of an additional second bednet, given that her follow-on study was carried out one year after the subsidized distribution of bednets with a lifetime of several years. Third, the BDM mechanism allows for individual bids per customer and thereby yields more precise, higher-resolution data on households' WTP as compared to take-it-or-leave-it approaches, where one price is offered to clusters of customers, which provides only WTP bounds.

Our main finding is that even high one-off subsidies do not decrease the WTP in the long run. The treatment group reveals a WTP that is 13 to 26 percent higher than in the control group. While this positive effect is not significantly different from zero using a conventional two-sided test, by applying a one-sided test we can rule out negative effects on the WTP at the six percent level. Although we cannot disentangle the learning effect from the anchor effect, our results confirm Dupas (2014) to the degree that any reference dependence is at least compensated by a positive learning effect. Explorative analysis of a third group outside the experimental villages suggests that no sizable spillovers from the treatment to the control group are at work.

In addition, we observe an average WTP of around 11 US$ and find that more than two-thirds of households make bids that exceed the 8.5 US$ that is usually charged for this ICS in nearby urban markets. This comes as a surprise given that it has so far proven to be extremely difficult for market-based ICS programs in Senegal to reach rural areas. Also in a global context, penetration rates for cookstoves have been found to be low even in areas in which people pay monetary prices for fuels (Bensch et al. 2015; Lewis and Pattanayak 2012; Putti et al. 2015). As we discuss in the concluding section, this finding suggests that barriers and frictions for vendors and thus risk premiums in such rural markets are high. These are costs that have to be covered by the end-user price to make the business attractive and hence the "in town" market price might simply not be high enough.

Taken together, our observation that the free distribution in the past does not harm today's marketability of improved stoves has important policy implications. The international community via the *Sustainable Energy for All* (SE4All) initiative of the United Nations envisages universal access to ICS or clean fuels by 2030, as also reflected in Sustainable Development Goal 7. The prevailing paradigm to achieve this goal is a market-based approach, implying that households are expected to pay cost-covering prices. On this note, our results suggest that one-off free distribution might not be in opposition to such a market-based approach and, in contrast, could even be a stepping stone toward a self-sustaining ICS market in the future.

The remainder of this paper is organized as follows. Section 2 reviews the literature and policy background. Section 3 outlines the research design, including the identification strategy and data collection. Section 4 presents the results, and Section 5 concludes.

2. Background

2.1. Literature and Policy Background on Improved Cookstoves

In recent years, political support for the dissemination of improved cookstoves (ICS) has grown considerably. The term 'improved' describes a wide range of replacements for traditional cooking methods, with a correspondingly large variation in performance. Major differences are related to costs and the degree to which the stove burns cleaner (see, for example, Jetter et al. 2012). The *World Health Organisation* (WHO) and the *Global Alliance for Clean Cookstoves* endorse smoke-free ICS, mainly electricity and gas, to combat adverse health effects of biomass cooking. The United Nations initiative SE4All pursues a broader approach in its endeavour to achieve universal access to modern cooking energy. They also count simpler biomass ICS as modern even if they are not 'clean' according to WHO standards, as long as they achieve high enough fuel savings relative to traditional stoves (World Bank 2017). The ICS used in the present study qualifies as 'modern' in the SE4All nomenclature but not as 'clean' in WHO's reading (see next section for more details on the so-called Jambaar ICS).

The role of subsidies in increasing ICS adoption is a matter of an ongoing debate (Simon et al. 2014). Most agencies and national governments reject subsidization of cookstoves, primarily based on concerns about financial sustainability and reference-dependent behaviour of recipients. The latter assumes that subsidization spoils the long-term WTP and thus the establishment of a self-sustaining ICS market. Others count on carbon finance, including the United Nations REDD+ scheme to fund ICS subsidies (see, for example, Beyene et al. 2015a) and some governments use pro-poor arguments to justify free distribution.

In the academic literature, evidence on the effectiveness of cookstove dissemination and adoption challenges is growing. Martin et al. (2011) summarize the state of research on improved cooking and emphasize the urgency of addressing the issue of biomass cooking from an environmental and health policy perspective. In general, livelihood and environmental improvements from improved cooking technologies materialize via two channels.

First, reduced woodfuel consumption directly reduces workload or monetary expenses, depending on whether fuels are purchased or collected. Environmental benefits stem from mitigated forest degradation and deforestation (Bailis et al. 2015). Ahrends et al. (2010) emphasize the role of woodfuels in tropical deforestation, as next in importance to agricultural land clearance. Deforestation is not only problematic for the local environment and economy (see, e.g., Myers et al. 2013), but also contributes an estimated 6 to 17 percent of global anthropogenic carbon dioxide emissions (van der Werf et al. 2009).

The second channel relates to reductions in smoke emissions and smoke exposure. ICS with improved combustion processes or chimneys to channel the smoke outside can achieve health-improving[1] reductions in household air pollution (Grieshop et al. 2011; Jetter et al. 2012). The non-linear particulate exposure-response relationship found in medical research suggests that large reductions in smoke exposure are required to ensure positive health effects (see, for example, Burnett et al. 2014; Jamison et al. 2013; or Pope et al. 2011). However, as can be seen in Yu (2011) and Bensch and Peters (2015), even simple ICS may bring about health benefits by facilitating outside cooking (if portable) and reducing cooking duration, which both can lead to a considerable reduction of smoke exposure. Beyond its relevance for health, the soot contained in the smoke of cooking fires is the largest source of anthropogenic black carbon, a climate-forcing emission (Gustafsson and Ramanathan 2016; Lacey et al. 2017; Ramanathan and Carmichael 2008;

[1] Exposure to particulate matter induced by biomass cooking affects health in various ways and may lead to acute respiratory infections, stunted growth in children, pneumonia, chronic bronchitis in women, chronic obstructive pulmonary disease (COPD), cataracts and other visual impairments, cardiovascular diseases, lung cancer, tuberculosis and perinatal diseases (see, for example, Po et al. 2011; Ezzati and Kammen 2002; Amegah et al. 2014; Dherani et al. 2008; McCracken et al. 2012; Hosgood et al. 2010; Bruce et al. 2013; and Smith et al. 2014).

Shindell et al. 2012). There is a growing consensus that black carbon is the second-most important source of direct radiative forcing after CO_2 (Gustafsson and Ramanathan 2016; IPCC 2013).

A couple of studies provide evidence for substantial woodfuel reductions as a result of ICS adoption. This branch of literature thus serves as a proof of the concept for the first impact channel (see Adrianzén 2013; Bensch and Peters 2013, 2015; Bensch et al. 2015; Beyene et al. 2015a; Brooks et al. 2016; and Rosa et al. 2014). These studies find that livelihood can be improved and deforestation reduced. In all these studies, the positive findings hinge upon the technical design of the ICS; consumers will not use the stove unless it is in fact an improvement over their current stove.[2] Moreover, ICS have to be properly adopted and – if necessary – maintained by users because the partial, diminishing or improper use of ICS may entail few or no benefits, as observed in Hanna et al. (2016) and Usmani et al. (2017).

2.2. Improved Cookstoves in Senegal

Efforts to reduce the country's heavy reliance on traditional biomass fuels for domestic usage date back to the 1970s, when Liquefied Petroleum Gas promotion programs were launched (Schlag and Zuzarte 2008). Later initiatives also worked on the development of low-cost improved biomass stove models, including a program by the Government of Senegal – supported by Deutsche Gesellschaft für Internationale Zusammenarbeit (GIZ) – that successfully disseminates a charcoal version of the Jambaar ICS through its program Foyers Ameliorés aux Sénégal (FASEN).[3] Usage of both Liquefied Petroleum Gas and charcoal, however, is mainly limited to urban areas. In rural Senegal, where 57 percent of the Senegalese population lives, the primary cooking fuel of 86 percent of households is firewood, predominantly used in inefficient open-fire three-stone stoves or very simple metal stoves (AfDB 2016; ANSD 2014).

As an improved alternative for rural areas, FASEN also developed a firewood version of the Jambaar, which is under evaluation in the present paper and depicted in Annex B. It is a portable, maintenance-free single-pot stove with a fired clay combustion centre enclosed by a metal casing. Owing to these simple design improvements compared

[2] Not all stoves that are referred to as 'improved' really represent an improvement. For example, Burwen and Levine (2012) studied a simple mud stove touted as an ICS in Ghana; even in a controlled field lab setting, it did not perform better than the traditional counterparts.

[3] For more details on the Senegalese stove market development, see Bensch and Peters (2013).

to the traditional stoves, the woodfuel burns more efficiently and the heat is better conserved and directed toward the cooking pot. Bensch and Peters (2015) observe a savings rate of around 40 percent per stove utilisation under day-to-day conditions. This ICS can be considered as well-adapted to the local cooking conditions, which also explains the high usage intensity observed in the same study. Although it is not primarily designed to reduce smoke emissions, study participants exhibit fewer smoke-related disease symptoms, which may be due to increases in outdoor cooking and reduced cooking duration and thus less smoke exposure. The stove has a lifespan of around two to four years. Bensch and Peters (2015) also show that three and a half years after the randomisation in 2009, half the treatment households were still using the randomly distributed ICS, but only half of these ICS were in good condition as wear and tear became noticeable.

FASEN's approach is to train local manufacturers to produce and market the Jambaar stove. ICS are never produced locally in the villages but rather in Dakar; a few producers also exist in some secondary towns near the study area. Thus, to reach the rural areas, ICS have to be obtained in town and transported to the villages, by either individual customers or vendors. The ICS price in secondary towns is around 5,000 CFA Francs, for which the exchange rate to the US$ is about 590:1. This sum, equivalent to 8.5 US$, is about thrice the average daily wage for casual agricultural work in the study area. Prices in Dakar are considerably higher, at around 8,500 CFA F (13 US$). Various reasons explain the higher prices in Dakar compared to secondary towns. To start with, labour costs are higher. In addition, the Dakar producers concentrate on charcoal stoves and produce firewood Jambaars only on demand and mostly in smaller quantities, which leads to higher unit costs than in secondary towns. Moreover, the Dakar producers employ higher-quality inputs and more and better machinery than that used in secondary towns, again leading to higher costs but also higher quality. Later, in the results section, we emphasize that the ICS are generally not available directly in villages; as we will show, households did not obtain new ICS to replace the deteriorated stoves made available in 2009. This adds to the absence of a village price for the ICS. Traditional stoves, in contrast, can be acquired in the villages at low prices. Traditional metal stoves or open fire grills cost between 500 and 2,500 CFA F (0.85 to 4.3 US$) and three-stone stoves are usually homemade at zero cost (stove depictions can be seen in Annex B).

3. Experimental Design

The experiment underlying this study was conducted in the Peanut Basin region, located in central Senegal, around 200 kilometres southeast of the capital Dakar. The basin is Senegal's major agricultural region. Ninety-nine percent of households engage in farming (ANSD 2015) and nearly all land is under cultivation of subsistence and cash crops, mainly peanuts, millet, and cowpeas. In terms of access to basic infrastructure, including water, roads, schools and health facilities, the region ranks in the mid-range when comparing it to others in the country (ANSD 2009). Biomass production in this semi-arid zone is low and hence firewood is relatively scarce (Gill 2013).

The data used in this paper was collected in November and December 2015 in two types of villages: an experimental sample and a complementary non-experimental sample. We start by presenting the experimental sample that is used for the main analysis; it comprises twelve villages in which we conducted a randomized controlled trial (RCT) back in 2009, with previous follow-ups in 2010 and 2013 (see Bensch and Peters 2015). Randomization was done at the household level in 2009. Despite a lapse of six years since baseline, merely 17 of the original 253 randomly sampled households could not be re-interviewed in 2015.[4] Just as at baseline stage, the resulting sample of 236 households is composed of 40 percent of households in the experimental treatment arm, i.e., they received an ICS in 2009.

We adhered to a predefined experimental procedure to conduct the BDM real-purchase offer in order to obtain the WTP. In cooperation with a Senegalese survey partner, six local enumerators were trained to act as ICS sales agents.[5] The sample households were informed in advance about a visit of a stove seller, including a survey on energy use. The person responsible for taking financial decisions in the household was requested to be present during this visit. Both treatment and control households were visited individually. Once our team arrived in the household, enumerators started by presenting the Jambaar ICS. The in-town market price was not revealed. Main sales pitches were the same as those that business-as-usual vendors of the ICS program of the

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[4] Eight households changed location, two households merged into one, three households' respondents were deceased, four could not be located, and one household was not willing to participate in the interview. We tested for attrition following Fitzgerald et al. (1998), in a first step regressing attrition status on relevant household characteristics. For that purpose, we use the controls presented in Section 4 and extended them by additional controls used in the probit regressions performed in Bensch and Peters (2015) to validate the balancing achieved through the randomization. A slight degree of attrition seems perceivable, but none of the variables turns out to be significant, thus rendering any further attrition adjustment unnecessary.

[5] This rules out foreigner-presence effects as observed in Cillier et al. (2015).

Senegalese government are trained to use: quick cooking, safety, woodfuel savings, heat conservation, smoke reduction, cleanliness, and improvements in women's living conditions. We additionally announced that the ICS was produced in Dakar and is thus of supposedly better quality than the ICS produced in towns nearby. Moreover, households were explicitly allowed to make payments for their stove with the village chief within a timeframe of about two and a half months. This payment period was granted because we visited households in November, a time of year when households are particularly short on cash because the harvest period has just started. By the time of the payment, target households would have sold at least part of their harvest and thus would be able to make investments in durable goods.

Our field team then introduced the BDM purchase offer procedure to each interviewee: the bidder is asked to state his or her WTP for the ICS, knowing that the price is randomly drawn only after bidding.[6] Out of fairness considerations, we conducted the draw publicly and at the village level after all bids were made, so that one effective price applied to the entire village. The bidder could buy the product for the price drawn only if his or her bid equaled or exceeded the price drawn. If the bid fell below the drawn price, no transaction took place. In order to practice the procedure, the enumerators first conducted a hypothetical BDM game that involved a purchase offer of a solar lamp (see Annex C). All but two respondents then followed the invitation to make a bid.

During pre-tests, we noted that households were well able to grasp the bidding game and its rules and were hence able to confidently express their WTP. There is thus no indication that the BDM elicitation approach imposed unrealistic cognitive demands, a common problem with stated WTP approaches for environmental non-market products (Gregory et al. 1993).[7] The WTP elicited by BDM is widely seen as a very precise

[6] The random price determination makes the BDM mechanism a variant of the Vickrey (second-price) auction, where the final price is determined through competition between bidders (Vickrey 1961). Beltramo et al. (2015), for example, applied Vickrey auctions to study the effect of marketing messages and payment over time on the uptake of improved cookstoves in Uganda. A simple analysis of WTP for ICS in Bangladesh using the Vickrey auction is conducted by Rosenbaum et al. (2015). Alternatively, ICS adoption preferences have been studied based on discrete choice methods, by van der Kroon et al. (2014) and Jeuland et al. (2015), who used hypothetical decisions, and by Jagger and Jumbe (2016), who faced participants with a real choice between an ICS and a package of dry goods including sugar and salt of equal monetary value.

[7] Also note that villagers are not unfamiliar with paying for cookstoves; while the widely used three-stone stove is free of any monetary charge, 82 percent of sampled households have paid for a stove in the past.

approximation of a real-life WTP because of its incentive-compatible features (see Berry et al. 2015 for a discussion of the BDM method).[8]

After bidding for the stove, a structured questionnaire was administered using a tablet-based data collection application.[9] Later the same day, all survey participants came together to attend the public draw of the price. The draw balls contained prices between 4,500 and 6,000 CFA F (7.5 to 10 US$); this price range was not communicated to the participants. Successful bidders received the stove after signing contracts. Again for fairness reasons, we also informed households about the "in town" price of ICS of 5,000 CFA F and provided the contact details of vendors in town. Households were then allowed to withdraw from the contract. This happened in only five cases, implying very strong compliance among participants and thus reliability of the bid amounts. The same can be said about the subsequent actual payment behaviour; all but six households paid the full price via their village chief before the end of the payment period.[10]

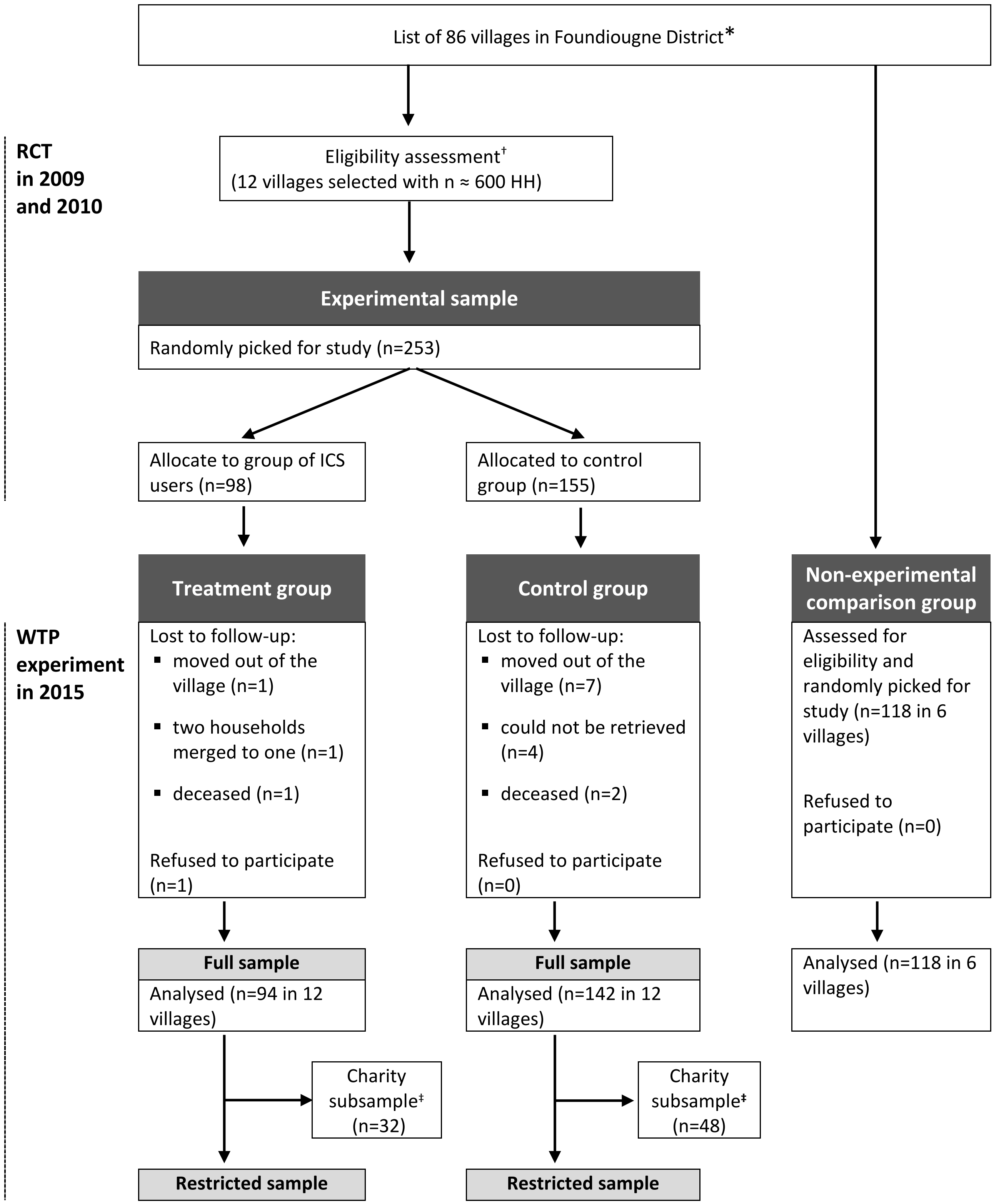
The survey in 2015 included six additional villages that had not been part of the 2009 RCT, where we applied exactly the same BDM and interview procedure with a random sample of 118 households. The villages were selected from the same department, at a location sufficiently remote from the twelve villages of the original sample. We refer to this group as the "non-experimental comparison group." It will provide complementary information on villages without any previous local exposure to ICS. The composition of the entire sample is depicted in the participant flow in Figure 1.

[8] The mechanism already has been widely used in laboratory settings, and also in field experiments to elicit consumer preferences for such diverse items as meat quality, rice origin, mosquito nets, water and hygiene, and rainfall insurance (Lusk et al. 2001; Morey 2016; Hoffmann 2009; Guiteras et al. 2016; Cole et al. 2014).

[9] The complete experimental procedure also included a purchase offer for another ICS type, the so-called Sakkanal, after the Jambaar stove purchase offer was finalized. The clearly demarcated sequence ensured that the second stove offer could not affect the bids for the Jambaar stove analysed in this article, since, when the Jambaar was offered, households were not aware that they would be presented with a second stove.

[10] As in a business-as-usual marketing approach, our team returned to the villages in order to take back the ICS from those households that did not pay the full price. In case households made (non-predefined) advance payments for the ICS, these were returned to these households as stipulated in the contract.

Figure 1. Participant Flow



Analysed (n=62 in 9 villages)

Analysed (n=94 in 9 villages)

Note: * Foundiougne is a district of 3,000 km^2 size in the south of the Peanut Basin region. All villages on the list were originally envisaged for an electrification intervention, which, however, was mostly abandoned, such that to date none of the surveyed villages was electrified.

† Eligibility criteria included the ecological zone, population size, main livelihood activities, infrastructure availability, and absence of access to ICS.

‡ Charity subsample refers to three sampled villages that were targeted by a recent intervention, which offered ICS at highly subsidized prices; see Section 4.1.

4. Results

4.1. Basic Descriptive Statistics

As a result of a low attrition rate between the randomization in 2009 and the 2015 survey, we retrieved 236 households in our experimental sample: 94 in the treatment group and 142 in the control group (see Figure 1). The sample composition and balancing is depicted in Table 1. We show household characteristics for four sets of controls: respondent-specific characteristics on the individual level as well as socio-demographic, economic, and cooking-related household variables. Additional household characteristics from these domains, which are not used as controls in the subsequent analysis, are also presented in Table 1.

Table 1. Descriptive Statistics and Randomization Test

	2015 data					2009 data
	Treatment		Control		Difference	Difference
	mean	sd	mean	Sd	*p*-value	*p*-value
Respondent Controls						
Age difference respondent to interviewer	16.86	(16.38)	16.11	(16.81)	0.73	-
Person taking financial decisions in HH present during stove purchase experiment (share)	0.67		0.63		0.56	-
Person responsible for cooking in HH present during stove purchase experiment (share)	0.51		0.57		0.33	-
Sociodemographic Controls						
Head of HH is female (share)	0.14		0.14		0.96	0.68
Head of HH attended koranic or Arabic school (share)	0.83		0.85		0.65	0.73
HH size	13.91	(8.59)	14.57	(10.28)	0.61	0.86
Economic Controls						
HH has cement flooring (share)	0.65		0.61		0.50	0.73
HH owns sheep (share)	0.47		0.47		0.96	0.88
HH's monthly telecommunication expenditures (CFAF)	16,400	(21,450)	14,340	(17,130)	0.43	0.19
Cooking Controls						
HH owns firewood ICS (share)	0.21		0.20		0.77	0.87
- firewood ICS given out in 2009	0.10		0.00		0.00	-
- firewood ICS from charity	0.06		0.16		0.03	-
HH mostly uses open fire for cooking (share)	0.62		0.56		0.36	0.69
HH buys firewood (share)	0.50		0.52		0.75	0.93
Additional key household characteristics						
HH's monthly total expenditures (CFAF)	149,500	(112,200)	147,300	(101,900)	0.89	0.48
HH possesses tile, zinc or cement roofing (share)	0.68		0.66		0.76	0.96
Motorized vehicle ownership (share)	0.27		0.31		0.47	0.42
Firewood collection time (hours per week)	9.2	(11.6)	10.7	(12.7)	0.37	0.08
Any HH member ever cooked on an ICS (share)	0.99		0.47		0.00	-
Number of observations	94		142			

Note: Expenditures are outlier-corrected by trimming figures that deviate more than three standard deviations (sd) from the mean to the value equalling the mean plus or minus three standard deviations. *p*-values refer to *t*-tests on the bivariate difference between treatment and control observations. In the farthest right column, this test is also conducted with the 2009 baseline data on the respective variables.

For all variables, *t*-tests confirm similarity between treatment and control observations both for the 2009 baseline and the 2015 follow-up wave. We see that, in line with our planning to conduct the follow-up with the person responsible for financial decisions in the household, two-thirds of respondents are actually the members taking financial decisions in the household. This is highly correlated with the sex of respondents, who are male in 63 percent of the cases (not shown in the table). Similarly, the age difference between the respondent and the enumerator, which reflects the specific interview situation, is highly correlated with the respondent age, which averages 47 years. Households are typically large in rural Senegal, which is also reflected in an average household size of 14.4. Table 1 also shows telecommunication expenditures as a proxy for income and two wealth proxies: roofing and sheep ownership.

About half the households sometimes buy their firewood and thus have a monetary incentive to invest in a fuel-saving stove, unlike those households that only collect wood. For this latter group, the return on an ICS investment is of a non-monetary nature. Finally, while a considerable share of households have at least one member who has ever tried some type of improved stove,[11] a fifth of the households already possess an ICS, fairly equally distributed across the treatment and control groups. As shown in Table 1, this can be explained, in part, by the ICS received in 2009 among the treatment group, which were mostly worn out but still in use. On the other hand – as we learned during the survey – small-scale initiatives recently sold ICS, out of charity, at highly subsidized prices in three of our twelve villages ("charity subsample" in the following). These were taken up more by control households, since virtually none of them had had an ICS at their disposal at the time of the charities' visits. In the remaining nine villages ("restricted sample" in the following), only one percent of households owned an ICS that had not been distributed in our 2009 randomization. In other words, in spite of the exposure to the new ICS technology induced by our previous study, virtually no household had made an effort to re-invest into ICS by obtaining one from the towns nearby or from vendors in Dakar. ICS were not used in the area at the time of our 2015 survey, except for households in the charity subsample and the few households still equipped with the stove provided in 2009. Later in our analysis, village fixed effects and a dummy on firewood ICS ownership will control for the particularity of this subsample. In addition, we test for the sensitivity of results to excluding the charity subsample completely. The same descriptive statistics and balancing tests as in Table 1 can be seen in Table A1 in Annex A for this restricted sample. The sample is similarly well balanced as the full sample, with the intended exception that firewood ICS owning households can now only be found in the treatment group.

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[11] This question was asked in an open way and did not explicitly refer to the Jambar ICS used in our experiment. Thus, some households may have referred to the Sakkanal, the Cire wood, or a fixed mud stove that is slightly improved compared to an open fire.

The same descriptive statistics and balancing tests as in Table 1 have also been compiled for the non-experimental comparison group, in that case compared to the entire experimental sample. They can be seen in Table A2 in the Annex. The non-random allocation into the two groups likely explains the few observed statistically significant differences. For example, the comparison group households have better roofing on average, whereas the experimental group exhibits higher shares of livestock ownership. Total expenditures are higher in the experimental group; per-capita expenditures are not. There is, thus, no clear indication of structural differences in the overall socio-economic conditions that would cause us to abstain from comparing the two groups in the supplementary non-experimental analysis in Section 4.3.

4.2. Impacts of Free Lifecycle Trial Period

Results on the impacts of the free lifecycle trial treatment period are presented in Table 2. We show specifications that use the raw WTP as the outcome. Table 2 additionally differentiates between results for the full sample as compared to the restricted sample, where we exclude the charity subsample with recent ICS interventions. Moreover, we show specifications with different sets of control variables. The reason is that we have two data waves at hand that can potentially serve as a baseline (2009 and 2015). *A priori*, neither of the two appears to be superior. The 2015 data more closely reflects the situation at the time of the WTP survey and is thus less noisy, while some of the variables may have been affected by the 2009 treatment, notably the cooking variables. The choice between the two waves is hence between precision and potentially endogenous controls.

Table 2. Willingness to Pay Impact Estimates

outcome:	Willingness to Pay (in CFA F)							
estimation method:	OLS							
village sample:	full sample				restricted sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Free lifecycle trial treatment	837.15 (723.87) [0.27]	793.49 (658.26) [0.25]	860.21 (638.48) [0.20]	826.50 (684.14) [0.25]	1239.76 (1089.19) [0.29]	1391.02 (904.89) [0.16]	1309.74 (1020.00) [0.24]	1675.07 (968.30) [0.12]
Marginal mean for comparison group‡	5994.00	6011.34	5970.91	5998.23	6591.39	6531.48	6508.59	6418.97
WTP increase	+14.0%	+13.2%	+14.4%	+13.8%	+18.8%	+21.3%	+20.1%	+26.1%
Maximal WTP drop according to 95% CI lower bound	-12.6%	-10.9%	-9.1%	-11.3%	-19.3%	-10.7%	-16.0%	-8.7%

Controls:								
Village	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Respondent	-	Yes	Yes	Yes	-	Yes	Yes	Yes
Sociodemographic	-	Yes	Yes (2009)	Yes	-	Yes	Yes (2009)	Yes
Economic	-	Yes	Yes (2009)	Yes	-	Yes	Yes (2009)	Yes
Cooking	-	-	Yes (2009)	Yes	-	-	Yes (2009)	Yes
Observations	234	234	230	234	154	154	152	154
Adjusted R-squared	0.04	0.07	0.06	0.06	0.00	0.07	0.06	0.07

Note: ‡ Calculated at the mean of the control variables. The four sets of controls are those introduced in Table 1. The restricted sample refers to those nine of the twelve villages where no recent ICS interventions took place. Due to very low rates of households having female heads or owning firewood ICS at baseline, the respective coefficients were not estimable in the equations using baseline covariates. We therefore replaced the two variables by household whose heads have more than one spouse and households owning a woodfuel stove (not open fire). Standard errors are in parentheses and *p*-values in squared brackets. Standard errors are clustered by village; * p<0.10, ** p<0.05, *** p<0.01.

We find that the free lifecycle trial increases household's WTP. The effect size is considerable, at 13 to 26 percent. Yet, even when we look at the restricted sample only and increase precision by adding the extended set of controls (Column 8), the effect is only borderline statistically significant, at the 12 percent level. The effect is reduced by half in size and is non-significant for the full sample that includes the charity subsample (14 percent, Column 4). Similarly, the effect size is smaller when not accounting for ICS ownership among other cooking covariates (Column 6). This is intuitive because the utility of a second ICS is obviously smaller. Further significant correlates of WTP are financial responsibility status of the respondent and gender of the head of household. The explanatory power of these controls does not seem to be strong, though (see Table A3 for coefficients of the full set of control variables). As expected, using 2009 instead of 2015 covariates leads to less precise estimates in the restricted sample (Column 7).

Table 3. Willingness to Pay Impact Estimates

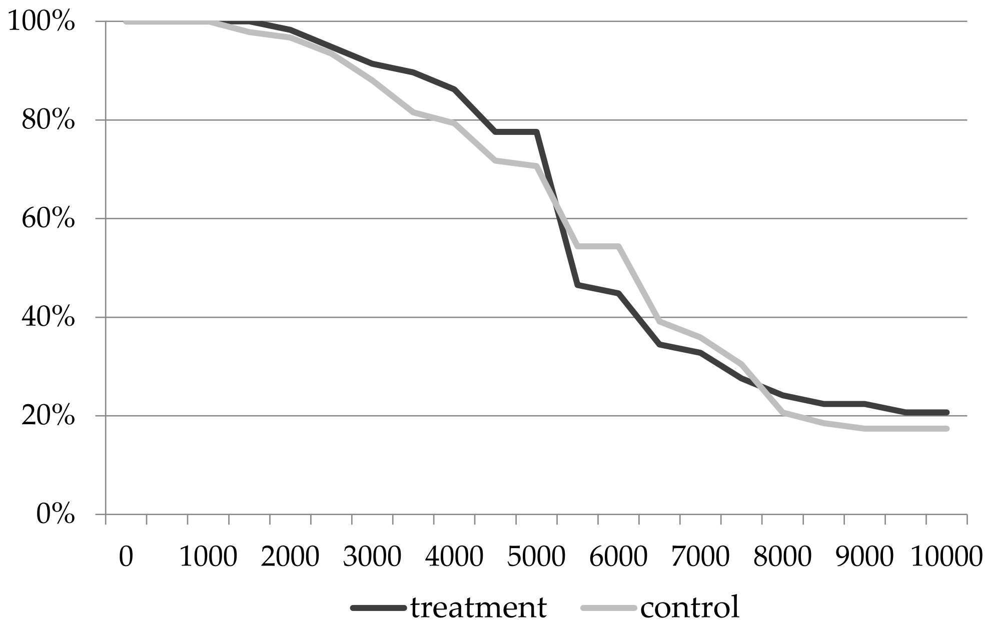
outcome:	Willingness to Pay (in CFA F)							
estimation method:	OLS							
village sample:	full sample				restricted sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Free lifecycle trial treatment	837.15 (723.87) [0.27]	793.49 (658.26) [0.25]	860.21 (638.48) [0.20]	826.50 (684.14) [0.25]	1239.76 (1089.19) [0.29]	1391.02 (904.89) [0.16]	1309.74 (1020.00) [0.24]	1675.07 (968.30) [0.12]
Marginal mean for comparison group‡	5994.00	6011.34	5970.91	5998.23	6591.39	6531.48	6508.59	6418.97
WTP increase	+14.0%	+13.2%	+14.4%	+13.8%	+18.8%	+21.3%	+20.1%	+26.1%
Maximal WTP drop according to 95% CI lower bound	-12.6%	-10.9%	-9.1%	-11.3%	-19.3%	-10.7%	-16.0%	-8.7%
Controls:								
Village	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Respondent	-	Yes	Yes	Yes	-	Yes	Yes	Yes
Sociodemographic	-	Yes	Yes (2009)	Yes	-	Yes	Yes (2009)	Yes
Economic	-	Yes	Yes (2009)	Yes	-	Yes	Yes (2009)	Yes
Cooking	-	-	Yes (2009)	Yes	-	-	Yes (2009)	Yes
Observations	234	234	230	234	154	154	152	154
Adjusted R-squared	0.04	0.07	0.06	0.06	0.00	0.07	0.06	0.07

Note: ‡ Calculated at the mean of the control variables. The four sets of controls are those introduced in Table 1. The restricted sample refers to those nine of the twelve villages where no recent ICS interventions took place. Due to very low rates of households having female heads or owning firewood ICS at baseline, the respective coefficients were not estimable in the equations using baseline covariates. We therefore replaced the two variables by household whose heads have more than one spouse and households owning a woodfuel stove (not open fire). Standard errors are in parentheses and *p*-values in squared brackets. Standard errors are clustered by village; * p<0.10, ** p<0.05, *** p<0.01.

Overall, while the precision of estimates is not high enough to establish a clearly positive treatment effect, the estimations consistently indicate that it is safe to reject a negative effect. A one-sided test on a negative impact, equivalent to half the p-value shown in Table 2, is significant at the 6 percent level in our preferred estimation in Column (8). Even at the lower bounds of the 95 percent confidence intervals presented in the table, the WTP does not go down by more than around 10 percent in the main specifications. We conclude from this that negative effects of reference dependence (if they exist) do not exceed positive learning effects.

We test the sensitivity of our results by running the same regressions as in Table 2 after excluding 15 households with ambiguous bidding behaviour. These include the households already referred to in Section 3 who (i) opted out of the experiment before submitting a bit (in the analysis above, their WTP was set at zero), (ii) did not sign the contract or (iii) did not pay after a successful bid. In addition, we exclude (iv) those with an unsuccessful bid who increased their WTP after the price draw (n=4). In our main analysis above, we considered households (ii) to (iv) as normal bidders. Given their small number, it is in line with expectations that results of this sensitivity test barely differ from the above findings. If at all, they slightly increase in size, such as, for example, the full and restricted sample estimates with controls (equivalent to Columns 4 and 8 in Table 2) from 830 to 950 CFA F and from 1,680 to 1,780 CFA F, respectively (not shown in the table).

Figure 2. ICS Demand Curve According to Bids in BDM Purchase Offer



Note: This figure refers to the restricted sample excluding the charity subsample.

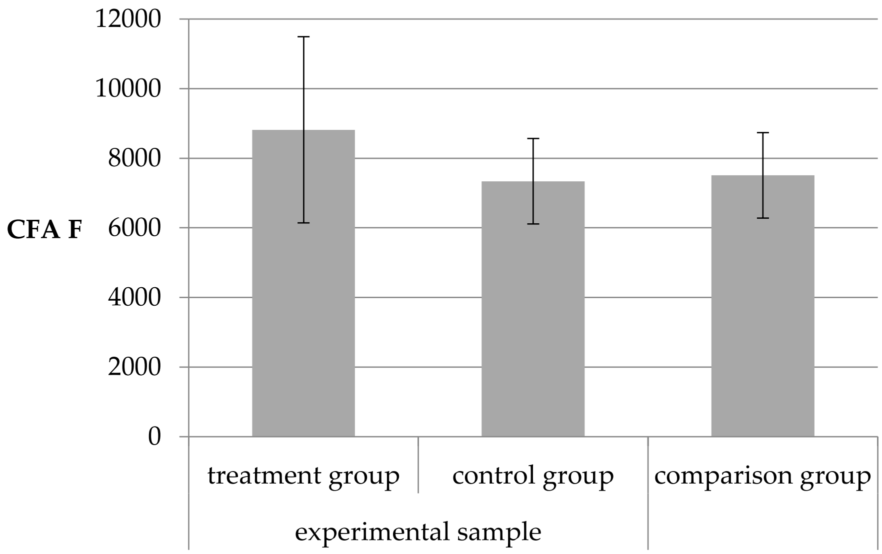
Beyond the treatment effect, a very notable result is the level of the willingness to pay (see also the ICS demand curve depicted in Figure 2). The simple means are 6,300 and 7,000 CFA F for the full and restricted sample, respectively, and thus clearly above the 5,000 CFA F price charged by ICS producers in nearby towns. In the full and restricted samples, 69 and 75 percent of households, respectively, make a bid that is higher than this “in town” price. Even if we take the higher Dakar price of 8,500 CFA F, still-remarkable shares of 16 and 19 percent make high enough bids. This is a surprising result given that commercial ICS programs that charge cost-covering prices both in Senegal and elsewhere in Africa are having tremendous problems with low adoption rates. We will therefore discuss the viability of a rural market or reasons for its absence in the concluding section.

4.3. Spillover Effects

Because ICS were randomly distributed within villages in the 2009 RCT, spillover effects from the treatment to the control group are possible. Control households may have learnt about ICS' benefits from treated neighbours in their village, but likewise they might anchor their WTP to the free ICS distribution to treatment households in 2009. This would be especially problematic if the absence of a clear treatment effect observed in the previous section was driven by reference dependence in the treatment group and indirect learning effects among control group households. In this case, our conclusion of no negative effects of free distribution would be false.

We test this in an exploration of aggregate spillovers by including the non-experimental comparison group in the estimation sample. Remember that these are six additional villages that had never been exposed to ICS before our visit (see Section 3). As can be seen in Figure 3, the WTP in these comparison villages barely differs from the WTP in the experimental control group. The data underlying this figure comes from estimating the same specification as in Table 2 (Column 8), now including a polytomous categorical treatment variable accounting for the three different groups: experimental treatment group, experimental control group, and the newly added non-experimental comparison group. The regression results presented in Table A4 in Annex A underpin the similarity between the control and non-experimental comparison groups, as indicated by a *p*-value of 0.77. The WTP of the treatment group is higher compared to the non-experimental comparison group, yet the estimate is less precise than the one comparing the treatment and control group (point estimate of 1,309 CFA F, *p*-value of 0.31).

Figure 3. Willingness to Pay in Experimental Groups and Comparison Group



Note: The values in this figure are derived from a regression model with the same specification as in Table 2 that now includes a polytomous categorical treatment variable with the comparison group as the base case. First, the marginal WTP mean for the comparison group (on the right) is calculated at the mean of the control variables. Second, the WTP means for the two experimental subsamples on the left are calculated by adjusting the comparison group mean by the point estimates of the respective coefficients in the same regression model. The lines indicate the 95% confidence intervals for these two coefficients. Third, the confidence band for the comparison group is derived from the same regression, using the control group as the base case. Applying wild bootstrap inference (Cameron et al. 2008) yields only marginally different confidence intervals than the ones depicted in the graph, which are based on conventional clustered standard errors.

Interpreting the comparison group's WTP as the counterfactual WTP in the complete absence of a previous free-distribution RCT, the similarity between the comparison and the control group suggests that there are no strong aggregate spillovers from the treatment to the control group. In addition, this observation makes it unlikely that there are survey effects, which might have been induced by reciprocity, for example.

5. Discussion and Conclusion

This paper studied whether give-away distribution of improved cookstoves (ICS) affects willingness to pay (WTP) six years after the ICS were randomized among a sample of households. It is thus the first to examine this effect after a full lifecycle trial period, in which households were given ample time to learn about the stoves, enabling them to make a well-informed repurchase decision. We find that treatment households reveal a 13 to 26 percent higher WTP in the repurchase offer than control households who had not used an ICS, though this effect is not significant across all specifications (*p*-

value of 0.12 in the main regression). Potential reference dependence effects are hence at least compensated by learning effects. By comparing our experimental sample to a comparison group that had never before been exposed to ICS, we furthermore provide indications that, if there were learning spillovers from the treatment to the control group, they would be compensated by reference dependence effects in the control group. The product's valuation among non-beneficiaries of a free distribution intervention may thus not only be affected through learning spillovers, as suggested by Dupas (2014), but also through reference dependence spillovers.

Dupas explicitly discusses the transferability of her findings to improved cookstoves. She expects that people "may underestimate the returns to switching" and thus hypothesizes that "one-time subsidies for cookstoves […] have the potential to boost subsequent adoption through learning effects." Overall, we confirm Dupas' prediction to the degree that free distribution does increase adoption in the long run. The fact that the vast majority of households did not acquire an ICS themselves between the follow-ups in 2010 and 2015 calls attention to the need to guarantee easy access to the technology. The policy implication of this finding is striking: free cookstove distribution emerges as a policy option that is not only effective in triggering high uptake in the short run (Rosa et al. 2014; Bensch and Peters 2015; Beyene et al. 2015b) but also in the long run. We thereby complement the branch of literature on the validity of one-time subsidies and cost-sharing related to important products for the poor (see also Bates et al. 2012).

It is also the absolute level of WTP revealed by households in our study that is remarkable. With an average of around 11 US$, clearly exceeding "in town" market prices, the WTP is very high compared to previous cookstove WTP studies (Beltramo et al. 2015; Mobarak et al. 2012). With a repayment rate of almost 100 percent, participants took the offer and their bids seriously.[12] In Bensch and Peters (2016), we examine different reasons for this high WTP across the sample in more detail. We argue that the high wood scarcity in the region plays an important role, as well as specific features of our BDM approach, notably its implicit door-to-door marketing feature. Because of the individual household visits and the lottery situation, customers probably dedicate more attention to the offer than they would in the case of regular shop offers. This is particularly true for products that attract less attention in everyday life, as is the case for cookstoves. The typically male financial decision maker in the household tends to neglect

[12] This cannot be taken for granted. See, for example, Grimm et al. (2017) and Tarozzi et al. (2014), who use payment targets similar to ours and observe repayment rates of between 60 and 70 percent.

them, and also faces many competing pressures. Not least, the two-month payment target in the harvest period may have increased adoption rates. Among other things, it helped households to commit themselves to buy the cookstove. This commitment device character has also been observed by Duflo et al. (2011) for time-limited fertilizer discounts in Kenya. To the contrary, Hawthorne effects are quite unlikely, since the randomization was done six years ago. Still, these factors should be taken into account when interpreting the WTP levels and their transferability to other settings.

The absence of a vibrant local ICS market despite the relative high WTP points at a variety of barriers and frictions that make rural market exploration a highly risky endeavour. Vendors in such a market environment would have to price in risk premiums, leading to rural end-user prices that exceed “in town” prices. Having said this, strong external effects of ICS, as well as the poverty alleviation effects on the private level, provide economic arguments to subsidize cookstoves, even on a permanent basis. To the extent that climate-relevant emissions are reduced through a reduction of deforestation or black carbon emissions, carbon finance could be an additional funding source. This would also considerably increase the political feasibility of long-term subsidy schemes.

Beyond concerns about funding sources and reference dependence, what are further main arguments against subsidies and free distribution? It is sometimes argued that cost sharing helps target users with higher marginal benefits while avoiding over-treatment (Cohen et al. 2015). This so-called screening effect, however, is clearly competing with credit and liquidity constraints that hamper adoption, in particular if poorer households are targeted (Tarozzi et al. 2014). A related concern about free distribution is that positive prices not only induce screening effects, but also sunk cost effects. After having paid a positive price for a product, people might feel committed to using it (see Arkes and Blumer 1985). For various products, however, this concern about underutilization has been rebutted (see, for example, Ashraf et al. 2010, Cohen and Dupas 2010, and Grimm et al. 2017). For the particular case of cookstoves, Bensch and Peters (2015) and Rosa et al. (2014) observe very high usage rates in free cookstove distribution programs. Beyene et al. (2015b) show in an RCT in Ethiopia that households who received a stove for free use it even more than those that paid positive prices.

The paradigm that today’s subsidies induce detrimental effects on tomorrow’s markets has for long suffocated the subsidization discussion at early stages. Evidence is growing that these categorical concerns are not justified. Indeed, if our findings on high adoption intensities and long-run appreciation are confirmed in future research, subsidization (including free distribution) of technologies generating positive external effects can be a cost-effective tool to tackle many grievances in developing countries. The calibration of this subsidy policy may rely on a step-wise approach, where in a first step the market potentials and purchasing power in new intervention areas are examined using a methodology similar to what this study has done. In a second step, the region-specific evidence would be used to inform the roll-out at scale in the region, which may involve sustainable subsidy schemes. One crucial aspect is to communicate clearly that a one-time free distribution today is no entitlement for a subsidy tomorrow. Shaped in such a way, subsidies might even facilitate self-sustaining markets by mobilizing long-term demand through learning effects.

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Annex A

Table A1. Balancing Test for the Restricted Sample

	2015 data					*2009 data*
	Treatment		Control		Difference	Difference
	mean	sd	Mean	sd	*p*-value	*p*-value
Respondent Controls						
Age difference respondent to interviewer	16.41	(14.68)	14.33	(16.93)	0.43	-
Person taking financial decisions in HH present during stove purchase experiment (share)	0.70		0.69		0.80	-
Person responsible for cooking in HH present during stove purchase experiment (share)	0.49		0.54		0.54	-
Sociodemographic Controls						
Head of HH is female (share)	0.13		0.12		0.82	1.00
Head of HH attended koranic or Arabic school (share)	0.84		0.82		0.75	0.28
HH size	14.40	(9.06)	13.87	(9.48)	0.73	0.10
Economic Controls						
HH has cement flooring (share)	0.63		0.57		0.50	0.94
HH owns sheep (share)	0.50		0.50		1.00	0.96
HHs monthly telecommunication expenditures (CFAF)	15,540	(23,240)	12,620	(13,830)	0.34	0.73
Cooking Controls						
HH owns firewood ICS (share)	0.10		0.01		0.01	0.86
HH mostly uses open fire for cooking (share)	0.61		0.56		0.54	0.49
HH buys firewood (share)	0.60		0.59		0.89	0.63
Additional key household characteristics						
HH's monthly total expenditures (CFAF)	150,000	(116,500)	137,300	(91,800)	0.48	0.78
HH possesses tile, zinc or cement roofing (share)	0.65		0.60		0.54	0.43
Motorized vehicle ownership (share)	0.29		0.22		0.35	0.89
Firewood collection time (hours per week)	12.0	(16.4)	11.5	(12.0)	0.51	0.58
Any HH member ever cooked on an ICS (share)	0.98		0.36		0.00	-
Number of observations	61		94			

Note: Expenditures are outlier-corrected by trimming figures that deviate more than three standard deviations (sd) from the mean to the value equalling the mean plus or minus three standard deviations. *p*-values refer to *t*-tests on the bivariate difference between treatment and control observations. In the farthest right column, this test is also conducted with the 2009 baseline data.

Table A2. Balancing Test for the Experimental and Non-Experimental Sample

	2015 data				
	Experimental (restr. sample)		Non-experimental		Difference
	mean	sd	mean	sd	*p*-value
Respondent Controls					
Age difference respondent to interviewer	15.14	(16.06)	15.42	(13.38)	0.89
Person taking financial decisions in HH present during stove purchase experiment (share)	0.69		0.81		0.03
Person responsible for cooking in HH present during stove purchase experiment (share)	0.52		0.47		0.35
Sociodemographic Controls					
Head of HH is female (share)	0.12		0.14		0.72
Head of HH attended koranic or Arabic school (share)	0.83		0.74		0.07
HH size	14.08	(9.29)	11.89	(7.34)	0.04
Economic Controls					
HH has cement flooring (share)	0.60		0.61		0.86
HH owns sheep (share)	0.50		0.38		0.04
HHs monthly telecommunication expenditures (CFAF)	13,480	(16,370)	10,420	(10,260)	0.08
Cooking Controls					
HH owns firewood ICS (share)	0.04		0.02		0.20
HH mostly uses open fire for cooking (share)	0.58		0.50		0.15
HH buys firewood (share)	0.59		0.51		0.21
Additional key household characteristics					
HH's monthly total expenditures (CFAF)	142,000	(100,500)	120,000	(84,700)	0.08
HH possesses tile, zinc or cement roofing (share)	0.62		0.77		0.01
Motorized vehicle ownership (share)	0.25		0.27		0.66
Firewood collection time (hours per week)	11.1	(13.9)	11.4	(17.2)	0.85
Any HH member ever cooked on an ICS (share)	0.60		0.19		0.00
Number of observations	156		117		

Note: Expenditures are outlier-corrected by trimming figures that deviate more than three standard deviations (sd) from the mean to the value equalling the mean plus or minus three standard deviations. *p*-values refer to *t*-tests on the bivariate difference between treatment and control observations.

Table A3. Willingness to Pay Impact Estimates (Full Regression Results)

outcome:	Willingness to Pay (in CFA F)							
estimation method:	OLS							
village sample:	full sample				restricted sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Free lifecycle trial treatment	837.15	793.49	860.21	826.50	1239.76	1391.02	1309.74	1675.07
	(723.87)	(658.26)	(638.48)	(684.14)	(1089.19)	(904.89)	(1020.00)	(968.30)
Age difference respondent to interviewer		-55.39*	-51.53*	-55.17*		-97.22**	-75.74**	-97.54**
		(26.16)	(23.04)	(25.51)		(32.72)	(31.52)	(34.02)
Person taking financial decisions in HH present during bidding		1769.04**	1502.47*	1816.34**		2779.18***	2881.30*	3041.92***
		(626.69)	(793.84)	(653.10)		(683.97)	(1445.02)	(577.77)
Person responsible for cooking in HH present during bidding		-216.91	-342.59	-103.57		-207.60	-411.59	139.93
		(1084.08)	(1370.81)	(1070.99)		(1562.17)	(2289.19)	(1452.04)
Head of HH is female		-878.13	-	-851.6		-2283.57***	-	-2212.99***
		(675.19)	-	(645.78)		(502.67)	-	(501.18)
Head of HH has more than one wife		-	1313.45	-		-	1793.54	-
		-	(1536.44)	-		-	(2380.37)	-
Head of HH attended koranic or Arabic school		686.07	-563.39	689.52		1254.00*	-1965.67	1167.02*
		(661.37)	(943.84)	(663.66)		(600.89)	(1408.64)	(534.25)
HH size, ln		252.60	331.37	305.20		37.52	212.42	174.71
		(672.82)	(745.13)	(737.69)		(909.24)	(1205.72)	(943.94)
HH has cement flooring		462.50	-625.13	466.65		1004.04	-1252.49	1120.92
		(635.18)	(710.97)	(572.37)		(797.45)	(1013.53)	(624.78)
HH owns sheep		-1434.75*	-660.19	-1426.08*		-1903.22*	-719.97	-1819.09
		(697.04)	(497.08)	(787.85)		(938.66)	(621.94)	(1017.33)
HH's monthly telecomm. expenditures, ln		179.17	17.74	176.86		297.24	43.92	277.68
		(158.57)	(152.32)	(163.23)		(283.16)	(211.41)	(298.93)
HH owns firewood ICS			-	-716.60			-	-3635.91**
			-	(955.82)			-	(1378.49)
HH owns woodfuel stove other than open fire			772.53	-			1683.93	-
			(788.27)	-			(1140.74)	-
HH mostly uses open fire for cooking			143.93	-317.37			608.15	-125.57
			(804.41)	(676.57)			(624.97)	(895.71)
HH buys firewood			1330.62	-118.81			1256.76	-3.74
			(1472.55)	(922.21)			(2240.30)	(1388.15)
Controls:								
Village	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Respondent	-	Yes	Yes	Yes	-	Yes	Yes	Yes
Sociodemographic	-	Yes	Yes (2009)	Yes	-	Yes	Yes (2009)	Yes
Economic	-	Yes	Yes (2009)	Yes	-	Yes	Yes (2009)	Yes

Cooking	-	-	Yes (2009)	Yes	-	-	Yes (2009)	Yes
Observations	234	234	230	234	154	154	152	154
Adjusted R-squared	0.04	0.07	0.06	0.06	0.00	0.07	0.06	0.07

Note: HH = households. The restricted sample refers to those nine of the twelve villages where no recent ICS interventions took place. Standard errors are clustered by village and presented in parentheses; * p<0.10, ** p<0.05, *** p<0.01.

Table A4. Willingness to Pay Impact Estimates

outcome:	Willingness to pay (in CFAF)		
estimation method:	OLS		
village sample:	restricted sample + comparison group		
	Coeff.	95% Conf. Interval	
	(1)	(2)	(3)
Treatment	1309.31	-1365.58	3984.19
	(1247.16)		
	[0.31]		
Control	-168.11	-1396.10	1059.88
	(572.54)		
	[0.77]		
Observations	272		
Adjusted R-squared	0.05		
Controls:			
Village	Yes		
Respondent	Yes		
Sociodemographic	Yes		
Economic	Yes		
Cooking	Yes		
Marginal mean for comparison group	7508.50	-1059.88	1396.10

Note: Results from a regression model with the same specification as in Table 2 that now includes a polytomous categorical treatment variable with the comparison group as the base case.

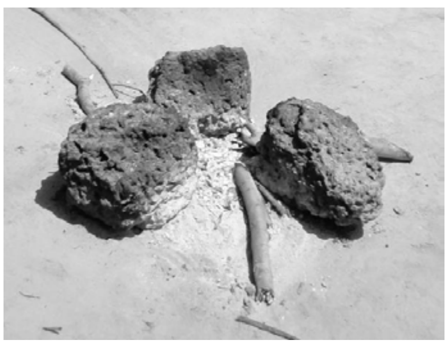
Annex B

Stove Types Used in the Survey Area

Stove type/ model name	Combustion chamber type	Fuel type	Feed type	Chimney	Portability	Approx. cost (US$)
Three-stone stoves	None	biomass	continuous	No	yes	-
Os	None	biomass	continuous	No	yes	1-2
Cire khatach	metal	crop residues	batch fed	No	yes	3-5
Cire wood	metal	wood	continuous	No	yes	3-5
Malagasy stove	metal	charcoal, (wood)	continuous	No	yes	3-5
Jambaar Wood	ceramic	wood	continuous	No	yes	10

Open fire stoves

Three-stone stoves



Os

Traditional metal stoves

Cire khatach (crop residues)

Cire wood

Malagasy stove

Improved Cooking Stove (ICS) Jambaar



Sources: Author’s own photographs.

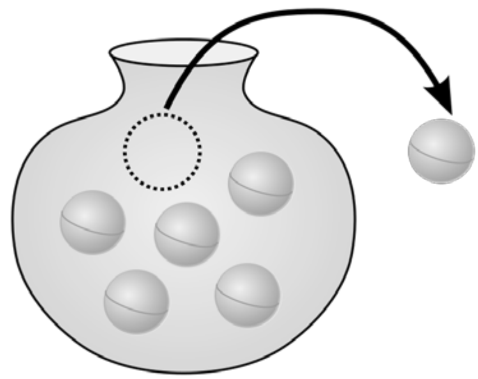
Annex C

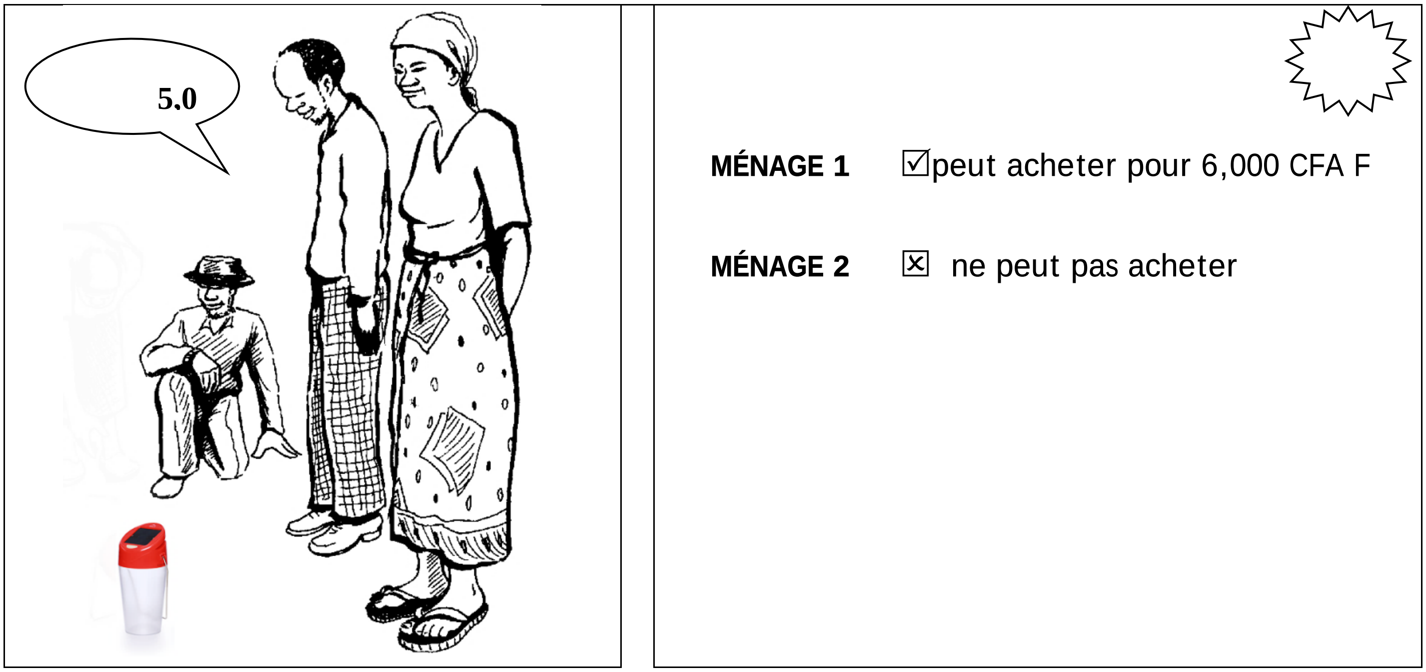
Showcard Used to Practice the BDM Procedure

LE MATIN	***L'APRÈS-MIDI***
VISITES AUX MÉNAGES	**TIRAGE AU NIVEAU DU VILLAGE**

MÉNAGE 1

MÉNAGE 2





Note: The showcard explains the four steps in our BDM procedure for the exemplary case of a solar lamp. On the left (step 1), two households bid for the solar lamp in the morning. On the right, the subsequent village lottery in the afternoon is shown (steps 2 and 3) and the lottery results are compared with the households' bids (step 4): household 1 can buy the lamp, household 2 cannot.

Sources: Developmentart.com; courtesy of d.light; derivative of Quartl, CC BY-SA 3.0.