One-off subsidies and long-run adoption – Experimental evidence on improved cooking stoves in Senegal

Bensch, Gunther and Peters, Jörg

RWI - Leibniz Institute for Economic Research, University of Passau

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BY GUNTHER BENSCH AND JÖRG PETERS*

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Free technology distribution can be an effective development policy instrument if market-driven adoption is socially inefficient and hampered by affordability constraints. Yet, policy makers often oppose free distribution, arguing that reference dependence lowers the willingness to pay (WTP) and thus market potentials in the long run. For improved cookstoves, this paper studies the WTP six years after a randomized one-time free distribution in 2009. We demonstrate that the cookstoves were intensely used by the treatment group households in the years after randomization until they reached their designated lifetime. Using a real-purchase offer, we find that both treatment and control households reveal a remarkably high WTP in 2015. The estimated confidence interval suggests that we can exclude a substantial negative effect on the treatment group. The policy implication is that one-time free distribution does not necessarily undermine future market establishment and thus can be an effective policy instrument if rapid dissemination is the objective.

Keywords: energy access, real-purchase offer, reference dependence, supply chains, technology adoption, willingness to pay

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

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 externalities 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, Berry and Shapiro (2010) for water disinfectants, and both Mobarak et al. (2012) and Pattanayak et al. (2018) for improved cooking stoves (ICS). Based on this observation, Mobarak et al. (2012) make a case for subsidies or free distribution as policies to overcome this type of under-adoption. Bensch and Peters (2015) and Pattanayak et al. (2018) in fact show that free distribution can be an effective instrument to trigger rapid short-term ICS uptake among the poor.

In this article, we study the effect of a one-time free ICS distribution on the revealed willingness to pay (WTP) for the same ICS technology six years later, a period that exceeds the free stove's lifetime. We thereby test whether a free distribution spoils 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 WTP to prices previously paid for the product, a behavioral pattern also known as reference dependence (Köszegi and Rabin 2006). For experience goods, learning effects from a free trial may also influence future WTP, which is increasing in case of positive learning and decreasing if learning is negative (Levine et al. 2018; Fischer et al. 2014).

In her seminal paper, Dupas (2014) tests these two forces of learning and reference dependence for the case of insecticide-treated bednets and finds important learning effects from own experimentation, but no anchoring around previously subsidized prices. Fischer et al. (2014) study these factors for the free distribution of consumable drugs. They observe a statistically insignificant decrease in adoption of a drug with positive learning potentials, whereas adoption significantly decreases for products

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with no or negative learning potentials. While we cannot disentangle reference dependence and learning effects as done by Dupas and Fischer et al., we extend their work on the net long-run effects of a free distribution policy for the case of ICS.

The adoption of ICS is desirable from a public policy perspective as reflected in *Sustainable Development Goal 7* on universal access to affordable and clean energy by 2030. 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). Improved cooking is high on the policy agenda because of positive external effects on deforestation and climate change (Martin et al. 2011; Shindell et al. 2012; Bailis et al. 2015). It is partly due to those non-internalized benefits that uptake of ICS is mostly low (Lewis and Pattanayak 2012; Putti et al. 2015). Other reasons stretch from cash and credit constraints to high discount rates or intra-household bargaining patterns (see e.g. Bensch, Grimm and Peters 2015, Levine et al. 2018, and Alem, Hassen and Köhlin 2018).

The data used in our study was collected in 2015 by surveying 371 households from 18 villages in rural Senegal. The identification strategy mainly relies on the exogenous variation stemming from a randomized controlled trial (RCT) 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 offered both treatment and control households the same type of ICS, now at positive prices, in order to elicit their WTP. On top of 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 use this non-experimental comparison group to explore the existence of spillovers within our experimental sample, for example through social network effects and learning from neighbors (see Maertens and Barrett 2012; Krishnan and Patnam 2013).

To estimate the WTP in all three groups, we use the Becker-DeGroot-Marschak (BDM) mechanism, an incentive-compatible real-purchase offer procedure (Becker, DeGroot and Marschak 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 the dominant cooking fuel and 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 some towns located around five to 20 kilometers away, something that many villagers, however, are not aware of. The type of ICS under analysis is adapted to local cooking patterns 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 and Bensch and Peters 2015).

Our paper mainly complements Dupas' (2014) work in two ways. First, we assess the replicability of her findings for another base technology in a different setting. Second, our study pushes further in that we test the effect of a free full lifecycle trial period on demand, as the new ICS is offered after the first ICS has exceeded its expected durability. This is an extension to Dupas, because a household needs several bednets and she assesses adoption of a second product, given that her follow-on study was carried out one year after the subsidized bednet distribution with a lifetime of several years. We furthermore complement Fischer et al. (2014) by providing evidence on a durable product. Compared to both products, learning effects and reference dependence can be expected to differ for our case of an ICS.

Our main finding is that even a free distribution does not lead to a sizable decline in the WTP in the long run. The treatment effect point estimates are positive, yet only borderline statistically significant for certain specifications using a conventional two-sided test. To assess the concern that free distribution spoils future WTP we additionally apply a one-sided test and can rule out negative effects at the eight to eleven percent significance level. The 95 percent confidence interval indicates that the

worst-case decrease in the WTP at the interval's lower bound is between eight and 15 percent.

Our results confirm Dupas (2014) to the extent that any reference dependence is at least compensated by a positive learning effect. Explorative analysis of a comparison group outside the experimental villages suggests that no sizable spillovers from the treatment to the control group are at work. Our results also confirm the finding of Fischer et al. (2014) inasmuch as free trials do not decrease future demand for a product that entails positive learning.

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 9 US$ that are usually charged for this ICS on markets in towns nearby. This is noteworthy given that it has so far proven to be extremely difficult for market-based ICS programs in Senegal (as elsewhere in the developing world) to reach rural areas. 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. To make the business attractive, these costs have to be covered by the end-user price and hence the in-town market price might simply not be high enough for rural areas (see as well Adams et al. 2016). Our results provide a proof of concept that if rapid adoption is a policy goal, subsidization of free distribution does not hamper the establishment of future market-based distribution.

The remainder of this article is organized as follows. The following section reviews the literature and policy background. The section on *Experimental Design* outlines the research approach, including the identification strategy and data collection. The fourth section presents the results with a supplementary analysis of potential spillovers and the last section concludes and discusses the implication of our findings for ICS support policies.

Background

This section summarizes the literature on ICS and the international policy debate as well as the Senegal specific background on improved cooking policies.

Literature and Policy Background on Improved Cookstoves

In recent years, political support for the dissemination of 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 Organization* (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 endeavor to achieve universal access to modern cooking energy. They also count simple biomass ICS that are not 'clean' according to WHO standards as 'modern', 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 details on the Jambaar ICS).

The role of subsidies in increasing ICS adoption is a matter of an ongoing policy debate (Simon et al. 2014). Most agencies and national governments reject subsidization of cookstoves, primarily based on concerns about financial sustainability and the assumption 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). The livelihood and environmental improvements from improved cooking technologies generally

materialize via two channels. First, reduced woodfuel consumption directly reduces workload or monetary expenses, depending on whether fuels are purchased or collected. It also brings about environmental benefits that stem from mitigated forest degradation and deforestation and, in turn, climate change mitigation (Bailis et al. 2015; Ahrends et al. 2010; Myers et al. 2013; 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, Marshall, and Kandlikar 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 Langbein, Peters and Vance (2017), Yu (2011) and Bensch and Peters (2015), even simple ICS may bring about health benefits from reduced smoke exposure by facilitating outside cooking and shorter cooking durations. Beyond its relevance for health, the soot of cooking fires is the largest source of anthropogenic black carbon, an important climate-forcing emission (Lacey et al. 2017; Ramanathan and Carmichael 2008; Shindell et al. 2012).

A number of studies provide evidence for substantial woodfuel reductions as a result of ICS adoption (see Adrianzén 2013; Bensch and Peters 2013, 2015; Bensch, Grimm and Peters 2015; Brooks et al. 2016; Gebreegziabher et al. 2018; and Rosa et al. 2014). In all these studies, the positive findings hinge upon the effective and perceptible improvements of the ICS compared to the baseline stove. More precisely, consumers

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[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, Quansah and Jaakkola 2014; Dherani et al. 2008; McCracken et al. 2012; Hosgood et al. 2010; Bruce et al. 2013; and Smith et al. 2014).

will only use the new stove if it fits into the cooking habits and if it yields reductions in fuel consumption or smoke exposure at manageable adaptation costs. Indeed, 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, Duflo and Greenstone (2016) and Usmani, Steele and Jeuland (2017).

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 (LPG) 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).[2] Usage of both LPG 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).[3]

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 the Appendix. It is a portable, maintenance-free single-pot stove with a fired clay liner inseparably bound to a metal cladding with a mix of cement and ashes. Owing to simple design improvements compared 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 utilization under day-to-day conditions. This ICS can be considered as well-adapted

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

[3] See Wiedinmyer et al. (2017) for urban-rural differences in Ghana.

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. Bensch and Peters (2015) also show that three and a half years after the randomization in 2009, half the treatment households were still using the randomly distributed ICS. Half of these ICS were in good condition as wear and tear became noticeable, which is in line with expectation, since the stove has a lifespan of around two to four years according to the FASEN program.

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 9 US$, is about three times the average daily wage for casual agricultural work in the study area. Prices in Dakar are considerably higher, at around 8,500 CFA F (13 US$). Various reasons explain the higher prices in Dakar compared to secondary towns. To start with, labor 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 and not readily available in nearby towns; as we will show, households did not obtain new ICS to replace the deteriorated stoves provided in 2009. Hence, there is also no 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 the Appendix).

Experimental Design

The experiment underlying this study was conducted in the Peanut Basin region, located in central Senegal, around 200 kilometers southeast of the capital Dakar. The basin is Senegal's major agricultural region. Ninety-nine percent of households engage in farming and nearly all land is under cultivation of subsistence and cash crops, mainly peanuts, millet, maize and cowpeas (ANSD 2015). 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 scarce (Gill 2013).

The data used in this article 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, where treatment was stratified by village. 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

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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, Gottschalk and Moffitt (1998), in a first step regressing attrition status on relevant household characteristics. For that purpose, we use the control variables presented in the *Results* section and extended them by additional control variables 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.

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 making 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. Stove prices were not revealed. Main sales pitches were the same as those that business-as-usual vendors of the ICS program of the 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 only 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.[6]

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[5] This rules out foreigner-presence effects as observed in Cillier, Dube and Siddiqi (2015).

[6] The delayed payment period potentially offers an opportunity to game the system. Given that participants did not need to pay right away, they could have made a higher bid than their own ad-hoc valuation in order to find out more information about the usefulness of the ICS or potentials to resell and default in case the ICS turns out to be useless. One would expect this strategic bidding to be higher in the control group, since the treatment group is already fully informed. Yet, there is no indication for this; default rates are extremely low in both the treatment and the control group.

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.[7] Out of fairness considerations, we conducted the draw publicly and at the village level after all bids had been 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 A). 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, Lichtenstein and Slovic 1993).[8] The WTP elicited by BDM is widely seen as a precise approximation of a real-life WTP because of its incentive-compatible features (see Berry, Fischer and Guiteras 2019 for a discussion of the BDM method).[9]

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[7] 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, Derby and Dutta (2015). Alternatively, ICS adoption preferences have been studied based on discrete choice methods, by van der Kroon, Brouwer and van Beukering (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.

[8] 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.

[9] 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, solar kits, rice origin, mosquito nets, water and hygiene, and rainfall insurance (Alem and Dugoua 2018; Lusk et al. 2001; Morey 2016; Hoffmann 2009; Grimm et al. 2018; Guiteras et al. 2016; Cole, Stein and Tobacman 2014).

Figure 1: Participant Flow
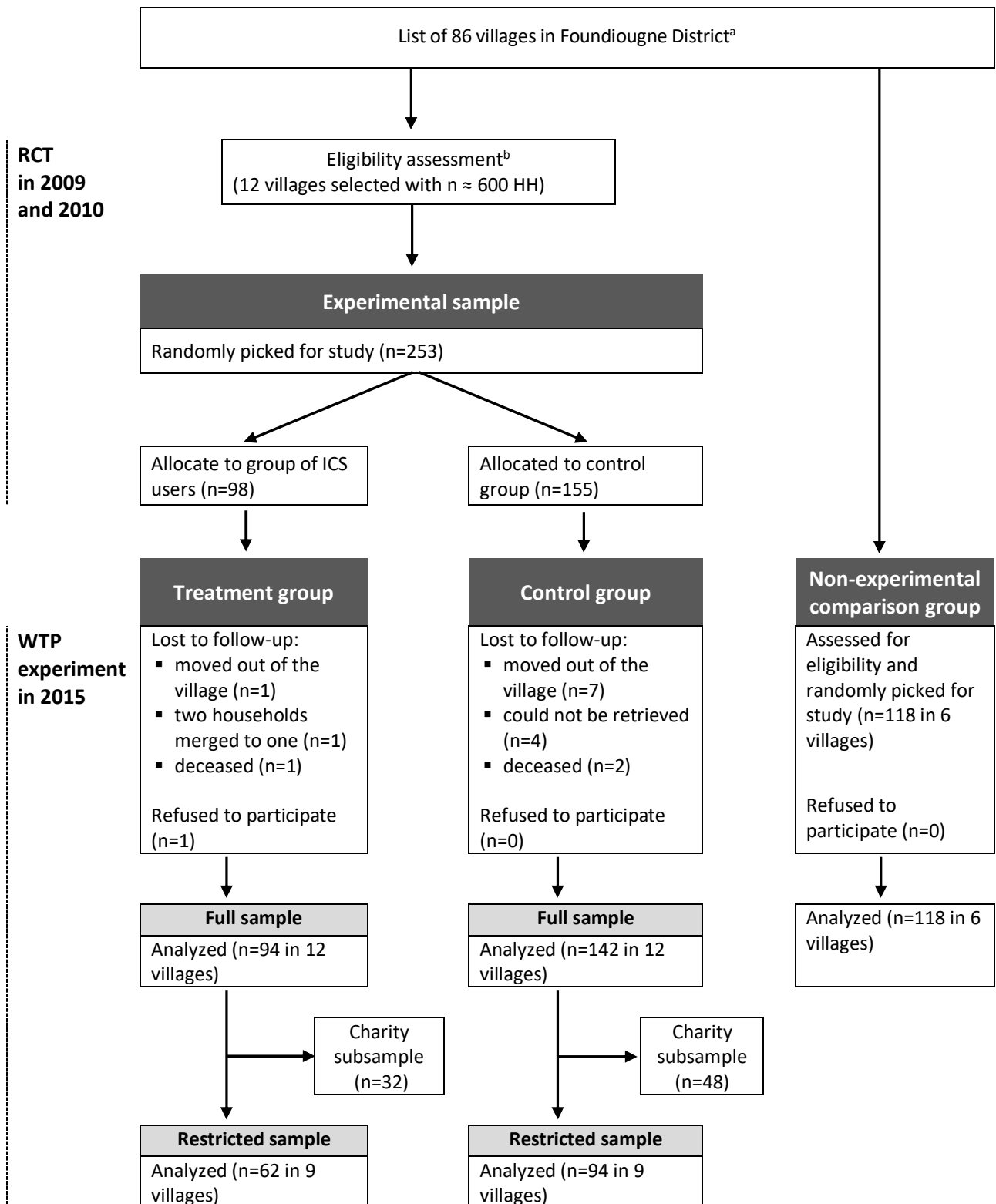
Note: [a] Foundiougne is a district of 3,000 km2 size in the south of the Peanut Basin region. All villages on the list were originally

envisaged for an electrification intervention, which, however, was mostly abandoned, such that to date none of the surveyed

villages was electrified. [b] Eligibility criteria included the ecological zone, population size, main livelihood activities, infrastructure

availability, and absence of access to ICS.

After bidding for the stove, a structured questionnaire was administered using a tablet-based data collection application. [10] 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 and can thus not have affected the bidding of households.[11] For reasons of fairness and transparency, we then informed households that the ICS is available at around 5,000 CFA F in towns nearby (the "in-town price" in the following) and provided the contact details of vendors. Successful bidders could withdraw from the commitment to buy the ICS or otherwise received the stove after signing contracts. Withdrawals 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 behavior; all but six households paid the full price via their village chief before the end of the payment period.[12]

The survey in 2015 included six additional villages that had not been part of the 2009 RCT, where we applied 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 participant flow in Figure 1 presents the composition of the entire sample and

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[10] The experimental procedure also included the same BDM procedure 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 analyzed in this article, since, when the Jambaar was offered, households were not aware that they would be offered a second stove.

[11] Later in the Results section we show the entire distribution of bids. There is no indication for an effect of the price range on the bidding behavior of households such as a kink in the distribution below 4,500 or above 6,000 CFA F. This is furthermore confirmed by the fact that 30 percent of participants made bids below 4,500 CFA F.

[12] 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.

depicts another division of the sample at the bottom of the figure into the restricted sample and the charity subsample. 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 (80 percent of buyers of the Jambaar in these villages paid less than 200 CFA F or 0.3 US$). These were taken up more by control households, simply because many treatment households still had the ICS they received in 2009 at the time of the charities' visits. This can have various implications for the WTP, for example because these households bid for a second ICS, but also because the charity ICS itself might have induced reference or learning effects. Therefore, later in the analysis we explore how excluding this "charity subsample" affects the results in the remaining nine villages ("restricted sample" in the following).

Results

We first present the descriptive statistics on the balancing of the three groups (treatment, control and comparison) and the ICS usage prior to our real-purchase offer in 2015, followed by results on the WTP and the effects of the lifecycle trial period.

Basic Descriptive Statistics and Balancing

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 Table 1: Descriptive statistics on control variables). In Table 1: Descriptive statistics on control variables, we show descriptive statistics for those variables that we include as controls in the impact analysis in the next subsection. These variables either refer to the characteristics of the household member who participated in the WTP bidding game or were elicited before the randomization in 2009. In line with expectation, treatment and control group are balanced in 2009.

Table 1: Descriptive statistics on control variables

	Treatment mean (sd)	Control mean (sd)	Difference *p*-value
Respondent-specific characteristics at time of stove purchase experiment (elicited in 2015)			
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 baseline variables (elicited in 2009)			
Male head of household with more than one wife (share)	0.36	0.32	0.48
Head of HH attended koranic or Arabic school (share)	0.78	0.76	0.78
HH size	14.90 (13.58)	15.25 (15.79)	0.86
Economic baseline variables (elicited in 2009)			
HH has cement flooring (share)	0.32	0.30	0.70
HH owns sheep (share)	0.64	0.65	0.88
HH's monthly telecommunication expenditures (CFAF)	4,780 (5,160)	5,880 (8,160)	0.25
Improved firewood stove baseline variable (elicited in 2009)			
HH owns woodfuel stove other than open fire	0.36	0.43	0.30
Number of observations	94	142	

Note: See Bensch and Peters (2015) for a comprehensive set of balancing tests. Expenditures are outlier-corrected by trimming figures that deviate more than three standard deviations (sd) from the mean to the value equaling the mean plus or minus three standard deviations. *p*-values refer to *t*-tests on the bivariate difference between treatment and control observations.

As for the respondent-specific variables, 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 financial decision makers within 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; the average household size is 14.4. Table 1 also shows telecommunication expenditures as a proxy for income and two wealth proxies: flooring and sheep ownership.

The same descriptive statistics and balancing tests as in Table 1 can be seen in Table B1 in Annex B for the restricted sample and the non-experimental comparison group. For the restricted sample, it does not come as a surprise that the sample is similarly well balanced as the full sample, given that we stratified the randomization by village. For the non-experimental comparison group, we show the descriptive statistics in comparison to the whole experimental sample. Since we do not have data from 2009 for the non-experimental subsample, all values refer to 2015. Despite a careful selection of comparison sites, a couple of variables reveal a statistically significant difference. Taking also other household variables not shown in the table into account, there is, however, no indication of clear structural differences in the overall socio-economic conditions that would cause us to abstain from comparing the two groups in a supplementary non-experimental analysis below. For example, the comparison group households have better roofing on average, whereas the experimental group exhibits higher shares of livestock ownership. Expenditures are higher in the experimental group; per-capita expenditures are not.

Cookstove Usage and Intermediate Outcomes

The impact of the free lifecycle trial treatment on households' WTP critically depends on the imprint the treatment had on people's cooking habits, especially with regard to learning. We therefore discuss ICS ownership and usage after 2009 and a variety of cooking-related variables from 2015 in Table 2.

The ICS ownership rate in 2015 is at 20 percent, fairly equally distributed across the treatment and control groups. The reason for this is that uptake of the charity ICS is higher in the control group, where 17 percent own a charity ICS. In the treatment group, 11 percent own a charity ICS and 10 percent still have the 2009 ICS. ICS purchases other than those from the charity have virtually not taken place after 2009: in both the 2013 and the 2015 survey, only one household from the restricted sample reported to own a (self-purchased) Jambaar ICS that had not been distributed in our

2009 randomization. Other free distribution activities have not taken place. Table 2 thus conveys the intermediate finding that virtually no household had made an effort to (re-)invest into ICS by obtaining one from towns nearby or from vendors in Dakar. Cross-selling between treatment and control households had not happened either.

As described in Bensch and Peters (2015), adoption of the randomized ICS was high at the extensive and the intensive margin. In 2010, virtually all treatment households used the ICS regularly and the vast majority of meals was prepared on the new ICS: the ICS was the primary cookstove in 85 percent of households with ICS. In line with the expected lifetime of two to four years, ownership declined over time. Only about 14 percent stopped using the ICS within the first two years and in 2013, almost four years after the randomization, still 51 percent were using the ICS (see Table 2 and Bensch and Peters 2015). Even those 10 percent of treatment households that still own a randomized ICS in 2015 use it regularly, on average twice a day.

Table 2: Intermediate outcomes on cookstove usage

	Treatment mean (sd)	Control mean (sd)	Difference p-value
HH owns firewood ICS (share)	0.21	0.20	0.77
HH owns firewood ICS given out in 2009	0.10	0.00	0.00
HH owns firewood ICS from charity	0.11	0.17	0.18
HH owns woodfuel stove other than open fire	0.59	0.63	0.45
Usage duration of firewood ICS given out in 2009			
up to two years	0.14	-	-
at least two years	0.86	-	-
at least three and a half years	0.51	-	-
at least six years	0.10	-	-
HH mostly uses open fire for cooking (share)	0.62	0.56	0.36
HH buys firewood (share)	0.50	0.52	0.75
Firewood collection time (hours per week)	9.23 (11.57)	10.71 (12.69)	0.37
HH ever had a firewood ICS	1.00	0.39	0.00
Any HH member ever cooked on a firewood ICS (share)	1.00	0.47	0.00
Number of observations	94	142	

Table 2 also presents other key cooking-related household characteristics: 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 materializes in terms of time savings. The share of households that have at least one member who has ever tried some type of improved stove is relatively high, although this also includes other stoves considered as improved than the Jambaar ICS such as fixed mud stoves.

Impacts of Free Lifecycle Trial Period

Results on the impacts of the free lifecycle trial treatment period are presented in Table 3. The *Free lifecycle trial treatment* coefficient in the table corresponds to β_1 in the following OLS regression setup:

(1) $\quad WTP_i = \beta_0 + \beta_1 Treatment + \beta_2' X_i + \mu_i$

where the raw *WTP* is the outcome, *i* refers to the individual household, X_i to the vector of control variables, and μ_i to the error term. Standard errors are clustered by village for being our primary sampling unit. We run the regressions for the full sample and for the restricted sample in which we exclude the three charity villages. For both samples, we estimate a parsimonious model in which we only control for village fixed effects (Column 1 and 4) and a standard model in which we control for the variables presented in Table 1, i.e. the 2009 covariates and the respondent-specific characteristics elicited during the 2015 survey (Column 2 and 5). The point estimate in the full sample (Column 2) suggests that the free lifecycle trial increases household's WTP. The effect size is at 15 percent, but not statistically different from zero at conventional levels. Coefficients of control variables are not shown in the table, but can be taken from Table B2 in Annex B. Significant positive correlates of WTP are financial responsibility status of the respondent, a younger age of the respondent (reflected in the age difference to the interviewer) and, for the restricted sample, baseline ownership of a stove other than open fire.

Columns (4-6) depict the results for the restricted sample, so after dropping the charity villages. The free lifecycle treatment effect is higher (at 20 percent now), but less precise. The difference in point estimates between the full sample and the charity subsample portends the important role that existing ICS in a household have for the WTP revealed in the bidding game. Note that we interpret this only as a robustness check. It is difficult to tell what is driving this change in effect size, because of the unpredictable mechanisms that might be at work: many households in the charity subsample bid for a second ICS, which decreases their average WTP. At the same time, the charity ICS itself could have induced reference and learning effects, which increases noise because the stove is a similar make but maybe of different quality than the one offered in our experiment.

As discussed in the previous subsection, also in the restricted sample a notable fraction of treatment households still owns and uses the 2009 ICS. It is likely that their WTP is affected as well. In our main specifications in Columns (2) and (5) we do not account for this and thereby evaluate the real-world effect of a free-distribution ICS program on the adoption of the same ICS six years later. In this real-world interpretation, we treat people who still own an ICS as a natural part of the policy population, including their downward effect on the average WTP, since this is also what a real market would face if it reaches out to the region sometime after the free distribution intervention.

Yet, one might also be interested in the treatment effect of a free distribution on the WTP when all ICS have deteriorated, so on a green field. These coefficients give us a better idea of the net effect of learning and reference dependence, since for this we ideally want to know each household's WTP for a new stove, and not a second one. For this purpose, in an exploratory analysis we include a further control variable for whether the household still owns the 2009 ICS. As can be seen in Column (3) and (6), this leads to a higher average treatment effect. These estimations, though, must be interpreted with care, since the additional control variable is endogenous. The survival of the randomized ICS may also be due to household characteristics that are correlated with unobservable WTP determinants: Those households who still use the 2009 ICS

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might have a particularly high (or low) valuation of the ICS, which also drives the WTP.

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)
Free lifecycle trial treatment	863.48	910.34	1298.17	1283.93	1335.51	1668.66
	(733.27)	(625.27)	(691.41)	(1105.98)	(1006.53)	(1103.71)
	[0.26]	[0.17]	[0.09]	[0.28]	[0.22]	[0.17]
HH still owns randomized ICS in 2015	-	-	-4152.81	-	-	-5108.87
			(726.88)			(1466.06)
			[0.00]			[0.01]
Controls:						
Village	Yes	Yes	Yes	Yes	Yes	Yes
Respondent variables	-	Yes	Yes	-	Yes	Yes
Baseline covariates	-	Yes	Yes	-	Yes	Yes
Marginal mean for control group[a]	5987.17	5968.67	5815.53	6586.04	6565.81	6435.17
WTP increase	+14.4%	+15.3%	+22.3%	+19.5%	+20.3%	+25.9%
Maximal WTP drop according to 95% CI lower bound	-12.5%	-7.8%	-3.8%	-19.2%	-15.0%	-13.6%
Observations	233	233	233	153	153	153
Adjusted R-squared	0.04	0.07	0.09	0.00	0.07	0.09

Note: [a] calculated at the mean of the control variables. See Table B2 in Annex for coefficients of the full set of control variables.

Standard errors clustered by village are in parentheses and *p*-values in squared brackets.

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 3, is significant at the eight and eleven percent level in our main estimations in Columns (2) and (5). As a very conservative estimate of the free lifecycle effect, Table 3 also shows the lower bound of the 95% confidence interval. Even at these lower bounds, the WTP does not go down by more than 8 percent in the full sample and 15 percent in the restricted sample. We conclude from this that free distribution is very unlikely to have substantial negative effects on adoption in the long run; negative

effects of reference dependence (if they exist) seem to be largely compensated by positive learning effects.

Given that we did not randomize subsidy levels in the 2009 RCT and do not avail of any other exogenous variation in determinants of learning and reference dependence, unlike Dupas (2010) and Fischer et al. (2014) we cannot dig deeper empirically into these mechanisms. We do have some potential proxies from the survey data for an exploratory assessment, but they show too little variation: ICS usage intensity in 2010 may serve as a proxy for learning (although it would also be endogenous to the valuation of the ICS), but virtually all treated households (88 percent) used the ICS at least twice per day, so we cannot expect differences in learning for them. We furthermore asked questions on *beliefs about returns*, which could be related to learning. Yet, the answers do not show any exploitable variation either: In both the treatment and the control group, 99 percent of households stated in 2015 that they expected the new ICS to reduce their expenses on energy.

Beyond the treatment effect, a very notable result is the level of the WTP. The simple means are 6,300 and 7,100 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. A convenient feature of the BDM mechanism is that – different from simple take-it-or-leave-it approaches – it allows for individual WTP estimates per customer and thus yields higher-resolution data that can be depicted as a demand curve (see Figure 2). It can be derived from this curve that 68 and 75 percent of households in the full and restricted samples, 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 shares of 16 and 19 percent make high enough bids. This is a remarkable 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.

Figure 2 also suggests that WTP among treatment households may not always be strictly above the WTP of control households. We run quantile regressions to compare treatment and control units more thoroughly across different WTP levels. Point estimates of the impact coefficient remain positive across the entire conditional WTP distribution (see Figure C1 in Annex C), thus supporting the results on the mean impact found above. Moreover, in Annex C we conduct some robustness checks on outlier sensitivity, since twelve percent of households in the treatment group and seven percent in the control group make bids higher than 10,000 CFA F. Results on point estimates and confidence intervals are very similar. In another sensitivity analysis we drop the few households that opted out of the purchasing process (see Annex C as well).

Figure 2: ICS demand curve according to bids in BDM purchase offer
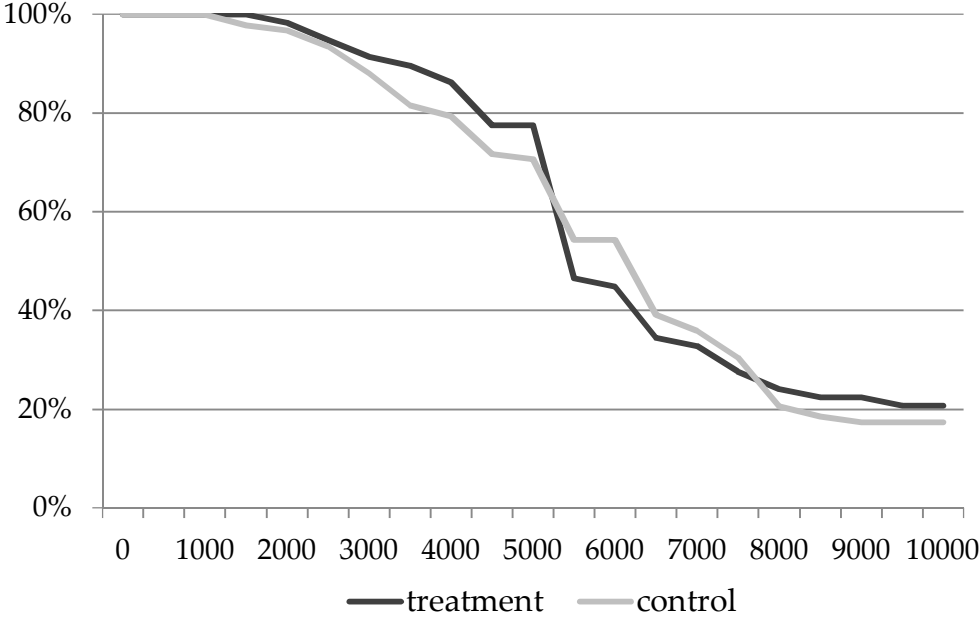
Note: This figure refers to the restricted sample excluding the charity subsample.

Spillover Effects

Because ICS were randomly distributed within villages in 2009, treatment and control group households are likely to be in contact so that spillover effects are possible, both for learning and for reference dependence. Control households may have learnt about

ICS benefits from treated neighbors in their village, but likewise they might anchor their WTP to the zero price ICS that treatment households received in 2009. This would be especially problematic if the null effect observed in the previous section was driven by reference dependence in the treatment group that spilled over to the control group households (indirect reference dependence). In this case, our conclusion of no negative effects of free distribution would be false.

We explore 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 *Experimental Design*). As can be seen in Figure 3, the WTP in these comparison villages barely differs from the WTP in the experimental control group (restricted sample). The data underlying this figure comes from estimating a variant of the specification presented in Table 3: Willingness to pay impact estimates, Column (5) (see Table B3 in Annex B for the regression results). Three changes are made: first, instead of the treatment-control dummy, the specification now includes 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. Second, since we do not have 2009 data for the non-experimental sample and in order to account for the slight imbalances described above, we now include control variables measured in 2015. Third, in order to only include those variables that are plausibly non-responsive to the 2009 treatment, we do not include an ICS ownership variable (summary statistics are shown on the right side of Table B1).

Suppressing concerns about a selection bias for a moment and 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 eases concerns about survey effects, which might have been induced by reciprocity and our repeated presence in the experimental villages, for example.

Figure 3: Willingness to pay in experimental groups and comparison group
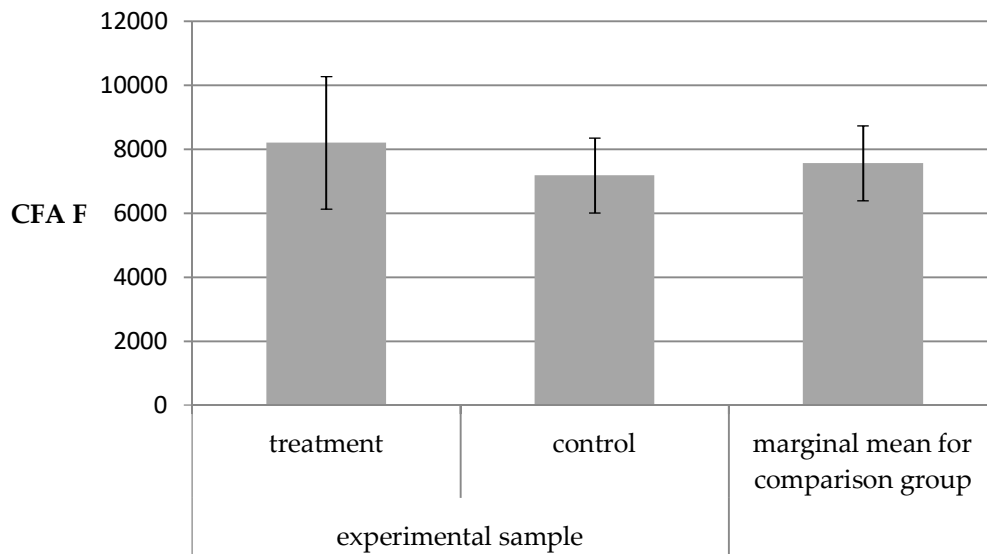
Note: The values in this figure are derived from a regression model with the same specification as in Table 3, Column (5) that now includes a polytomous categorical treatment variable with the comparison group as the base case. Moreover, all 2015 variables listed in Table B1 in Annex B are included as control variables – except for the ICS ownership variable. 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, Gelbach and Miller 2008) yields only marginally different confidence intervals than the ones depicted in the graph, which are based on conventional clustered standard errors.

Discussion and conclusion

Our empirical analysis has shown that free distribution of improved cookstoves (ICS) is very unlikely to substantially decrease the willingness to pay (WTP) for the product in the long run. Even the worst-case WTP decreases determined at the lower bound of 95 percent confidence intervals might be politically acceptable in the light of the high adoption triggered by the free distribution and the strong external effects of ICS. We thereby follow up on Dupas' (2014) seminal work on how one-off subsidies for malaria bednets affect adoption. She explicitly discusses the transferability of her findings for ICS, arguing that people "may underestimate the returns to switching" and thus she hypothesizes that "one-time subsidies for cookstoves […] have the potential to boost

subsequent adoption through learning effects." We tentatively confirm Dupas'
prediction to the degree that point estimates for the net effect are positive, although
not statistically significant. While we cannot disentangle reference dependence from
learning effects, it seems that the former is at least compensated by the latter after free
distribution. These results are also consistent with what Fischer et al. (2014) observe
for products with positive learning.

A further notable result of our study is the absolute level of WTP revealed by both
treatment and control households. With an average of around 11 US$, the WTP is very
high compared to previous cookstove WTP studies (Beltramo et al. 2015; Mobarak et
al. 2012) and considerable fractions also pay prices above what vendors charge in
town. Participants took these expressions of their WTP seriously as evidenced by a
repayment rate of over 95 percent among those households who were allowed to buy
the stove.[13] One plausible reason for the high WTP is the firewood scarcity in our study
region, which is representative for other Sahelian places, but might differ from more
biomass-abundant regions, for example more tropical parts of Africa.[14]

Despite the high WTP there is no vibrant local ICS market reaching the villages.
Virtually no household acquired an ICS between the distribution in 2009 and our 2015
survey. Why has the market not reached out to these villages? The reasons likely relate
to very high transaction costs on rural markets that have to be added to the in-town
market price. Likewise, while generally the BDM method is found to be a very accurate
reflection of the true WTP (see Berry, Fischer and Guiteras 2019), one might speculate
that the WTP observed in our set-up is higher than what people reveal on regular
markets. More specifically, due to the door-to-door marketing feature that is implicit
to the BDM method, especially male household members (oftentimes the financial
decision makers) probably dedicated more attention to the offer than they usually do

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[13] This cannot be taken for granted. See, for example, Grimm et al. (2018) and Tarozzi et al. (2014), who use
payment targets similar to ours and observe repayment rates of between 60 and 70 percent.

[14] In Bensch and Peters (2017), we qualitatively explore different explanations for this high WTP in more detail.

to regular market offers. Not least, the two-month payment target in the harvest period served as an interest-free short-term loan and commitment device. Financing components proved to increase the willingness to pay already in similar contexts, for example for latrines in Cambodia (Ben Yishay et al. 2017) and improved cookstoves in Uganda (Levine et al. 2018), but not for solar kits in Rwanda (Grimm et al. 2018). Private vendors could replicate all this, but it obviously increases transaction costs. This bespeaks a variety of barriers and frictions that make rural market exploration a highly risky endeavor. Vendors in such a market environment would have to price in risks and logistics, leading to rural end-user prices that exceed urban prices considerably (see also Adams et al. 2016, Barriga and Fiala 2018, Grimm et al. 2018, and Levine et al. 2018).

In sum, this paper and our previous study (Bensch and Peters 2015) have shown that households are eager to use the ICS even if it is received as a gift, which is in line with Bates et al. (2012) as well as Grimm et al. (2017). More specifically, our study shows that households reveal a high valuation when being offered an ICS for purchase – but do not make a pro-active effort to obtain one in town. This suggests that policy interventions should increase the salience of ICS, for example through vigorous marketing approaches (including a strengthening of supply chains) that reach out into the villages. In addition, because this would further increase transaction costs, partial subsidies are probably required to make the ICS business attractive in rural areas. The strong positive livelihood and environmental effects provide the necessary economic arguments to subsidize ICS. Carbon finance could be a long-term funding source as long as the respective ICS effectively reduces climate-relevant emissions. Moreover, in some areas like very arid regions, fuelwood scarcity might call for an urgent policy intervention. Especially in such contexts, free distribution could be an effective

instrument to achieve a rapid increase in ICS adoption and our analysis provides a proof of concept that this does not necessarily spoil future markets.[15]

That said, some external validity concerns apply (see Peters, Langbein and Roberts 2018). It is important to emphasize that adoption is case-specific and likely to vary across regions and product types, including ICS types. Subsidization might decrease the future WTP if no learning or even net negative learning is involved (Fischer et al. 2014; Luoto et al. 2012). This is likely to hold true for ICS that involve high non-fuel usage costs, for example, because they are not convenient to use or imply intensive maintenance work (see, for example, Hanna, Duflo and Greenstone 2016 and Beltramo and Levine 2013). Moreover, while we observed a high usage intensity despite the free distribution, concerns about screening and sunk cost effects reducing usage intensity in give-away programs might be stronger in other settings (see Arkes and Blumer 1985, Ashraf, Berry and Shapiro 2010, Beyene et al. 2015b, Cohen and Dupas 2010, Cohen, Dupas and Schaner 2015). Such heterogeneities in behavior and product features call for region- and product-specific experimentation similar to what this study has done before rolling out any subsidy policy (see also Berry, Fischer and Guiteras 2019; Lybbert et al. 2018).

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[15] Omotilewa, Ricker-Gilbert and Ainembabazi (2019) come to a very similar conclusion in their study on one-time subsidies for hermetic grain storage bags that have newly been introduced among smallholders in Uganda.

Appendix: 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
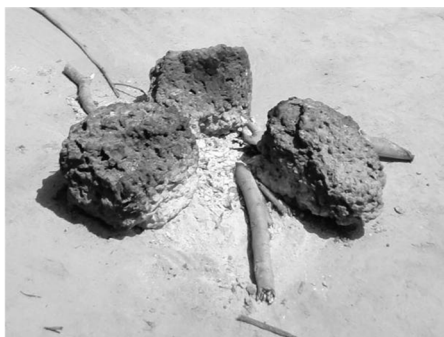
Three-stone stoves   Os

Traditional metal stoves

Cire khatach (crop residues)   Cire wood   Malagasy stove

Improved Cooking Stove (ICS) Jambaar

Sources: Authors' own photographs

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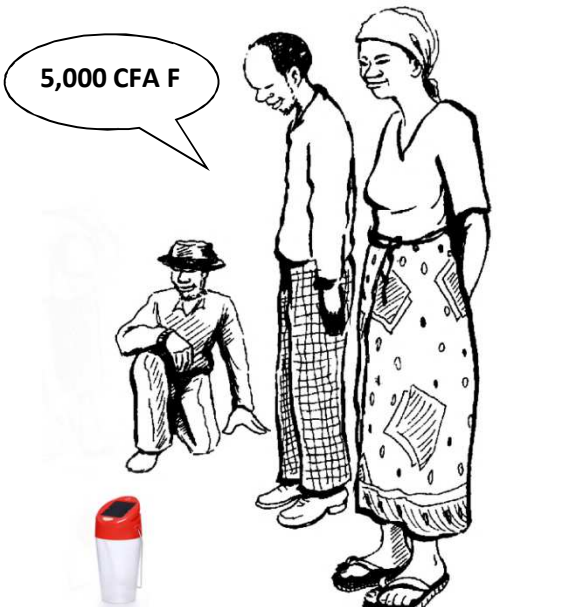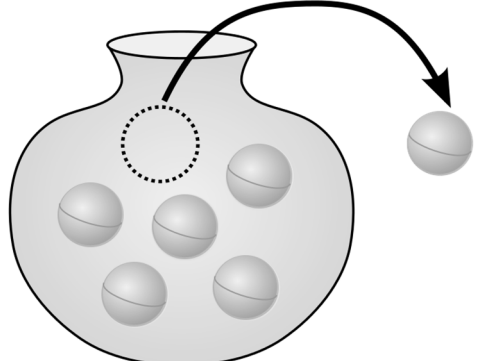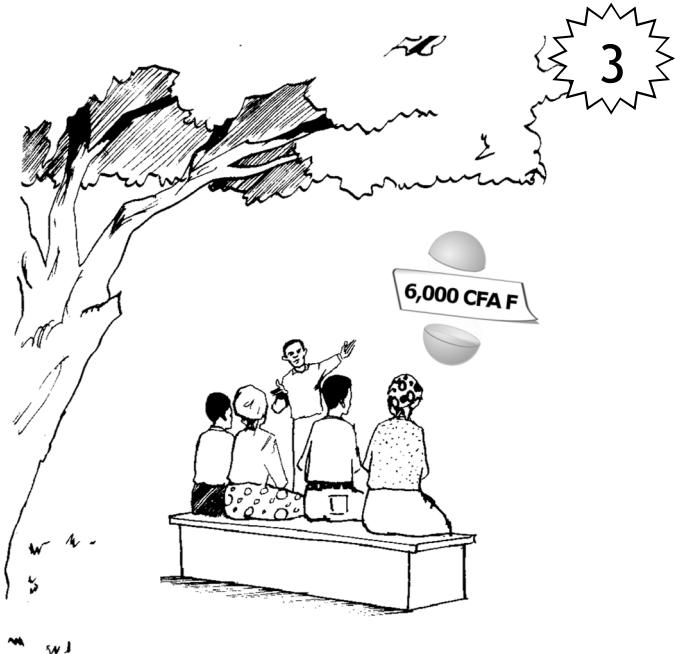
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Annex A: Showcard used to practice the BDM procedure

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.

Annex B: Background data

Table B1: Balancing test for the restricted sample and for the experimental and non-experimental sample

	restricted sample			experimental and non-experimental sample		
	Treatment mean (sd)	Control mean (sd)	Difference p-value	Experimental (restr. sample) mean (sd)	Non-experimental mean (sd)	Difference p-value
Respondent-specific characteristics at time of stove purchase experiment (elicited in 2015)						
Age difference respondent to interviewer	16.41 (14.68)	14.33 (16.93)	0.43	15.15 (16.06)	15.42 (13.38)	0.89
Person taking financial decisions in HH present during stove purchase experiment (share)	0.70	0.69	0.80	0.69	0.81	0.03
Person responsible for cooking in HH present during stove purchase experiment (share)	0.49	0.54	0.54	0.52	0.47	0.35
Sociodemographic baseline variables (elicited in 2009 [restricted sample] and in 2015 [exp. vs non-exp.])						
Male head of household with multiple spouses (share)	0.45	0.28	0.02	0.40	0.33	0.28
Head of HH attended koranic or Arabic school (share)	0.81	0.73	0.30	0.83	0.74	0.07
HH size	16.74 (16.15)	13.28 (9.65)	0.10	14.08 (9.29)	11.89 (7.34)	0.04
Economic baseline variables (elicited in 2009 [restricted sample] and in 2015 [exp. vs non-exp.])						
HH has cement flooring (share)	0.31	0.30	0.91	0.60	0.61	0.86
HH owns sheep (share)	0.65	0.65	0.96	0.50	0.38	0.04
HH's monthly telecommunication expenditures (CFAF)	4,570 (5,550)	4,980 (7,260)	0.70	13,490 (16,370)	10,420 (10,260)	0.08
ICS baseline variable (elicited in 2009 [restricted sample] and in 2015 [exp. vs non-exp.])						
HH owns woodfuel stove other than open fire	0.47	0.50	0.70	0.58	0.61	0.62
Number of observations	62	94		156	117	

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

Table B2: 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)
Free lifecycle trial treatment	863.48	910.34	1298.17*	1283.93	1335.51	1668.66
	(733.27)	(625.27)	(691.41)	(1105.98)	(1006.53)	(1103.71)
HH still owns randomized ICS in 2015	.	.	-4152.81***	.	.	-5108.87***
			(726.88)			(1466.06)
Age difference respondent to interviewer		-53.00*	-49.87*		-79.91*	-76.91*
		(25.12)	(25.40)		(34.76)	(36.44)
Person taking financial decisions in HH present during SPE		1685.34*	1800.83**		3157.53**	3067.88**
		(801.50)	(710.92)		(1209.39)	(1087.38)
Person responsible for cooking in HH present during SPE		-282.89	-132.83		-349.51	-259.44
		(1434.63)	(1347.07)		(2221.92)	(2079.71)
Male head of HH with more than one wife		1019.00	824.76		1398.89	1153.38
		(1333.12)	(1320.21)		(2054.37)	(2014.33)
Head of HH attended koranic or Arabic school		-674.91	-708.23		-1955.47	-1984.21
		(1101.48)	(1113.26)		(1715.61)	(1693.04)
HH size, ln		609.69	698.53		394.45	719.93
		(635.37)	(540.40)		(1122.53)	(879.07)
HH has cement flooring		-722.44	-617.96		-1240.25	-1092.03
		(735.43)	(716.10)		(1075.97)	(1045.33)
HH owns sheep		-648.87	-591.73		-693.07	-790.19
		(516.55)	(537.69)		(676.11)	(764.67)
HH's monthly telecomm. expenditures, ln		-24.61	-23.35		3.46	15.72
		(145.35)	(141.27)		(211.69)	(205.73)
HH owns woodfuel stove other than open fire		954.87	1060.74		1840.28*	1729.25
		(825.64)	(780.28)		(937.13)	(936.73)
Controls:						
Village	Yes	Yes	Yes	Yes	Yes	Yes
Respondent variables	-	Yes	Yes	-	Yes	Yes
Baseline covariates	-	Yes	Yes	-	Yes	Yes
Observations	233	233	233	153	153	153
Adjusted R-squared	0.04	0.07	0.09	0.00	0.07	0.09

Note: 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; HH = households; * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table B3: Willingness to pay impact estimates (for treatment, control and comparison group)

	Willingness to pay (in CFAF)		
outcome:			
estimation method:	OLS		
village sample:	restricted sample + comparison group		
	Coeff.	95% Conf. Interval	
	(1)	(2)	(3)
comparison group (non-experimental)	reference case		
treatment group (experimental)	636.13 (965.29) [0.52]	-1434.22	2706.47
control group (experimental)	-386.79 (544.82) [0.49]	-1555.30	781.72
Observations	271		
Adjusted R-squared	0.05		
Controls:			
Village	Yes		
Respondent variables	Yes		
Sociodemographic variables (2015)	Yes		
Economic variables (2015)	Yes		
Marginal mean for comparison group‡	7562.55	6394.04	8731.06

Note: Results from the regression model underlying Figure 3. ‡ calculated at the mean of the control variables.

Annex C: Sensitivity Analyses

We run different complementary analyses to scrutinize the sensitivity of our main results. To start with, in Figure C1 we estimate quantile regressions to compare treatment and control units more thoroughly across different WTP levels.

Figure C1: Quantile regression results for the free lifecycle trial impact on WTP
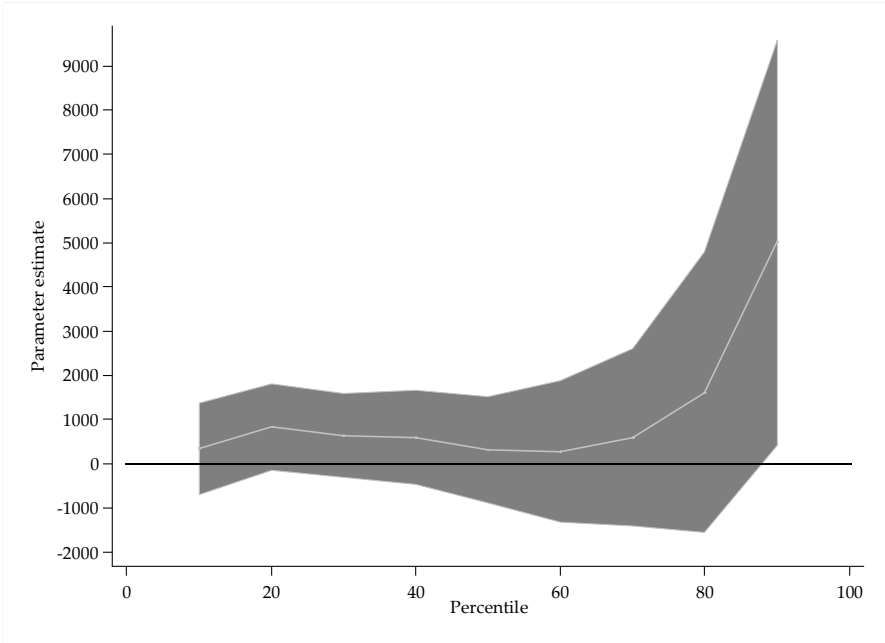
Note: Results from a quantile regression using the specification of column (5) in Table 3.

As a less outlier-sensitive alternative to our estimation equation (1), we run the same OLS regression with the natural logarithm of the WTP as outcome variable. This confirms our results, although the estimate in the unrestricted sample is less precise and generally hints to lower levels of percentage increases in WTP: point estimates of treatment effects (corresponding to the "WTP increase" in Table 3) are at six percent (p=0.44) for the estimation equivalent to Column (2) and twelve percent (p=0.27) for the estimation equivalent to Column (5). The maximal WTP drop according to 95% CI lower bound is 9.5 percent and 10.7 percent, respectively, for these two specifications.

We also test the sensitivity of our results by running the same regressions as in Table 3 after excluding 15 households with ambiguous bidding behavior. These include the

households already referred to in the *Experimental design* section of the main text (i) who opted out of the experiment before submitting a bid (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 2 and 5 in Table 3) from 910 to 1,080 CFA F and from 1,335 to 1,360 CFA F, respectively.