Supply-side Constraints to Technology Adoption:

The Market for Energy-efficient Cookstoves in Rural Senegal

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December 2024

Abstract:

Increasing the diffusion of technologies with positive externalities is a global policy priority. We investigate the market for energy-efficient biomass cookstoves in rural Senegal, which have private and external benefits, but uptake is low. Our pre-specified study covers stove producers and intermediary vendors on the supply side, and we elicit demand from rural households using real purchase offers. We randomly relax supply-side constraints among vendors by providing demand information, marketing materials, and a transportation grant to bridge the last mile to villages where customers reside. We find that demand information and marketing materials alone do not increase vendor sales, but only in combination with the transportation grant. Yet, overall sales are still too low to deliver noteworthy take-up at the household level. We also show that market conditions are structurally challenging: markups are lower than for competing products and demand is highly elastic and volatile. This combination of high risks and low profits need to be addressed by policy interventions if large-scale diffusion is the goal.

JEL: D22, D47, O13, Q28.

Keywords: Technology adoption, supply side, improved cooking, information constraints

Acknowledgements: This research was primarily funded by the International Initiative for Impact Evaluation (3ie) and received complementary funding from the German Federal Ministry of Education and Research (BMBF) within the framework of the Strategy „Research for Sustainability" (FONA), BMBF funding code: 01LA1802A, and the Duke University Energy Initiative. Usmani gratefully acknowledges support from the Duke Global Health Institute and the Cornell Atkinson Center for Sustainability. We are thankful to 3ie for technical review and support throughout the study. Field work was implemented by Samba Mbaye, Medoune Sall and colleagues at the Centre de Recherche pour le Développement Economique et Social (CRDES). We are grateful for expert advice from Birame Faye from the Government of Senegal, Christoph Messinger, Mireille Afoudji Ehemba, Viviane Sagna and the Energising Development Senegal team. We thank Jana Eßer and Daniela Breitmaier for excellent research assistance. Ann-Kristin Reitmann, Marcello Pérez-Alvarez, Gunther Bensch, and conference participants at the EAERE (Limassol) and CSAE 2024 provided helpful comments. This research was pre-registered in the Registry for International Development Impact Evaluations (RIDIE) under RIDIE-STUDY-ID-59c9e0f49a591 in September 2017. The pre-analysis plan (PAP) was published in March 2019 prior to endline data collection. We obtained ethics approval from DUKE University campus Institutional Review Board (IRB Protocol Number 2018-0061). All correspondence to Maximiliane Sievert (maximiliane.sievert@rwi-essen.de).

1. Introduction

People in rural areas of low-income countries often do not use improved technologies that can increase social welfare (Cirera et al., 2022; Keller, 2004; Suri & Udry, 2022). Considerable research has examined demand-side barriers to uptake and sustained use of such technologies (Foster & Rosenzweig, 2010). Supply-side constraints, however, have received less attention, and little is known about how improved technologies, typically available only in urban markets, reach rural consumers. The role of intermediaries between producers and consumers have mainly been studied in agricultural value chains, and mostly in the context of how rural produce reaches urban markets (Barrett et al., 2022). The limited empirical research on the reverse direction – from urban to rural markets – focuses almost exclusively on technologies that enhance agricultural production, such as improved seeds, fertilizer, or storage technology (Aggarwal et al., 2022; Aker et al., 2023; Minten et al., 2013; Suri, 2011), or more generally on retail chains for consumer goods (Atkin & Donaldson, 2015; Grant & Startz, 2022).

We investigate the urban-rural supply chain by examining the market for a specific consumer good with positive externalities in Senegal, namely energy efficient biomass cookstoves (EEBCs). EEBCs can generate benefits for people's time use, local forests, and the global climate (Bailis et al., 2015; Bensch et al., 2021; Krishnapriya et al., 2021; Shindell et al., 2012). Globally, nearly three billion people use traditional stoves burning firewood and charcoal (IEA, 2020; Rose et al., 2022). The potential for social welfare gains from technology diffusion in this sector is substantial, as the efficiency of traditional stoves is low and can be improved at modest costs (Jeuland et al., 2018; Jeuland & Pattanayak, 2012). In Sub-Saharan Africa (SSA), biomass for cooking accounts for more than 75% of total primary energy demand (IEA 2019, excluding South Africa). While promoting the diffusion of EEBCs is high on the policy agenda[1], their adoption remains very low, particularly in rural SSA. In rural Senegal, where our study is located, 94% of households use biomass fuels (IEA et al., 2023) and EEBC penetration is only 2% (Jeuland et al., 2021).

Our study provides a comprehensive assessment of the entire EEBC market in Senegal by surveying all key actors along the supply chain: urban EEBC producers, intermediary vendors

[1] See for example ESMAP (2015) for a description of policy initiatives and the role of EEBC (referred to as "improved cooking solutions").

in rural markets, and potential rural customers. We pre-specified the study in its full scope. Our aim is to pinpoint barriers on the supply and demand side that may explain low adoption rates. First, in a village demand survey, we elicited rural consumers' revealed willingness to pay (WTP) for EEBCs using a real-purchase offer, seeking to understand rural demand for these products. Second, we conducted a randomized controlled trial (RCT) among vendors in rural markets to test if information frictions hinder the development of the rural EEBC market. Our underlying theory is that vendors face a high-cost-high-risk situation where reducing information frictions can incentivize vendors to enter the rural EEBC market. The theory is informed by formative research we conducted, prior to the pre-analysis plan and the RCT, among policy practitioners, EEBC producers, and rural market vendors. In our RCT, we provided vendors with (1) information about urban producers and rural demand as well as marketing materials, and (2) a cash grant meant to visit rural customers and gain experience with selling EEBC. Third, we explore alternative supply-side barriers descriptively, by examining vendor and producer markups for different cookstoves. The markups are crucial for price formation and indicate whether the costs of reaching rural consumers are prohibitively high. The analyses generally follow our pre-analysis plan (PAP), published prior to endline data collection.[2]

We study the market for a simple and low-cost EEBC known as the *Jambar*. The Senegalese Government has promoted production of the *Jambar* since the mid-2000s, mainly by supporting urban producers. Two versions of this stove exist: a charcoal version for urban markets and a firewood version for rural markets. Both are locally manufactured in urban areas. The *Jambar* has widely penetrated urban areas (Rose et al., 2024), but diffusion into rural areas is low, despite considerable savings potential (Bensch & Peters, 2015) and a WTP that clearly exceed urban prices (Bensch & Peters, 2020).[3]

More specifically, our empirical approach consists of the following modules: In our village demand survey, we measured revealed WTP through an incentive-compatible Vickrey second-price auction (Vickrey, 1961) with over 900 participants in 60 villages. Furthermore,

[2] All deviations from the PAP and the rationale for those deviations are enumerated in Appendix A – Table A. 1. Most relevant deviations are the addition of three non pre-specified analyses: (i) the analysis of markups along the supply chain, (ii) the more thorough analysis of demand information and (iii) a longer-term data collection.

[3] If not otherwise stated we refer to the *Jambar* firewood version, not the charcoal version. The firewood *Jambar* is well-adapted to local cooking needs, which is a major difference to other EEBC that have been studied in the literature (Jeuland et al., 2020).

we collected detailed information on the cost structure of stove production and pricing from all *Jambar* producers – all located in urban areas – in the country. We further tracked stove trade at 60 weekly rural markets, which serve as the critical bridge between urban producers and rural consumers. Finally, we conducted an RCT among 127 vendors of kitchenware frequenting these rural markets, randomly assigning them to a control arm or to one of two treatment arms. Vendors in the first treatment received *Jambar* demand information elicited in the village demand survey, contact information of urban *Jambar* producers, and a starter kit with two *Jambars* and marketing materials (henceforth referred to as the 'information treatment'). Vendors in the second treatment group received the full information treatment plus a one-time unconditional cash grant worth USD 31 (the 'grant treatment').[4] This grant was nominally earmarked for *Jambar* transport (for example, from markets to villages), but this conditionality was not enforced. To increase statistical power, following McKenzie (2012), we tracked our key impact indicators, monthly sales of producers and vendors, on a monthly basis for 12 months before and 7 months after the RCT. Additionally, we conducted two long-term follow-ups with vendors two and 2.5 years after intervention.

Our village demand survey suggests that around 10 percent of consumers are willing to pay the firewood *Jambar* price charged by rural market vendors, indicating that there is a market segment that could be served commercially. We used this data to create the information material for both treatment arms in the RCT. In this RCT we find that vendors in the grant treatment group increase firewood *Jambar* sales by approximately 0.9 stoves per month following the intervention. Given the near-zero baseline sales (around 0.08 stoves per month), this effect is substantial in relative terms, reflecting a more than 1000 percent increase. In contrast, we find no discernible impact of the information treatment without the grant. It is also noteworthy that the effect of the grant is driven by a small number of what we call "super-sellers" who start selling large quantities of *Jambars*, while the sales of many grant-treated vendors do not respond at all.

These results show that some vendors can be incentivized to enter the *Jambar* market. The overall effect size on the market is an increase of 43 additional firewood *Jambars* sold per month. This is not irrelevant given the modest investment of our intervention, but probably

[4] We convert all local prices to USD using the 2018 exchange rate of 575 USD=1 XOF.

negligible given our intervention's substantial regional scope: we targeted over half of Senegal's administrative regions (including the most densely populated ones), more than 40 percent of all rural markets in these regions, and nearly 60 percent of the relevant vendors. The regions served by our study markets are home to approximately 0.5 million firewood using households. Given the substantial latent demand for improved cooking solutions in our study regions, the intervention cannot be considered successful if large-scale diffusion is the policy goal.

Exploring the cost and price structures of producers and vendors suggests that profit margins in the *Jambar* market are not sufficiently high to offset the multiple risks and uncertainties associated with marketing EEBCs. At baseline, we find that firewood *Jambar* markups are lower than those on other stoves, for both producers and vendors, which disincentivizes firewood *Jambar* sales. Increasing the sales price is impossible without losing a significant customer share: If vendors increased the price by around 3 USD to 16.60 USD, thereby doubling their markup, our village demand survey suggests they would lose 23% of their customers. Increasing markups by 150% (+4.50 USD) would reduce demand by more than 70%. Additionally, we observe substantial heterogeneity in WTP across villages, and lower WTP than was found in a prior, similar study (Bensch & Peters, 2020). This hints at considerable demand variation across years and villages, for example due to agricultural yields.

Our paper is unique in its comprehensive approach to mapping the complete market for an under-adopted consumer good with positive externalities. It contributes to the literature on efficient consumer good distribution structures and the role of intermediaries. For example, while intermediaries are often assumed to exploit their market power, leading to a decrease in welfare, recent empirical papers document that rural consumers can benefit from multiple intermediaries in consumer good supply chains. Grant and Startz (2022) show that longer supply chains for consumer goods like apparel and electronics have positive welfare implications in Nigeria, arising from economies of scale for sourcing. Atkin and Donaldson (2015) observe that intermediaries' markups for branded consumer goods (mainly food, drinks, and beauty products) are substantially lower in remote rural areas of Ethiopia and Nigeria compared to urban areas. They also identify high fixed trade costs as the reason for low markups, resulting in low product variety in many rural areas in low-income countries.

A large and related literature deals with the establishment of well-functioning and integrated markets for agricultural produce (Aker, 2010; Allen, 2014; Arimoto et al., 2019; Bergquist & Dinerstein, 2020; Burke et al., 2019; Casaburi & Reed, 2022; Emran et al., 2021; Iacovone & McKenzie, 2022; Macchiavello & Morjaria, 2015). While similar mechanisms such as information frictions are under scrutiny here, all these papers look at the relationship between rural producers selling their produce to urban consumers or exporters, often through intermediaries. The dynamics of supply chains in the reverse direction – from urban to rural areas – are arguably different. Our paper complements the small literature on urban-to-rural supply chains for agricultural inputs such as fertilizer and seeds, which shows that high transport costs, poor infrastructure, and market structure inhibits adoption of beneficial inputs (Aggarwal et al., 2022; Aker et al., 2023; Minten et al., 2013; Suri, 2011).

Our findings furthermore relate to recent empirical evidence in the technology diffusion literature explaining under-adoption of technologies that are ostensibly beneficial to adopters. For example, Alpizar et al. (2024) show that adopters' uncertainty about the life span and performance of resource-conserving technologies, together with risk aversion and high discount rates, can explain low technology adoption. Relatedly, Iacovone & McKenzie (2022) document unintended side effects of introducing new technologies to solve firm coordination problems in the vegetable supply chain, highlighting the complex nature of competition among small retailers.

Our study also responds to calls for studying supply chains in rural stove and fuel markets (Clean Cooking Alliance, 2019; Lewis et al., 2015; Pattanayak et al., 2019; Puzzolo et al., 2019; Shupler et al., 2021). Pattanayak et al. (2019) show that a combination of supply and demand promotion stimulates adoption of EEBC and electric stoves. We try to push further by cleanly separating the role of supply-side constraints.

2. Background

2.1. The Senegalese cookstove market

To lower the adverse effects of cooking with biomass using inefficient traditional combustion methods, the Government of Senegal promotes a simple and low-cost EEBC, called the *Jambar*, as part of its national FASEN (*Foyers Amelioré au Sénégal*) cookstove program. The *Jambar* is produced in Senegal and has two designs, which are adapted for charcoal and firewood, respectively (Figure 1, stoves (1) and (2)), and which come in three different sizes that cater to different cooking needs and pot sizes. The charcoal stove is tailored to urban and peri-urban areas where charcoal is the dominant fuel. In rural areas, which are the focus of this study, firewood is the dominant fuel because it can often be collected free of charge, in contrast to charcoal that has to be bought. Switching from firewood to charcoal does not make sense for most rural households because the opportunity cost of firewood collection rarely exceeds charcoal costs.[5] The producer retail price for the firewood *Jambar* varies from USD 8 to USD 14, depending on the stove size and retail location. The FASEN program provides a list of proposed retail prices that is not binding, but often used by producers to set their retail price.

The *Jambar* is well-adapted to local cooking needs and prior research has shown that the firewood version can substantially reduce households' firewood consumption (Bensch et al., 2024; Bensch & Peters, 2015). Since 2007, the FASEN program has trained artisans to produce and market the *Jambar*, aiming to establish a self-sustaining market for the stove. FASEN's supply-side activities primarily focus on training and providing producers with the skills and equipment needed to produce the *Jambar*. It also strives to establish partnerships with end users such as women's groups and other cooperatives. In addition, FASEN engages in information campaigns aimed at stimulating demand in rural areas.[6]

Jambar producers are metalworking artisans (e.g. whitesmiths) who typically work in small workshops in urban centres.[7] Between 2007 and 2018, FASEN reportedly trained 228 such producers, who sold over 1.4 million *Jambar* cookstoves over that same period, according to FASEN's monitoring data. Apart from *Jambars*, these producers also manufacture other EEBCs, traditional stoves, doors, and windows. According to our survey of producers, *Jambars* are one of the "most important" sources of revenue for 70 percent of producers and a source of revenue of "medium importance" for 24 percent. However, most of these sales were of the charcoal variant (responsible for 76 percent of all sales). Sales of the firewood variant were substantially lower and fluctuate from month to month. Thus, fewer than half of all *Jambar* producers produce firewood *Jambars*.

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[5] In urban areas around 25 percent of households use charcoal and only 15 percent use firewood. In rural areas, 74 percent of households use firewood and only 13 percent use charcoal (ANSD & ICF, 2020).

[6] The activities include TV and radio campaigns, cooking demonstrations, contests, and distribution of printed materials such as flyers, posters, road signs and T-shirts.

[7] More information on the producers can be found in Appendix C.

Products coming from urban producers or abroad and reaching rural consumers are typically traded in rural weekly markets, known as *loumas*. These markets take place in peri-urban and rural areas on fixed weekdays and offer food products, clothes, kitchen items, sanitary products, and electronics to the local population. Rural households typically travel to urban areas only for relatively hard-to-find products, making these markets a key commercial touchpoint for households. In these markets, cookstoves are usually sold by highly mobile, entrepreneurial vendors who specialize in kitchenware and related products. These kitchenware vendors often operate in multiple markets (three on average at baseline) to maintain more continuous business, and travel to urban areas to buy stoves directly from producers or wholesalers. One third of these vendors are female. Most do not have formal education except for Koranic schooling. In our baseline survey, these vendors' average revenues in a typical month amounted to around USD 2600.[8] Figure 1 shows the main stove types sold in rural markets. Stoves (1)-(3) are simple EEBC manufactured by FASEN-trained producers, while stoves (4)-(7) are basic, traditional stoves, with the last (7) being the one most used. Supply of very advanced (i.e., gasifier or electric) stoves is rare.

Figure 1: Main stove types offered in rural markets

Firewood *Jambar*	Charcoal *Jambar*	*Sakkanal*	Simple Metal Stoves		Traditional stoves	
(1)	(2)	(3)	(4)	(5)	(6)	(7)

Note: Stoves (1)-(3) are EEBCs. Simple Metal Stoves (from left to right): Bili (4), Bili amelioré (5). Traditional stoves: Malgache (6), Nopale (7). The Jambar comes in three different sizes that cater to different cooking needs and pot sizes.

2.2. *Theoretical rationale of the randomized intervention*

Parts of our empirical approach is explorative and descriptive in nature, but the core – the RCT among intermediary vendors – is based on a clear theory of change. We developed this theory collaboratively with policy practitioners in the Government of Senegal's *Jambar* dissemination program FASEN by identifying potential key barriers in the urban-to-rural *Jambar* supply chain: information frictions among the intermediary vendors at rural markets. The

[8] The revenue distribution is right-skewed, with the median revenue amounting to only 870 USD.

randomized intervention therefore incentivizes these vendors to experiment with entering the firewood *Jambar* market in order to experience the profits that can be made. These vendors normally sell their produce at the rural marketplace and rarely reach out to villages to offer their products directly to rural customers at home. In baseline interviews, vendors stated their belief that demand for *Jambar* was low and that customers were poorly informed about *Jambar's* benefits.[9] Our overarching hypothesis is that testing these beliefs by piloting exemplary *Jambar* sales will help vendors update their business strategy. Our hypothesis is informed by earlier *Jambar* demand studies in rural Senegal (Bensch & Peters, 2020), which provide evidence that the willingness to pay might be high enough to make the business profitable. It is nevertheless a risky undertaking, and hence cost barriers of transportation and customer search are reasonably prohibitive from the vendor's perspective. Our theory therefore suggests that these barriers for vendors to explore the market potential in the villages need to be removed for them to enter the *Jambar* market.

We developed two different treatment arms: one treatment that uses ground-proofed demand information and marketing materials to reduce information frictions directly (the 'information treatment'); and a second treatment that additionally features a transportation grant for experimenting with selling the *Jambar* in rural areas (the 'grant treatment'). The transportation grant was meant to generate experiences in visiting villages that in turn would lower information frictions about profitable business opportunities. We pre-specified this hypothesis in our pre-analysis plan. We analyse the reduced form effects of the two treatments on vendor's cookstove sales. The treatments could also generate second round effects, for example on competition or market power, that we abstract from explicitly. Measuring such impacts is beyond the scope of this study.

3. Data and empirical approach

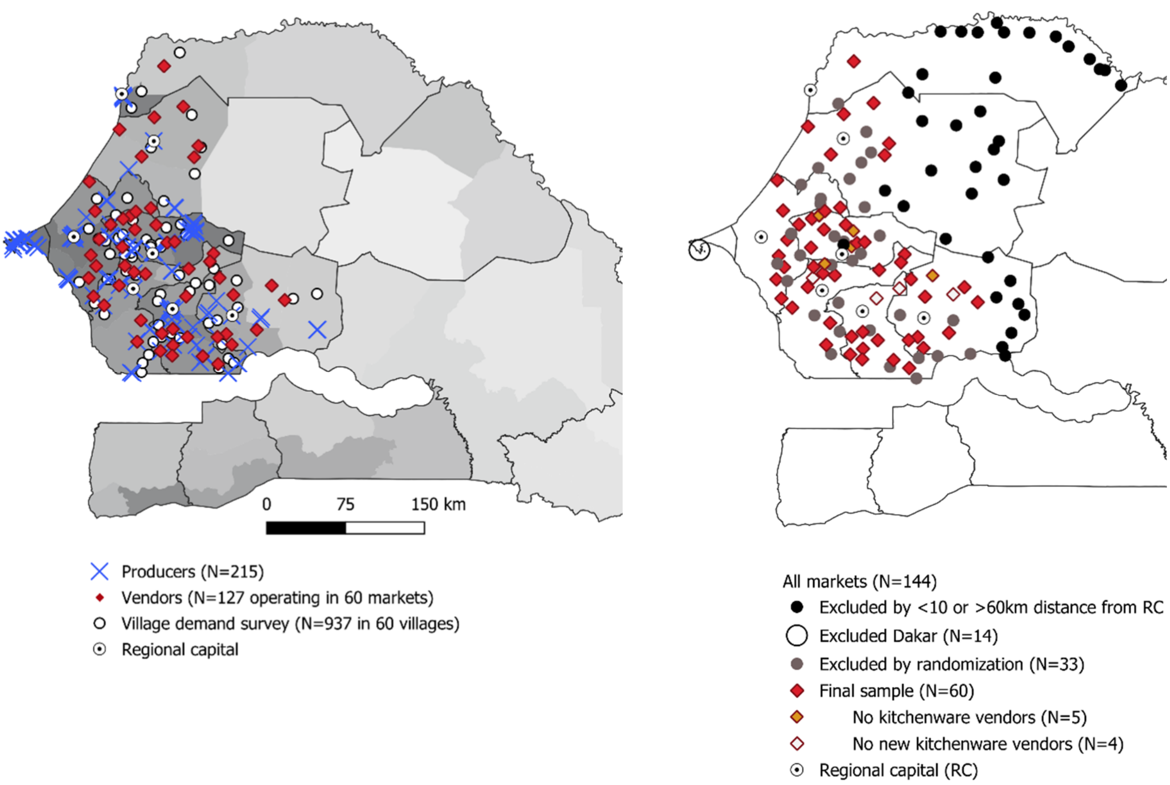
We combine multiple surveys and cover three key market actors in the eight (out of 14) most populous administrative regions of Senegal: producers, vendors, and consumers. Figure 2 – Panel A displays the survey locations for all study components and Figure 3 displays the timeline of the surveys and our randomized intervention. We carried out an in-person baseline survey with *Jambar* producers and rural market vendors in November 2017; this was followed

[9] See Appendix C1 for more details.

by the village demand survey in December 2017. Between February 2018 and November 2019, we tracked producers' and vendors' sales through phone surveys. In the middle of this detailed tracking period, in March 2019, we implemented the randomized intervention among vendors. We then carried out an in-person follow-up survey with vendors in December 2019, which was supplemented by two longer-term sales tracking phone calls carried out in 2021. Only the longer-term sales tracking had not been pre-specified, and we refer to the 2018/2019 sales tracking as the *regular tracking period* in what follows. We describe each of the components in detail next. Study materials and further details can be found in Appendix B.

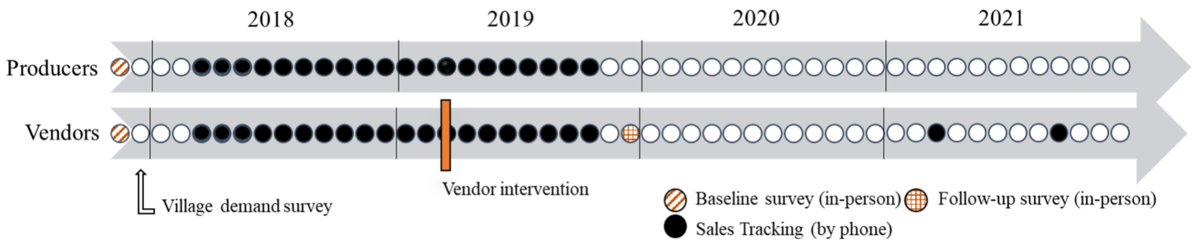
Figure 2: Survey locations and market sampling

A: Survey locations **B:** Market sampling



Source: Own presentation. Panel A: Color intensity describes population density ranging between 8.7 (lightest gray) to 22.6 (darkest gray) people per km^2. Population data from the Minnesota Population Center (Minnesota Population Center, 2020) based on 2013 census data by the National Agency of Statistics and Demography (ANSD). Areas calculated using QGIS3. Panel B: The radius within 10 and 60km from RCs was constructed using geodetic distance. The map shows 9 markets where either no or no new kitchenware vendors, who had not yet been interviewed on a priorly surveyed market, were present.

Figure 3: Study timeline



Note: Each circle represents one month.

3.1. Producer survey

We attempted to perform a full census of FASEN-trained *Jambar* producers, eventually surveying 215 out of the 228 producers who had reportedly received training. The remaining producers could not be located due to out-of-date contact information provided by FASEN. Producers manufacture and sell the *Jambar* in eight of Senegal's 14 regions (Figure 2 – Panel A). The producer surveys included in-person baseline interviews, and sales tracking via mobile phone conducted over 20 months. For the sales tracking, we asked producers to report sales of the firewood *Jambar* since the prior interview. The first interview referred to sales over the prior month. Out of 215 producers surveyed at baseline, only two producers never reported sales. Conditional on reporting, the mean producer participated in 9.1 sales tracking calls. Basic descriptive statistics from the producer surveys can be found in Appendix C – Table C. 3.

3.2. Vendor surveys

For establishing the vendor sample, we first identified a relevant sample of rural markets. We followed a multi-stage approach (Figure 2 – Panel B). First, we mapped all 144 markets located in the eight regions where FASEN-producers were operating.[10] Second, given our rural focus and to manage survey logistics, we excluded the highly urbanized Dakar region and all 37 markets located less than 10 km or more than 60 km from a regional capital. We thereby excluded markets primarily catering to urban demand and very remote areas where selling *Jambars* would be particularly difficult. This selection procedure identified areas to which firewood *Jambars* seemed most likely to diffuse. Finally, we randomly selected 60 markets out

[10] We received a list of 144 markets by the Senegalese government, which we verified via mobile phone calls with local authorities.

of the 93 markets satisfying these distance criteria, making sure to select at most one market in each *commune* (the Senegalese administrative division immediately above the village level). Note that apart from the 60 surveyed markets, the surveyed vendors operate in 15 additional markets that we did not survey (as these vendors move back and forth between multiple markets).

Next, we identified a total of 221 kitchenware vendors in the 60 sample markets. Not all of these vendors sold traditional stoves or EEBCs at baseline. We thus categorized them into three vendor types: (i) kitchenware only vendors, (ii) kitchenware and traditional-stove vendors, and (iii) kitchenware, traditional stove, and EEBC vendors.[11] Of these vendors, 73 percent consented to participate. We randomly excluded 29 vendors because, due to budget constraints, we could only interview a maximum of four vendors per market. The final sample consists of 127 vendors (Figure 4).[12] The most common reason for refusal was that interviews were scheduled during busy market days, when vendors time was at a premium. Accordingly, our sample may underrepresent particularly busy vendors.

Field teams selected vendors using a tablet-based randomization tool. If more than four vendors per market consented to participate in our study, we prioritized vendors as follows: we first chose all vendors who had already been selling EEBCs at baseline, then vendors who had been selling traditional stoves (but not EEBCs), and lastly those vendors who had only been selling kitchenware. Within each category, selection of who to enrol was random. This oversampling of stove-selling vendors ensured that the business priorities of sample vendors were broadly aligned with the intervention goal of marketing the firewood *Jambar*. Our final vendor sample thus consists of 127 vendors, representing 58 percent of all kitchenware vendors operating in the selected markets.

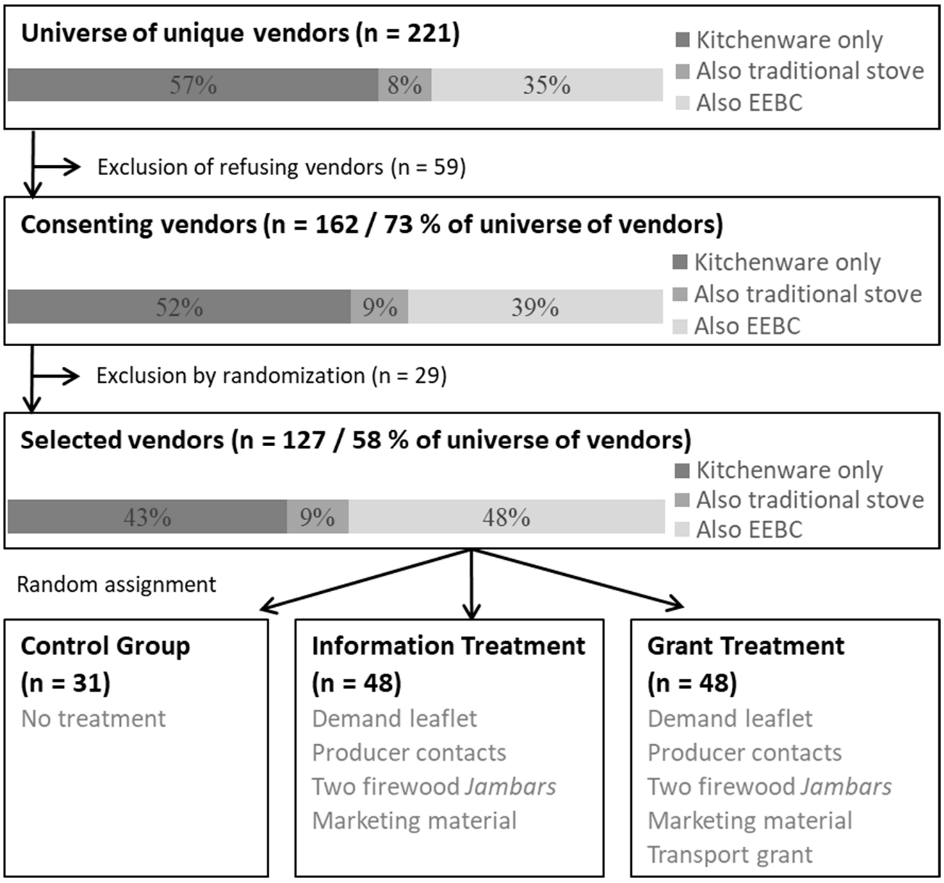
The vendor surveys also included in-person interviews and sales tracking via mobile phone. We implemented two in-depth in-person surveys, at baseline and 9 months after the intervention. These surveys covered a large range of business and vendor characteristics (including information on general business characteristics, markets of operation, product

[11] EEBC stoves sold at these markets include the *Jambar* and the *Sakkanal* stove. The latter is not as efficient as the *Jambar* stove, but still more efficient than traditional stoves. See Section 2.1 for an overview of stoves sold at these markets.

[12] Refusal is distributed over all three 'types' of vendors, but the share of refusers was highest among EEBC vendors (18 percent), followed by kitchenware vendors (seven percent), and lastly by traditional stove vendors (two percent).

range, turn-over, costs, and customers). The monthly sales tracking focused on vendor sales of all stove types over a period of 20 consecutive months, 12 month pre-intervention and seven months post-intervention (Figure 3). In April 2021 (i.e., 17 months after the regular tracking period) and again in October 2021, we implemented two additional rounds of sales tracking to understand longer-term impacts. These longer-term tracking surveys are not pre-specified. Participation in the monthly vendor tracking was high, but most vendors missed several months. Participation rates do not differ across experimental groups. We discuss attrition and corresponding robustness checks extensively in Appendix D and Section 4.3.

Figure 4: Vendor sampling and randomized intervention



Note: "Also EEBC" refers to vendors who had already been selling EEBC at baseline, "Also traditional stove" refers to vendors who had only been selling traditional stoves, "Kitchenware only" refers to kitchenware vendors who had not been selling any stoves at baseline.

3.3. *The village demand survey*

After selecting our study markets, we identified potential *Jambar* customers within the markets' catchment area. We randomly selected 60 villages from the census of all Senegalese villages located within 30 kms of one of our 60 study markets and having a population between 400 and 1600 people (in 2015). Approximately 15 households were randomly selected from household rosters within each village. With these households, we conducted sealed-bid, second-price ("Vickrey") auctions.[13] The total sample size for this demand study was 937 households. Field teams shared information about the benefits of the *Jambar* and conducted stove demonstrations before explaining the Vickrey auction rules. Enumerators highlighted how bidding one's maximum WTP for the *Jambar* was the optimal strategy as participants risked losing the chance to purchase the device if they were to underbid, and of overpaying if they were to overbid. We randomly varied interview length and the location where households cast their sealed bids (long interviews and casting the sealed bid at home vs. short interviews and casting the sealed bid during a public event, respectively).[14] The auction event, where results were revealed, was public for both groups. Once auction results were revealed, the highest bidder was invited to purchase the *Jambar* at a price equal to the second-highest bid.[15] If this individual refused the purchase offer, field teams moved on to the bidder who cast the next highest bid.[16] The auction ended with the acceptance of a purchase offer, at which point the auction winner made a lump-sum payment for the *Jambar* to field teams, and in turn received the stove.

3.4. Randomized intervention

We designed the study to test the impacts of two different treatment packages: an information and marketing materials treatment (the 'information treatment') and a second treatment that additionally features a grant (the 'grant treatment').[17] Both treatments included the following four information and marketing components: (1) a demand leaflet summarizing results from the village demand survey for specific villages, including contact details of a local village authority, in order to reduce search costs in identifying consumers, (2) a leaflet with *Jambar* producer contacts designed to lower costs associated with reaching out to, and negotiating with, multiple producers, (3) two sample firewood *Jambars* (medium sized) to reduce costs and

[13] Bidders in a Vickrey (1961) auction cast secret bids. The winner (the auction participant with the highest bid) pays a price equal to the second-highest bid. This characteristic of Vickrey auctions makes them incentive compatible; bidders are incentivized to bid (and thus reveal) their true value for the item being auctioned.

[14] We further assigned villages within the long-survey arm to one of two different types of auctions: those featuring only the firewood *Jambar* or those also featuring a second improved stove that is currently not available in Senegal (Jeuland et al., 2021).

[15] Each village could have only one winner. In case of multiple bidders casting the winning bid, a winner was selected by drawing lots.

[16] Although auction results were not binding in any legal sense, in practice only six percent of participants declined to follow through with their purchases (see Jeuland et al. (2021) for additional details).

[17] All intervention materials are displayed in Appendix B.

risks inherent to experimenting with (selling) new technologies, and (4) further marketing materials to attract customers and provide information on the benefits of the *Jambar*. The grant treatment additionally included (5) an unconditional cash grant of USD 31 labelled as a "transport grant," suggesting—but not requiring—that vendors use it for transporting *Jambars* to consumers in villages.[18]

Vendors were randomly assigned to one of the treatment groups (each N=48) or a control group (N=31). This random assignment was stratified based on (i) the number of markets the vendor sells in regularly at baseline, and (ii) whether the vendor belonged to a "high" or "low" revenue group (above and below median total monthly revenue, respectively) at baseline. Final lists of randomly assigned vendors were provided to field teams, who delivered the intervention during a one-time, one-on-one, in-person visit with vendors in markets and explained the purpose and deployment of each element.[19] The randomization was successful in producing three balanced groups with few meaningful differences between the groups. Balance of key vendor characteristics is provided in Appendix C2.

Our empirical specification for measuring the impacts of the intervention on vendor's monthly sales relies on a generalized difference-in-differences approach. We estimate the following specifications to estimate the impact of receiving any treatment (equation 1a), and of receiving the information or the grant treatment (equation 1b):

$$Y_{i,m} = \beta_1(Treatment_i) + \beta_2(Treatment_i \times post_m) + \gamma_m + \epsilon_{i,m} \quad (1a)$$

$$Y_{i,m} = \beta_1(info_i) + \beta_2(grant_i) + \beta_3(info_i \times post_m) + \beta_4(grant_i \times post_m) + \gamma_m + \epsilon_{i,m} \quad (1b)$$

where $Y_{i,m}$ represents reported monthly stove sales by vendor i in month–year m; $Treatment_i$ is a binary variable that equals one if vendor i was assigned to *either* of the two treatment groups; $post_m$ is a binary variable that equals one for all month–years in the post-treatment period; $info_i$ and $grant_i$ are binary variables that equal one if vendor i is assigned to the

[18] Our field team conveyed that the grant is labelled for transportation purposes as following: "We would like to support your ability to go to these villages. We know that often vendors do not have the financial resources to take their products to villages. We would therefore like to give you XOF 18,000 for arranging transport, which you can use to bring improved stoves to near-by villages and present it to households. If you are successful, you can use your revenues and go to even more villages."

[19] We purposefully decided not to randomize at the market level due to the small sample size and since vendors are highly mobile and visit several markets. This means we cannot rule out spillovers treated to control vendors and even observe some indication that spillovers effectively happened. This means we possibly underestimate treatment effects. For more discussion, see Appendix D2.

information or the grant treatment group, respectively; γ_m represents a month–year fixed effect; and ϵ_i represents a vendor-specific error term. We cluster standard errors at the vendor level.

4. Results of demand estimation and randomized vendor intervention

4.1. Demand estimation

We collected demand information in order to include it in the pre-specified randomized information package. Exploring these data further shows that a significant portion of the population is interested in firewood *Jambars*. The mean WTP for the *Jambar* is USD 5.6, with around 10 percent of respondents willing to pay the average rural market retail price of USD 13.6 for the medium-sized firewood *Jambar* that our vendors reported at baseline (Figure 5). These results corroborate the findings from an earlier study on households' WTP in 18 villages in central Senegal, which found even higher WTP levels (Bensch & Peters, 2020).

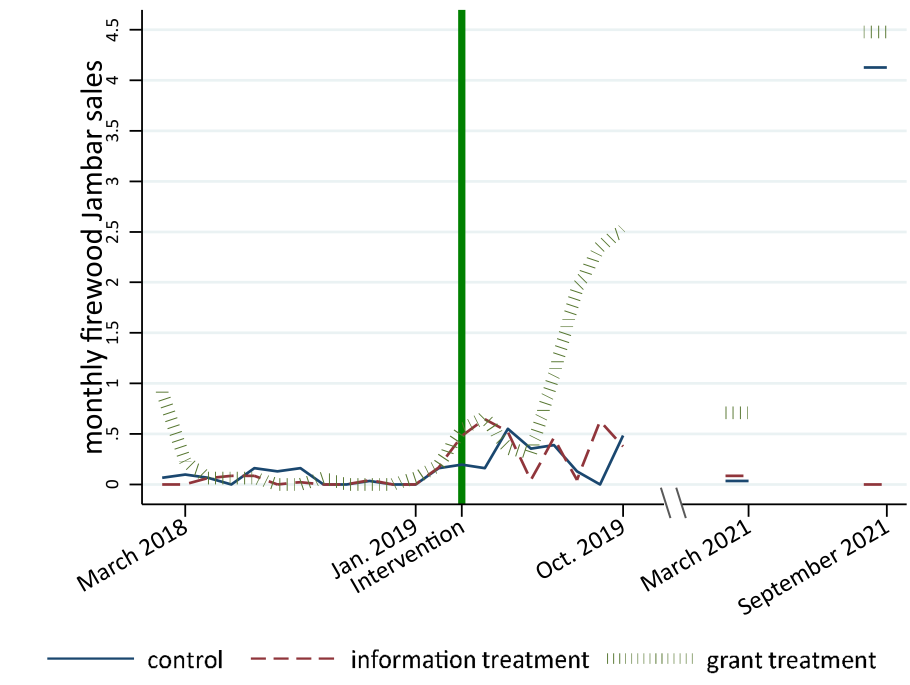
Figure 5: Consumer demand for Firewood *Jambars* in rural Senegal

Note: The demand curve is based on bids by households in sealed-bid, second-price ("Vickrey") auctions with 937 randomly selected households in 60 villages. WTP for a medium-sized (7kg) firewood Jambar.

4.2. Results of randomized vendor intervention

Treatment uptake was generally high, with around 80 percent of vendors in both the grant and the information groups using at least one of the elements included in our intervention. Overall, the distributed materials and information led to significantly higher marketing and outreach activities among treated vendors. The share of vendors who engaged in any marketing activity increased by roughly 30 to 40 percentage points, from a pre-intervention mean of 43 percent (details in Appendix C4). As pre-specified as our secondary outcome, we examine the use of specific intervention components: The highest uptake was for marketing materials such as shirts and posters, ranging from 38 to 60 percent across experimental groups and elements (see Appendix C4). Only 30 percent of vendors receiving the grant treatment reportedly used the money for transporting stoves. Only three vendors effectively visited villages to offer stoves, as we had suggested when vendors received the grant. However, 57 percent of the grants that were not used to transport stoves were reported to have been diverted to cover other business costs, such as purchasing additional demonstration stoves.[20] Interestingly, 19 percent of control vendors also reported using at least one of the elements, suggesting the presence of cross-vendor spillovers.[21] Impact estimates that follow may hence be interpreted as lower bounds.

Figure 6: Vendors' average monthly firewood *Jambar* sales over time, by experimental group



Note: The green line indicates the timing of the intervention (March 2019). Attrited vendors in the March and September 2021 sales tracking are set zero. Results are qualitatively the same if we replace attrited vendors with the attriting vendors' all-time maximum monthly sales over all prior months. The latter approach is conservative, as it maximizes sales of more control vendors than treatment vendors, given that more control vendors attrited.

[20] The second most important use of grants not used for transporting stoves are other private needs (40 percent).

[21] Most used elements by control vendors are marketing materials. Interestingly, some also report having used the transport grant, potentially by participating in joint sourcing of stoves.

Figure 6 presents average monthly firewood *Jambar* sales over time, our primary outcome. Prior to the intervention, reported monthly sales were flat and close to zero. Following the intervention, we observe a sharp increase in reported sales among both information and grant vendors. Grant vendors' sales continued to grow throughout our regular tracking period, which ended seven months after treatment.[22] In contrast, sales in the information group decreased quickly and leveled off slightly above baseline values. The spike in sales in the information and marketing group can largely be explained by sales of the freely provided demo stoves.

Exploring our longer-term measurements in March 2021 show that two years after the intervention, grant treatment vendors' sales remained higher than those of information treatment and control vendors. Six months later, in September 2021, control vendors had caught up with grant vendors, however, while information and marketing vendors hardly sold any firewood *Jambars*.[23] The two longer-term measurements in individual months (unlike the continuous tracking during the regular tracking period) must be interpreted with care, as they may be affected by seasonal or idiosyncratic short-term fluctuation. For example, the increase in sales of the control group in September 2021 is nearly entirely driven by one vendor who sold a very large quantity of firewood *Jambars* in that month. Qualitative follow-up interviews with control vendors who started to sell firewood *Jambars* revealed that nearly all had become aware of our intervention, and most believed it had raised interest in the firewood *Jambar* among their customers. It is thus possible that spillovers are responsible for the overall upward trend in firewood *Jambar* sales. The finding of sustained sales until 2021 is remarkable, especially given the disruption by COVID-19 policies.[24]

To quantify the impacts of our intervention on vendors' average monthly sales of the firewood *Jambar*, we estimate a generalized difference-in-differences model (see Section 3.4). The focus

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[22] We corrobate these graphical findings in an event study specification. See Appendix C6 for details.

[23] This longer term analysis was not pre-specified as it only became possible through additional funding secured at a later stage.

[24] Between March 27 and June 30, 2020, the Senegalese government proclaimed a health emergency and took measures to curb the spread of COVID-19. Measures included a reduction of working days in fixed markets and bans on traveling between departments, on rural markets, and on public gatherings and festivities. Weaker measures (night curfews) were re-introduced between December 2020 and March 2021 in the regions Dakar and Thiès. When asked in March 2021, 73 percent of vendors indeed reported a negative impact of the pandemic (and pandemic-related measures) on stove sales and sales in general. Another 20 percent reported not knowing the direction of this effect. In response to open-ended questions, five vendors reported that they had abandoned the stove business.

is on the regular tracking period, given the higher reliability of the sales figures during this period and the specification in our PAP. Consistent with the graphical evidence provided in Figure 6, we find that after the end of the regular tracking period in March 2020, grant vendors were selling approximately 0.9 more firewood *Jambars* per post-intervention month relative to the full sample pre-intervention average of 0.08 sales per month (see Table 1). This corresponds to a total of 43 additional *Jambars* sold per month across all grant vendors, a statistically significant increase of over 1000 percent. In contrast, reported sales by the information vendors are statistically indistinguishable from those reported by control vendors. The test for no difference in the two treatment coefficients is insignificant (p-value 0.15). [25]

In Appendix C7 we also show vendor's market-level sales of other stoves. Our intervention appears to have generated modest co-benefits via increased sales of another EEBC variant (*Sakkanal* stove) which is slightly less efficient than the *Jambar* stove. This is plausible as sales of the two EEBC may be co-integrated within vendors' business models, with similar producer, consumer, transport, and pricing structures. Sales of other stoves have not been affected.

Table 1: Impact on vendors' monthly firewood *Jambar* sales (regular tracking period)

	(1)	(2)
Any treatment X Post	0.51 (0.102)	
Information X Post		0.15 (0.625)
Grant X Post		0.88* (0.0675)
Observations	2667	2667
Same		0.15
Joint		0.100
Pre-intervention mean	0.078	0.078
Adjusted R-sq.	0.013	0.019
Year-month FEs	Yes	Yes
Vendor FEs	No	No

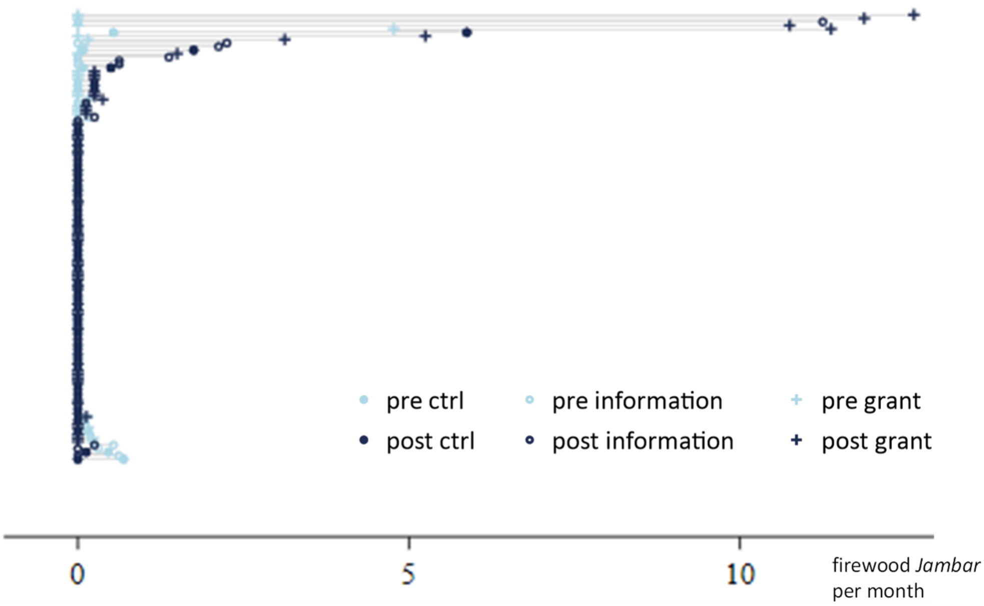
*Note: P-values in paretheses. Standard errors clustered at the vendor level. * p < 0.1, ** p < 0.05, *** p < 0.01. The "Joint" row reports the p-value for the test for joint significance of the two treatment coefficients. The "Same" row reports the p-value for testing the hypotheses that there is no difference in the treatment coefficients.*

[25] If we pool the two treatment arms, the effect is still positive and borderline in-significant (p-value 0.102). The longer-term results are also presented in Appendix C5. In line with the graphical evidence, the effects stay positive but turn insignificant when adding the September 2021 sales.

4.3. *Impact heterogeneity and robustness of results*

Figure 7 explores each vendor's average monthly firewood *Jambar* sales before the intervention against sales after the intervention during the regular tracking period.[26] The figure reveals that most vendors did not sell firewood *Jambars* during the study period. A total of 84 sample vendors, i.e., 66 percent of the sample, never sold any *Jambar*, either before or after the intervention. The increase in post-treatment sales was driven by around 30 vendors and, in particular, by a small number of "super-sellers" who sold more than five *Jambars* per month on average (five in the grant group, and one each in the information and control group).[27]

Figure 7: Vendors' average monthly firewood *Jambar* sales, pre- and post-treatment



Note: This graph plots average monthly sales pre- and post-treatment for each individual vendor during the regular tracking period. Light blue markers represent average sales pre-treatment and dark blue markers post-treatment average sales. Filled dots represent control group vendors, circles represent information treatment vendors, and crosses represent grant treatment vendors.

We furthermore investigate whether effects on firewood *Jambar* sales differ across different vendor baseline characteristics as pre-specified in our Pre-Analysis Plan: (i) large or small business (based on median monthly revenue as well as the median number of products sold), (ii) access to banking services, and (iii) engagement in marketing activities. We see that effects

[26] Note that this descriptive analysis was not pre-specified.

[27] Among the super-sellers, all three types of vendors are represented. Three super-sellers had already been selling EEBC at baseline, 2 had been selling other traditional stoves, and 2 were kitchenware only vendors. In Appendix C5 we also present regression results with an alternative outcome indicator. Instead of the number of stoves sold, we use a dummy indicating whether a vendor sold any stove before and after the intervention. Results are similar, showing that the grant treatment increases the share of vendors selling any stoves, while the information treatment has no significant effect.

are generally driven by smaller businesses and businesses who did not perform marketing at baseline. Tests for equality of the treatment effects however are not significant (see Table 2).[28]

Table 2: Heterogeneous impact of the intervention on vendors' monthly firewood *Jambar* sales

	(1) Above median revenue	(2) Above median number of products	(3) Marketing active	(4) Bank account
Any treatment X NO	1.13*	0.91*	0.89*	0.62*
	(0.021)	(0.044)	(0.072)	(0.047)
Any treatment X YES	0.32	0.52	0.22	0.89*
	(0.210)	(0.120)	(0.486)	(0.058)
Treatment effects are equal (p-value)	0.134	0.482	0.207	0.623
Information X NO	0.63	0.64	0.34	0.63
	(0.189)	(0.220)	(0.444)	(0.152)
Information X YES	-0.014	-0.0024	-0.098	-0.022
	(0.889)	(0.981)	(0.653)	(0.711)
Treatment effects are equal (p-value)	0.176	0.215	0.296	0.136
Grant X NO	1.66*	1.19	1.58*	0.61
	(0.051)	(0.102)	(0.078)	(0.162)
Grant X YES	0.64	1.05	0.47	1.85**
	(0.172)	(0.100)	(0.332)	(0.043)
Treatment effects are equal (p-value)	0.292	0.887	0.259	0.218
Observations	2667	2667	2667	2667
Year-month FEs	Yes	Yes	Yes	Yes

*Note: Marketing active = 1 if vendor reported to perform any marketing activity at baseline. P-values in parentheses. Standard errors clustered at the vendor level. * p < 0.1, ** p < 0.05, *** p < 0.01. Adjustment for multiple hypothesis testing renders all coefficients insignificant. Results including vendor FEs are not displayed but are qualitatively the same. Available upon request.*

We demonstrate the robustness of our results with regards to (i) the empirical specification (inclusion of vendor fixed-effects, sequential inclusion of control variables, ANCOVA analysis) and (ii) the impact indicator (using vendors' sales in a typical month as reported during the in-person endline survey) in Appendix D2. We also check sensitivity of the results to outliers and attrition in reporting (non-response at endline and non-response during monthly sales reporting) (also Appendix D2). Our results are robust to a large set of sensitivity analyses. Winsorizing or trimming outliers changes our results, however, which is not

[28] In Appendix C5, we also explore regional heterogeneity which suggest that our intervention was particularly successful in one region. Due to the small sample size, we cannot formally test this.

surprising since the effect is driven by outliers, as discussed above. The fact that our positive treatment effect for the grant treatment is driven by very few "super-sellers" raises suspicion that it might be driven by a coincidental grouping of high-performers in the grant group. In Appendix D2 – Figure D. 2, we show with an approach inspired by randomization-based inferential procedures (Athey & Imbens, 2017) that this is unlikely. One caveat in interpreting the results is our somewhat selective sample, which may underrepresent particularly busy vendors. Those vendors who did not agree to participate because they did not have the time for interviews, may have responded differently to our treatment. The direction of bias is unclear. They may have sold more *Jambar* due to economies of scale or greater entrepreneurial capacity. However, they may also have sold fewer *Jambar*, as we observe that our intervention particularly incentivized rather low-performing vendors to enter the *Jambar* business. Furthermore, we believe that our sample represents a realistic target population of a real-world policy intervention, which is likely to face a similar selection into the intervention.

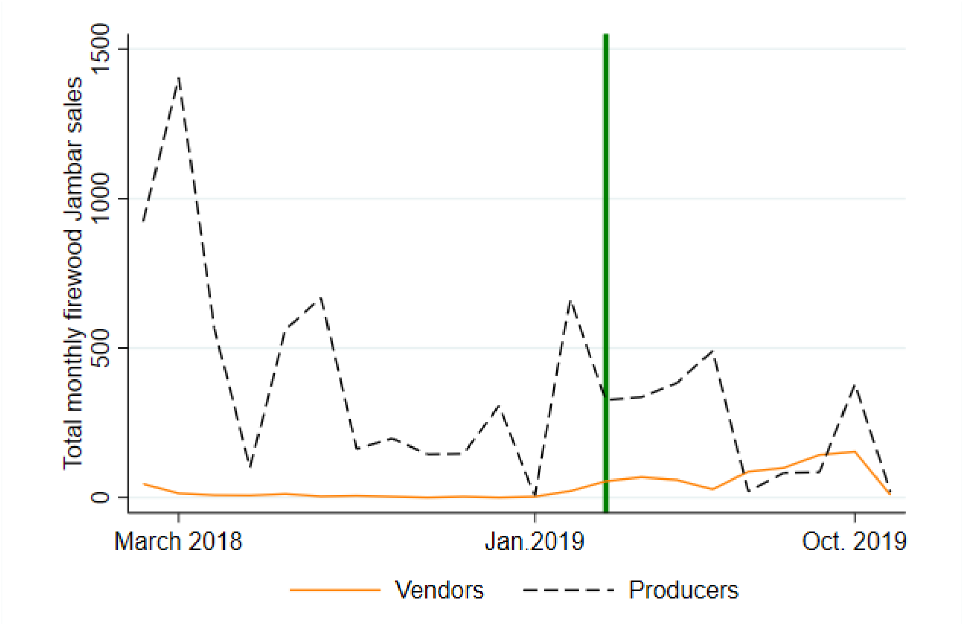
4.4. Economic significance of observed impacts

Is the statistically significant increase of 43 additional *Jambars* sold per month across all grant vendors economically significant? Figure 8 compares the total firewood *Jambar* sales of surveyed vendors to the total firewood *Jambar* market by plotting them against producer sales over time. The analysis of producer sales was pre-specified as our tertiary outcome. There are large fluctuations in producers' sales, and a clear downward trend. Pre-intervention, all producers together reported selling on average 450 firewood *Jambars* per month. Post-intervention reported sales only amounted to 235 firewood *Jambars* per month. We see no evidence that the start of the vendor-level intervention had a positive impact on production. The effect size of 43 additional *Jambars* corresponds to 18 percent of all producer sales post-intervention. This is a non-trivial amount, but it is small in comparison to the general downward trend in producer sales.[29] *Jambar* sales numbers are also low when compared to

[29] We might underestimate the size of the overall *Jambar* market, as other producers produce and market a counterfeit version of the *Jambar*, in addition to the FASEN-trained producers. Most vendors who sell the *Jambar* confirm having seen such counterfeits that are distinguishable because they do not feature an official FASEN sticker. In the vendor sales tracking, we both counted original and conterfeit *Jambars*, so the existence of counterfeit stoves does not challenge our findings regarding the low *Jambar* sales volumes among intervention vendors. Some of the additional demand may have been served by producers outside of our FASEN-producer sample, though. We are able to match only 44 percent of the producers that vendors report purchasing from to our FASEN-producer sample, suggestive of a substantial counterfeit *Jambar* market. Yet, the matching may be noisy as vendors may refer to producers by different names or to names of their co-workers they engage with.

charcoal *Jambar* sales. At baseline, producers sold more than four times as many charcoal *Jambars* as firewood *Jambars*.

Figure 8: Firewood *Jambar* market volume, by market actor



Note: The green line indicates the timing of the intervention. "Vendors" refer to all vendors in our sample, "Producers" refers to the universe of FASEN-trained producers.

Considering potential demand for firewood cookstoves in rural areas, vendors' sales also appear low. According to the 2023 census, the seven regions where our producers operate (excluding Dakar) host approximately 1 million households (ANSD, 2023), of which 54 percent use firewood as their main cooking fuel (ANSD & ICF, 2020). These more than 0.5 million households constitute the potential consumer group, of which according to our demand analysis, around 10 percent (i.e., around 50,000 households) are willing to pay the rural market price.[30] This highlights that, despite a statistically significant effect in our sample, a small share of the market is currently served by rural market vendors.

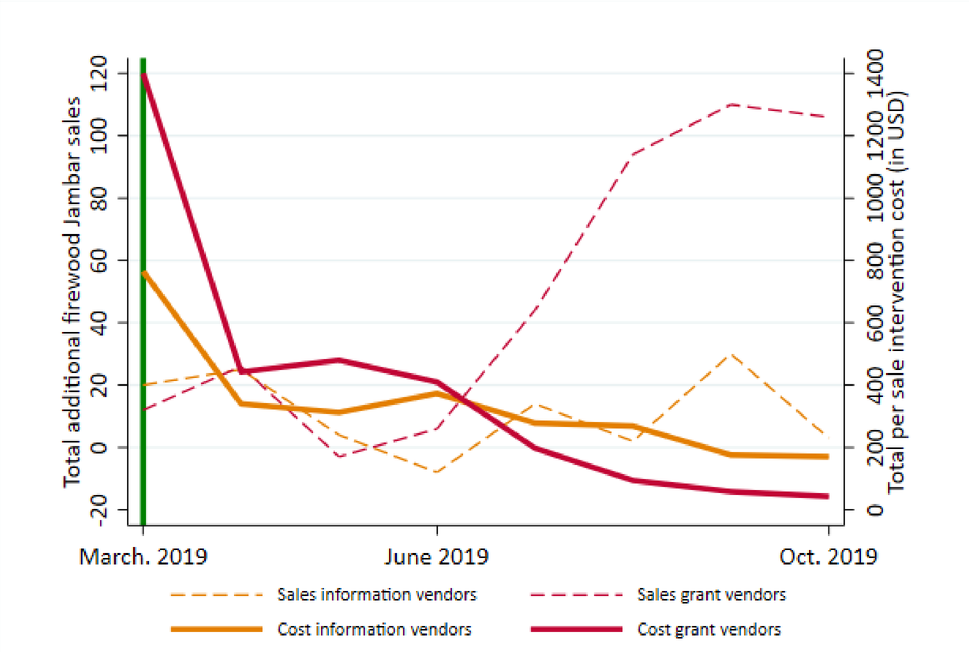
To assess the impacts from a cost-effectiveness perspective, we provide back-of-the-envelope calculations of the per *Jambar* diffusion cost. The total cost of delivering our intervention is roughly USD 319 per information treatment vendor and USD 350 per grant treatment vendor. The cost excludes monthly sales tracking cost, which was done for evaluation purposes only.[31] Figure 9 spreads the total intervention cost over the additional firewood *Jambar* sales triggered

[30] On average, Jambar firewood stoves have to be replaced every two years.

[31] This calculation hence implicitly assumes that the regular sales tracking did not impact vendors' sales. The cost includes, among others, costs for materials, sample stoves, and transport grants, a permanent junior staff, field personnel and logistical costs to identify vendors and deliver the intervention, and implementation of auctions in villages.

by our intervention. Specifically, the solid lines show the cost per *Jambar* disseminated over time for the information and grant group separately. We calculate these additional sales conservatively.[32]

Figure 9: Intervention cost per additional firewood Jambar sale, by experimental group



Note: The right axis and solid lines display the total intervention cost per firewood Jambar sold; the left axis and dashed lines display Jambar sales. The figure spreads the intervention cost over the additional firewood Jambars sold over time.

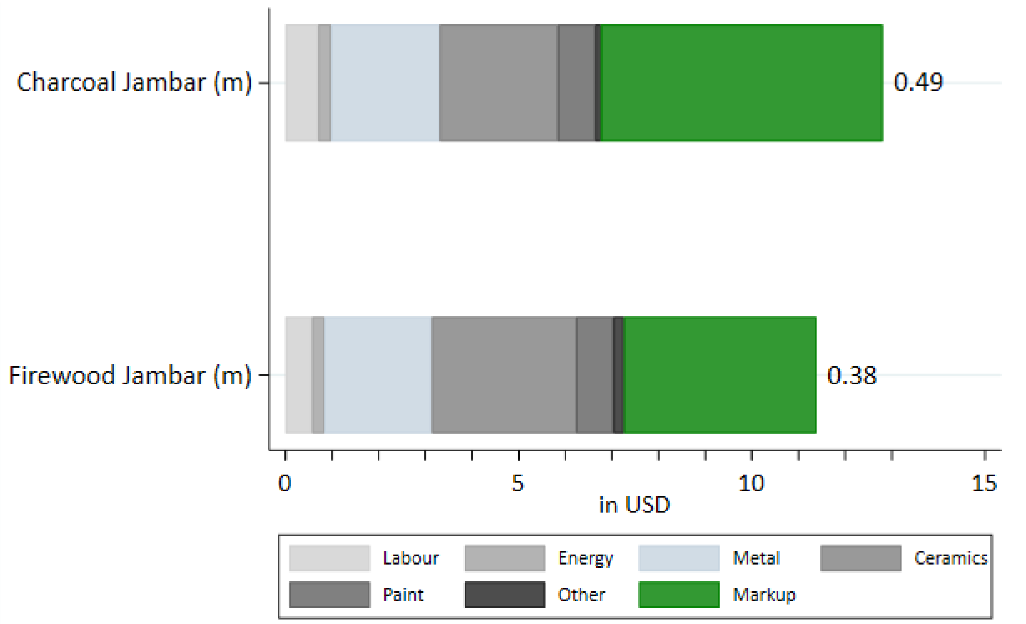
In the eight months from the intervention (March 2019) to the end of the main monitoring (October 2019), the total intervention cost per firewood *Jambar* diffused decreases from USD 765 to 170 for information treatment vendors and from USD 1400 to 43 for grant treatment vendors (Figure 9). While the dissemination cost via the information intervention seems out of proportion given the much lower price of the *Jambar* itself, the cost of dissemination via the grant treatment may prove competitive with alternative dissemination policies. Note that a large share of the cost is associated with the village demand survey, a treatment element that may or may not be necessary for the observed impacts to occur.

[32] To quantify additional firewood *Jambars* sold, we calculate the difference-in-differences in *Jambars* sold, i.e., we take the difference in firewood *Jambar* sales between each experimental group and the control group in each post-intervention month and subtract the difference in sales from the same month in the previous (pre-intervention) year.

5. Markups along the supply chain and profitability of *Jambar* firewood sales

In this section, we explore potential reasons why vendors and producers do not engage more substantively in this market. We examine the cost structure for *Jambar* producers and vendors and identify markups along the supply chain. For producers and vendors, we use data from the in-person baseline surveys. Sample sizes are small (ranging from four to 88 respondents per stove type), so point estimates must be interpreted with caution.

Figure 10: Stove-specific markup in retail prices (in USD at baseline), reported by producers



Note: Markup as share of retail price displayed next to each bar. m=medium sized Jambar (7 kg). Markups abstracts from some fixed and variable costs that are difficult to express in per-stove terms. The full bar shows producers' average retail prices. This data has substantial missing values, as producers could chose the type (charcoal or firewood) of Jambar to report on, and many producers abstained from reporting their stove retail prices. The calculations hence rely on a small number of observations, namely 10 for the firewood Jambar and 83 for the charcoal Jambar.

5.1. *Producers*

Producers spend on average between USD 7 and 8 per *Jambar* on labour, energy, metal, ceramics, paint, and other smaller inputs (such as cement, iron, and petrol; see Figure 10). Production costs are similar for the charcoal and firewood *Jambar*.[33] However, looking at the retail price, the charcoal variant is sold at substantially higher prices than the firewood variant and, hence, producers' markups are higher for the charcoal than for the firewood *Jambar*. This markup does not necessarily correspond to the producers' profit as some fixed costs (such as

[33] In a direct question, the majority of vendors state that the firewood *Jambar* is more expensive to produce than the charcoal variant, even though 37 percent state the costs are the same or even higher for the charcoal variant (5 percent).

rent, marketing expenses, and transportation costs) are not considered. The lower retail prices for firewood *Jambars* probably reflect the higher price elasticity of rural household demand, since producers follow retail price recommendations by FASEN, the national cookstove program. FASEN, in turn, is aware of the lower purchasing power of rural households.

5.2. Vendors

In comparison to producers, vendor markups appear to be even lower. Vendors reported the buying price they pay to producers or wholesalers per stove, the transport costs they bear for bringing a stove to their market or warehouse, and the per-unit retail price they charge for the stove in rural markets.[34] Using these three figures, we calculate vendors' markup. As with producers, this markup does not correspond to vendors' profit since some variable and fixed costs involved in marketing a stove are not considered, given difficulties associated with expressing them in per-stove terms.[35]

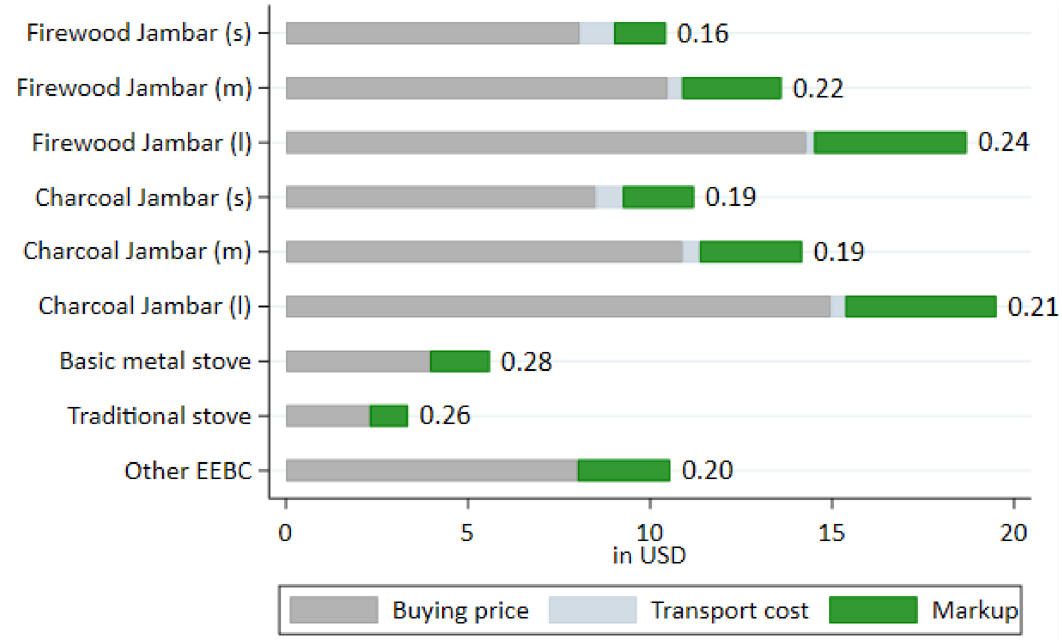
Vendors appear to set firewood *Jambar* prices such that they realize modest markups ranging from 16 to 24 percent, depending on the stove size and type.[36] These markup levels are comparable to charcoal *Jambar* markups, but slightly lower than markups for traditional or more basic stoves. Combined with possible risks associated with *Jambar* retail (e.g., *Jambars* are more fragile and can break during rough transport, retail requires higher up-front investment, demand patterns differ) these figures may explain why relatively few vendors find it worthwhile to engage in *Jambar* sales. For those vendors who do sell *Jambars*, it may explain why they do not focus more on selling *Jambars*, by for example doing village visits to sell higher quantities, but rather diversify their portfolio by selling other stoves and products.

[34] 33 percent of vendors get some stoves directly delivered by producers or wholesalers to rural markets or their storage house and pay a wholesale price that includes transport costs and stove costs. In these cases, transports costs are set to zero.

[35] For the vendor business as a whole, such other costs include on average 15 USD weekly for personnel, 5 USD weekly for market taxes and storage, 19 USD for other transport costs, expenses for water, food, electricity and security and of course other costs of sourcing adressed by our intervention.

[36] Buying prices reported by vendors are lower than selling prices reported by producers – possibly due to bulk purchases.

Figure 11: Stove-specific markup in vendor's retail price (in USD at baseline), reported by vendors



Note: Markup as share of retail price displayed next to each bar. s=small, m=medium, l=large. Markup equals retail price minus buying price and transport costs from the producer's or wholesaler's location to the vendor's rural market or warehouse. For 33 percent of vendors, the transport costs is included in the buying price. This data has substantial missing values, as many stove types are not sold at baseline or as vendors are not willing or able to provide cost calculations. We abstain from imputing these values. The calculations hence rely on a small number of observations ranging between eight and 44 per stove type.

To offset these challenges, vendors would have to increase the *Jambar* sales price. Our village demand survey shows, however, that this could be difficult (see Figure 5): *Jambar* demand is price-elastic around the current rural market price and increasing this price would greatly reduce households' demand. For example, if vendors increased stove prices for a medium-sized firewood *Jambar* from currently USD 13.6 to 16.6, thereby doubling their markup by 100%, they would lose 23% of their customers; a 150% increase, meanwhile, would reduce demand by more than 70%. It is important to note that we elicited demand in the villages – travel effort for rural customers to visit the weekly markets is not accounted for in our demand estimation. Conversely, vendor's markup does not account for costs of travelling to villages to offer the *Jambar*, which means that the markup for selling in the villages is effectively even lower. For our intervention, we estimated the cost of organizing transport for sending a single *Jambar* to a village to be around USD 2.70 per stove. Comparing this to the markup of around USD 3 on a medium-sized *Jambar* stove shows that these transport costs alone take almost all the markup from the vendors.

Figure 11 also highlights the differences in relative stove prices that consumers face. A medium-sized firewood *Jambar* costs on average USD 13.6 compared to USD 5.6 for a basic metal stove and USD 3.4 for a very simple, traditional stove. The simplest, three stone stove is typically not purchased at all. For low-income households, such differences in prices are highly salient.

Our village demand survey also illustrates further risks of *Jambar* sales: WTP varies substantially across villages, with the share of households willing to pay at least the market price varying between 0 to 47 percent. Depending on the region, the average WTP amounts to between USD 5–8. This is substantially lower than earlier estimates by Bensch & Peters (2020), who found an average WTP for the same stove of approximately USD 11 in 18 rural Senegalese villages. Differences in methodologies employed to measure WTP can partly explain the lower WTP we observe, underpinning the sensitivity and variability of demand.[37] Moreover, demand for *Jambars* might also fluctuate from year to year, due to weather patterns and agricultural yields. Thus, the challenge from the vendor perspective is to identify customers with sufficient WTP and to cope with seasonal and annual harvest-related demand fluctuations.

6. Conclusion

It is apparent from our work that some vendors are eager to market *Jambars* in rural areas, but that numerous factors impede widespread penetration. One possible interpretation is that our intervention was not intensive enough, and that a more intense training for vendors, higher or repeatedly offered transportation grants and other incentives, and improved rural demand information could increase sales numbers further. A better targeting to reach particularly busy vendors that are potentially underrepresented in our sample may also increase *Jambar* penetration. While we cannot rule out this explanation, the implementation of such an improved intervention and particularly better targeting would be challenging, while our intervention mimics a realistic real-world policy. We furthermore postulate another interpretation: information frictions and liquidity constraints are only one barrier preventing

[37] Both our survey as well as the Bensch and Peters (2020) survey were carried out immediately after the main harvest period in November and accordingly measure WTP in a high liquidity season. The main difference between our approach and that used by Bensch and Peters (2020) was that we relied on second-price Vickrey auctions (as opposed to the Becker-DeGroot-Marschak method) to elicit WTP. We also experimentally varied (i) the setting where auction participants cast their sealed bids for the *Jambar* (privately at the household, analogous to the door-to-door elicitation carried out by Bensch and Peters (2020), or at a public place in the village during a community event), and (ii) whether the *Jambar* was auctioned alone or along with another improved stove (see Jeuland et al. 2021 for additional details). Half of the Bensch and Peters (2020) sample had received a free stove six years before WTP elicitation. This group reveals a WTP that is 14-25 percent higher than households in villages without earlier stove distribution.

the rural Jambar market from developing, alongside further, potentially more decisive barriers.

Exploring producers and vendors markups highlights that uncertainties and hence risks are currently too high for most vendors to bear, given low potential profit margins. Markups for firewood *Jambars* get increasingly smaller along the supply chain when moving from urban to rural areas and are lower than for competing products. At the same time, demand for these *Jambars* exists, but varies substantially over villages, regions, seasons, and potentially years. Identifying profitable customer groups requires considerable effort, further lowering markups. Our village demand survey shows that demand is price elastic and retail prices can hardly be increased without losing the already small share of customers who are willing to pay the current *Jambar* rural market price. Our contribution to the academic literature thus constitutes an important piece of evidence to understand the difficulty of fostering and expanding rural markets, in which risks are high and information is costly.

On the supply side, incentives to engage in the *Jambar* market need to be improved, which might imply higher *Jambar* retail prices. Accordingly, continued, and complementary use of demand-side instruments to facilitate widespread adoption and uptake of these technologies are necessary. Financing schemes and grants, for instance, could increase affordability of EEBC as demonstrated in other contexts (Beltramo et al., 2015; Berkouwer & Dean, 2022; Levine et al., 2018; Munyehirwe et al., 2022), particularly if retail prices increase. A results-based financing mechanism, where subsidies are disbursed to supply side actors once a pre-agreed set of results has been achieved, may also serve to incentivize these actors to engage in the EEBC market (Particip, 2017). However, our research suggests that, for these actors to remain in the market, one-off subsidies are insufficient; rather, they require sustained support over a longer period.

Climate finance is the obvious way to fund subsidies, for example from voluntary carbon markets. EEBC can play an important role in reducing deforestation and promoting EEBC is therefore a cost-effective climate change mitigation measure (Bensch et al., 2021). Senegal has recognized the potential for emission reductions in the cookstove sector and has explicitly identified EEBC promotion in its National Determined Contributions. While some open questions remain on how to accurately measure and monitor the carbon emission reduction potential of EEBCs (Gill-Wiehl et al., 2024), carbon finance is a viable option to provide the necessary funding for potential financing and grant schemes.

7. References

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8. Appendix

Appendix A: Deviations from Pre-Analysis Plan

Table A. 1: Deviations from Pre-Analysis Plan

Section 4.1 and Section 5.2: We did not specify that we would analyse the WTP data in detail and only intended to use the demand information as part of the information treatment in the randomized intervention among vendors. When presenting and discussing our findings, we realized a more thorough presentation of the demand data was necessary to document that a) demand exists, but b) it is heterogeneous and volatile.
Section 4.2: We announced that we would carry out monthly mobile phone data collection until June 2019. Thanks to additional funding, we prolonged monthly data collection until October 2019 and conducted two additional rounds of data collection to measure longer-term impacts in 2021. Also in-person endline data collection shifted from June 2019 to November 2019. This also implies that all analyses of longer-term impacts were not announced in the PAP; yet they follow the specifications announced in the PAP.
Section 4.2: In our PAP we defined the following main outcomes: Vendors' monthly stove sales (primary outcome), vendors' market outreach activities (secondary outcomes), and producer's monthly stove sales (tertiary outcome). The primary outcome is discussed in detail in Section 4.2. and 4.3 and in line with the PAP. The secondary outcomes are discussed in Section 4.2. as treatment uptake. The discussion is somewhat less prominent than announced in the PAP, but all pre-specified analyses are provided in Section 4.2 and Appendix C4. The tertiary outcome is discussed in Section 4.4.
Section 4.3: We analyse the heterogeneity of impacts across individual vendors and regions. These analyses were not announced in the PAP but later turned out to be important for assessing our intervention's impacts transparently. We label these analyses as exploratory. For those heterogeneity analyses pre-specified, we do not apply the multiple hypothesis corrections announced in the PAP since these methods are not properly able to account for clustered standard errors, which are decisive in our empirical specification. Instead, we calculate sharpened false discovery rate adjusted q-values (Anderson, 2008; Benjamini & Hochberg, 1995).
Section 4.3 and Appendix D: We test for the robustness of our results towards heterogenous regularity in reporting even though it is not announced in the PAP. The obvious need for this analysis became apparent only after seeing very heterogenous response rates.

Also, the ANCOVA and randomization inference approaches were not pre-specified but are justified by the structure of the data.

We announced that we would impute missing dependent variables (monthly firewood *Jambar* sales), setting them equal to the mean of the respective outcome variable for the relevant treatment group. This is obsolete given the nature of our data collection, which asks for sales since the last call and thereby retrieves all sales in the analysis of sales in the regular tracking period.
For the analysis of sales in the longer-term period (that we did not pre-specify), we test two strategies in addition to the one announced in the PAP: 1) replacing missing dependent variables by their all-time maximum sales and 2) replacing them with zero. This is more conservative relative to the announced imputation method since it tests the extreme bounds.

Section 4.4: We see no evidence that the start of the vendor-level intervention had a positive impact on production of *Jambars* by urban producers. Sales of firewood Jambar trend downwards over the period of the study. This is why we do not study structural breaks in the time series data of producer sales as announced in the PAP. We do not combine the sales of producers and vendors as announced in the PAP (Hypothesis f) since this does not correctly measure the full market volume but double counts all stoves sales by vendors bought from our producers. We show both sales volumes separately.

Section 5: We did not pre-specify the analysis of markups along the supply chain. This analysis is exploratory and justified by the observation of small effect sizes in our RCT analysis.

Appendix C2: We announced that we would use 12 business and vendor characteristics in balance checks. To benefit from our extensive baseline survey and provide contextual information, we added additional variables to the balancing. We highlight pre-specified balance variables.

Appendix C4: We announced that we would test for the impact of our treatments on the use of multiple marketing materials (as secondary outcomes). This included five outcomes that ask directly for use of our treatment elements (e.g., use of posters).

- Our survey question on the outcome “whether vendors contacted village chiefs” was ambiguous and was therefore excluded from the analysis.
- The PAP listing was incomplete in that it did not list two elements of our treatments (whether T-shirts are used for marketing and whether vendors used transport grant). Note that the PAP mentions the two elements in other contexts. We include these outcomes in the analysis for completeness.

- We added the outcomes “any marketing activity” and “any intervention marketing tool” as aggregate outcomes, which had not been announced in the PAP.

Note: To enhance transparency of our analysis, we pre-registered this study in the Registry for International Development Impact Evaluations (RIDIE) under RIDIE-STUDY-ID-59c9e0f49a591 in September 2017. The PAP was published in March 2019 prior to endline data collection. We adhere to the PAP throughout the paper. Yet, we deviate in some aspects, when – only after endline data collection – announced procedures proved unreasonabl/impossible or non-announced procedures proved reasonable/ possible. This table lists all deviations and provides our rational behind them.

Appendix B: Details on study implementation and materials

Table B. 1 summarizes the key components of the study and their respective purpose.

Table B. 1: Study components

Component	Sample Size	Approach	Purpose
Producer survey	215 (full census)	In-person baseline survey	Descriptive statistics of producer businesses
Producer tracking	215 (full census)	Monthly phone-tracking of producers' firewood *Jambar* sales	Outcome in impact analysis
Vendor surveys	127	In-person baseline and follow-up surveys in 60 rural markets	Descriptive statistics of vendor businesses; Outcomes in impact analysis
Vendor tracking	127	Monthly phone-tracking of vendors' stove sales (all main stove types)	Outcome in impact analysis
Village demand survey	937	Experimental elicitation of WTP for the firewood *Jambar* among 937 households in 60 rural villages via sealed-bid, second-price auctions	Descriptive statistics of demand for *Jambar*. Demand information channeled to vendors as key component of the vendor-level intervention

During the regular tracking period, i.e., the 20 months sales tracking surveys in 2018 and 2019, we asked vendors to report all stove sales that had occurred since the prior interview. We thereby capture all stove sales, even if vendors cannot be reached every month. During these calls, vendors were asked to recall their stove sales, referring to a sales log sheet we had handed out to them for bookkeeping support (see Figure B. 1). The goal of these log sheets was to facilitate vendors' self-tracking of their sales and to reduce the burden of recall. In designing these sheets, we aimed to ensure that vendors who were numerate, but illiterate could easily use the sheets to track their sales. The sheet was designed after consultations with local partners and extensive piloting. Field teams also conducted demonstrations and practice rounds with vendors to provide an overview of how to record sales, underscoring any language could be used to complete the sheet. Approximately 68 percent of all reported sales were tracked using the log sheets. The remainder were based on pure oral recall. In the longer-term follow-up surveys in 2021, we elicited sales for the preceding month. Each vendor received a 500 CFA (USD 0.85) incentive payment in the form of cell phone credit after participating in a survey round. Producers received a similar sales log sheet. The key difference between the producer and vendor tracking is that we asked producers to report sales of only the firewood *Jambar*. This was done to prevent lengthy interviews, given the high quantities manufactured.

The materials distributed through the randomized vendor intervention were a village demand sheet (Figure B. 2) and marketing materials (Figure B. 3). The demand sheet conveyed information on households' WTP from the village demand survey. As each vendor operates in a unique set of rural markets, each vendor received a customized sheet that referred to seven villages nearest to the market from which they were enrolled. For each village, the following information was displayed:

- the highest bid for the firewood *Jambar* as displayed by the figure with two outstretched arms in Figure B. 2;
- the share of auction participants willing to pay more than XOF 7,000 or USD 12.30 (the firewood *Jambar's* approximate retail price as communicated to the research team by FASEN staff) as displayed by the share of black figures relative to white figures;
- the extrapolated total number of households willing to pay more than USD 12.30 (i.e., share of participants who bid above times the village's population) as displayed by house figure;
- the contact information of a local village authority displayed on the left; and
- the village population, as displayed on the left in Figure B. 2.

Figure B. 1: Log sheet for bookkeeping

A: Vendors

FICHE DE VENTE DES FOYERS

Nom du commerçant : ________________________

Nom et Numéro en cas de question : [redacted] , [redacted]

A	B	C	D	E	F	G
Sakkanal	Jambar à bois	Jambar à charbon	Malgache	Bili	Bili Amélioré	Taaru

Nom du village / ville du client	Nom de la région du client	Nom et prénom du client	Numéro de téléphone du client	Date de vente	Quantité des Foyers vendu	Tipe de Foyer vendu	Endroit de vente			Payement	
						A B C D E F G	Sur le Louma	Au village	En ville	Payement directe	Tranches
Nguene	Louga	Fatima SYLLA	338298564	25.8.2017	3 xxx	A A C		x		x	

B: Producers

FICHE DE VENTE D'ÉTUDE
FOYERS AMELIORES A BOIS

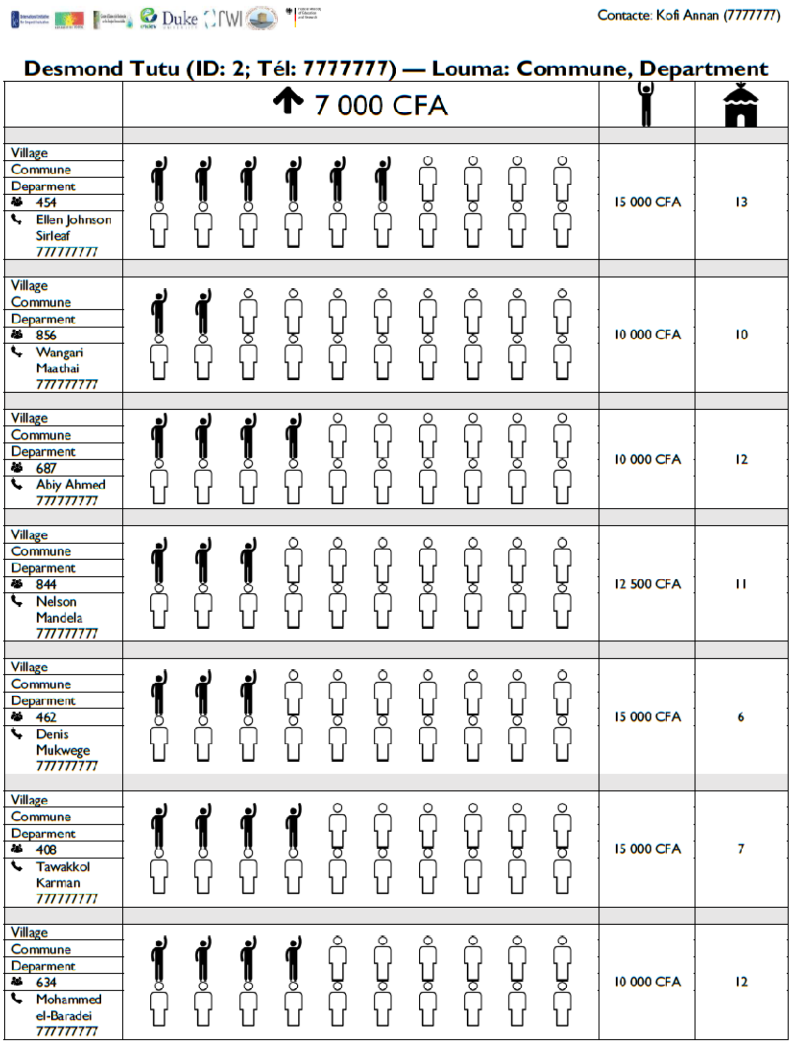
Nom du forgeron : ________________________

Nom et Numéro en cas de question : [redacted] , [redacted]

Numéro de téléphone du client	Nom et prénom du client	Nom du village / ville du client	Nom de la région du client	Date de vente	Quantité des Foyer amélioré vendu			Type de client				
					Jambar JEGG (4kg)	Jambar JABOOT (7kg)	Jambar JONGOMA (15kg)	Client individuel	Vendeur marché	Commerçant Louma	Groupement de femmes	Autre, spécifiez
338298564	Ousmane SYLLA	Nguene	Louga	25.8.2017	3 X X X		1 X		X			

Note: Panel A: On top, the vendor logsheet features a list of the most important stoves available on Senegalese rural markets. Below, vendors fill-out one row per client who bought a stove. The first entry is a non-existing example for explication purposes. Panel B: The producer log sheet tracked sales of firewood Jambar of 4kg, 7kg, and 15kg. The last rows capture whether the client is an end-user, a vendor on fixed markets, or a vendor on weekly markets, a women's group or other. Retailers who resell on both weekly and fixed markets are categorized as weekly market vendors, i.e., they at least sometimes sell on weekly markets. Women groups are always categorized as women groups, independent of whether they resell the stove.

Figure B. 2: Village demand leaflet (anonymized)



Contacte: Kofi Annan (7777777)

Desmond Tutu (ID: 2; Tél: 7777777) — Louma: Commune, Department

	↑ 7 000 CFA		
Village Commune Deparment 454 Ellen Johnson Sirleaf 777777777		15 000 CFA	13
Village Commune Deparment 856 Wangari Maathai 777777777		10 000 CFA	10
Village Commune Deparment 687 Abiy Ahmed 777777777		10 000 CFA	12
Village Commune Deparment 844 Nelson Mandela 777777777		12 500 CFA	11
Village Commune Deparment 462 Denis Mukwege 777777777		15 000 CFA	6
Village Commune Deparment 408 Tawakkol Karman 777777777		15 000 CFA	7
Village Commune Deparment 634 Mohammed el-Baradei 777777777		10 000 CFA	12

Source: Own presentation.

Figure B. 3: Marketing elements

A: T-Shirt with firewood *Jambar* logo

B: FASEN DinA4 leaflet with do's and don't on stove use

C: FASEN DinA3 poster

Note: prior to our experiment the marketing materials were developed, piloted and used by FASEN to support its outreach activities with Jambar producers.

Appendix C: Data

C1. Results from vendor baseline survey on adoption and outreach barriers

In structured interviews at baseline, we asked vendors about their perceptions of barriers to reaching new customers and the reason for low household uptake of *Jambars*. In response to an open-ended question about what was preventing them from reaching out to new customers, most vendors told us that liquidity constraints and high transport costs were the reason (see Table C1). They also felt that the price of *Jambars* was too high for households and that households were poorly informed about the benefits of *Jambars*.

To assess the relevance of our intervention, we asked vendors directly whether contact details of producers and demand information from villages would help them increase their *Jambar* sales. About two-thirds said it would.

Table C. 1: Vendor baseline opinion on business barriers

	Mean	SD	N
Why don't households adopt Jambar?			
Stove price	0.66	0.48	122
Lack of knowledge on *Jambar's* benefits	0.38	0.49	122
Cultural preferences / habits	0.13	0.34	122
Lack of knowledge that *Jambar* exist	0.12	0.33	122
Price and availability of fuels	0.09	0.29	122
Maintenance requirements and durability	0.00	0.00	122
Lack of availability	0.02	0.16	122
Don't know / refuse	0.08	0.28	122
Why don't you reach out to new regions to get new clients?			
No interest	0.28	0.45	127
Transport costs too high	0.27	0.44	127
Liquidity constraints	0.20	0.41	127
Don't know where to go	0.07	0.26	127
Low demand	0.07	0.26	127
Risk too high	0.06	0.24	127
Other	0.21	0.41	127
For increasing Jambar sales, ….			
… producer contacts very helpful	0.49	0.50	127
… producer contacts a bit helpful	0.20	0.40	127
… demand information very helpful	0.56	0.50	127
… demand information a bit helpful	0.10	0.30	127

C2. Vendor characteristics and balancing

Table C. 2 shows that the sampled vendors were around 40 years old at the time of the baseline survey, around two thirds are men and most of them had had no formal education except for Koranic schooling at the time of the baseline survey. At baseline, over 80 percent reported to have experienced business growth over the prior 12 months. Average revenues in a typical month amounted to around USD 2600. Only around half of the vendors had obtained credit over the last 12 months and liquidity constraints were one of the most important barriers to increase outreach. Few vendors bought EEBCs directly from producers, with most buying from wholesalers operating in urban areas. Only around half of vendors engaged in any marketing activities, mostly limited to word-of-mouth promotion efforts. At baseline, hardly any firewood *Jambars* were being sold (0.08 per month). The most sold stoves were charcoal *Jambars* (2.3 per month), but overall, quantities of manufactured stoves of any type at baseline were extremely low.

To check balance between our treatment arms, we performed pairwise t-tests for differences in means between the groups and display corresponding p-values. Given the small number of observations and the large number of balance checks, we also show pairwise normalized differences. We highlight stratification criteria and pre-specified balancing criteria. The results suggest that there were few meaningful differences between the groups at baseline. The grant group seems to perform slightly worse than the control and the information group, with lower average monthly revenues. In turn, the information group might be slightly better off with greater access to credit and less reported interest in increasing consumer outreach (possibly because sales are already at satisfactory levels). In sum, the balance checks point to the success of the intervention randomization.

Table C. 2: Balancing of vendor and business characteristics, at baseline

Variable	mean (1) Control	(2) Information	(3) Grant	t-test p-value (1)-(2)	(1)-(3)	(2)-(3)	Normalized difference (1)-(2)	(1)-(3)	(2)-(3)
	n=31	n=48	n=48						
Vendor characteristics									
Age[2]	41.61	39.60	41.06	0.384	0.836	0.523	0.202	0.048	-0.131
Male	0.61	0.67	0.69	0.631	0.501	0.829	-0.112	-0.156	-0.044
No education/ Koranic school[2]	0.61	0.69	0.67	0.501	0.631	0.829	-0.156	-0.112	0.044
Business characteristics									
Reported monthly revenues in USD[2]	2771	3190	1868	0.692	0.172	0.115	-0.092	0.316	0.323
Business growth 12months: negative[2]	0.07	0.13	0.06	0.391	0.972	0.298	-0.199	0.008	0.213
Business growth 12months: constant[2]	0.13	0.08	0.15	0.517	0.836	0.342	0.151	-0.048	-0.195
Business growth 12months: growth[2]	0.81	0.79	0.79	0.875	0.875	1.000	0.037	0.037	0.000
Monthly revenue above median[1]	0.58	0.50	0.52	0.489	0.608	0.840	0.161	0.119	-0.041
No. of markets vendors sells in[1, 2]	3.0	3.0	3.1	0.918	0.842	0.733	0.024	-0.046	-0.070
Days on markets (mnthl.)	20.87	21.10	19.81	0.904	0.605	0.472	-0.028	0.120	0.148
Employees	0.968	0.854	1.021	0.736	0.877	0.537	0.079	-0.036	-0.127
Collaborates with stove vendors	0.42	0.33	0.35	0.445	0.566	0.832	0.177	0.133	-0.044
Prefers present pay-off	0.90	0.75	0.94	0.092*	0.580	0.011**	0.388	-0.129	-0.514
Impression: entrepreneurial ambitions	0.48	0.48	0.54	0.968	0.621	0.545	0.009	-0.115	-0.124
Loans and credit									
Credit in last 12 months[2]	0.48	0.60	0.48	0.299	0.968	0.223	-0.241	0.009	0.250
Has a debtor[2]	0.81	0.85	0.85	0.582	0.582	1.000	-0.128	-0.128	0.000
Offers credit/installment[2]	0.90	0.96	0.92	0.332	0.840	0.404	-0.225	-0.047	0.171
Has a bank account[2]	0.36	0.46	0.44	0.369	0.471	0.839	-0.209	-0.167	0.042
Source of EEBC									
…city wholesaler[2]	0.39	0.35	0.33	0.770	0.631	0.832	0.068	0.112	0.044
…market distributor[2]	0.03	0.04	0.08	0.833	0.369	0.404	-0.049	-0.208	-0.171
…producer[2]	0.16	0.17	0.13	0.951	0.654	0.568	-0.014	0.104	0.117
….city market[2]	0.03	0.02	0.00	0.756	0.216	0.320	0.070	0.254	0.204
No. of producers buying Jambars from	0.42	0.35	0.46	0.723	0.855	0.534	0.083	-0.043	-0.128
Outreach barriers									
...locations unknown[2]	0.07	0.04	0.10	0.656	0.551	0.243	0.104	-0.139	-0.239
...no liquidity[2]	0.23	0.23	0.17	0.973	0.519	0.448	-0.008	0.150	0.156
…transport costs[2]	0.16	0.31	0.29	0.135	0.190	0.826	-0.346	-0.303	0.045
…low expected demand[2]	0.03	0.06	0.10	0.555	0.244	0.465	-0.137	-0.270	-0.150
…no interest[2]	0.42	0.17	0.29	0.013**	0.248	0.148	0.568	0.268	-0.296

*Note: ***, **, and * indicate significance at the 1, 5, and 10 percent critical level. [1] stratification criterion [2] pre-specified balancing criterion*

Table C. 2: Balancing of vendor and business characteristics, at baseline (cntd.)

	mean			*t*-test			Normalized difference		
	(1)	(2)	(3)	p-value					
Variable	Control	Information	Grant	(1)-(2)	(1)-(3)	(2)-(3)	(1)-(2)	(1)-(3)	(2)-(3)
Business-related costs									
Share of transport cost in total cost[2]	0.51	0.54	0.48	0.688	0.719	0.397	-0.093	0.084	0.174
Weekly business cost: transport[2]	15.95	23.96	17.52	0.243	0.745	0.284	-0.270	-0.075	0.220
Weekly business cost: employees[2]	10.02	20.87	14.19	0.263	0.521	0.447	-0.259	-0.149	0.156
Weekly business cost: market fee[2]	5.94	5.46	3.62	0.824	0.134	0.263	0.052	0.346	0.230
Importance of Jambar in business revenue									
None[2]	0.36	0.46	0.44	0.369	0.471	0.839	-0.209	-0.167	0.042
Low[2]	0.19	0.17	0.23	0.764	0.711	0.448	0.070	-0.086	-0.156
medium[2]	0.32	0.21	0.21	0.260	0.260	1.000	0.261	0.261	0.000
high[2]	0.13	0.17	0.13	0.654	0.959	0.568	-0.104	0.012	0.117
Monthly sales of __									
Jambar	0.067	0.035	0.127	0.345	0.638	0.367	0.219	-0.109	-0.185
Charbon Jambar	2.372	2.258	2.313	0.904	0.951	0.951	0.028	0.014	-0.013
Sakkanal	0.017	0.014	0.139	0.844	0.265	0.155	0.046	-0.258	-0.291
Basic metal stove 1	1.000	0.208	0.707	0.096*	0.588	0.082*	0.383	0.126	-0.355
Basic metal stove 2	0.201	0.199	0.345	0.992	0.606	0.593	0.002	-0.120	-0.110
Bili ameliore	0.104	0.050	0.202	0.453	0.473	0.162	0.174	-0.167	-0.286
Marketing outreach activities:									
Marketing brochures	0.032	0.021	0.000	0.756	0.216	0.320	0.072	0.287	0.204
Radio/TV marketing	0.065	0.021	0.083	0.328	0.762	0.172	0.227	-0.071	-0.280
SMS marketing	0.000	0.021	0.000	0.425	N/A	0.320	-0.185	N/A	0.204
Online marketing	0.065	0.063	0.042	0.972	0.656	0.650	0.008	0.104	0.093
Whatsapp marketing	0.000	0.042	0.063	0.255	0.160	0.650	-0.264	-0.325	-0.093
Makes wallpaintings	0.000	0.021	0.000	0.425	N/A	0.320	-0.185	N/A	0.204
Household visits	0.065	0.042	0.104	0.656	0.551	0.243	0.104	-0.139	-0.239
Stove demonstrations	0.032	0.021	0.000	0.756	0.216	0.320	0.072	0.287	0.204
Word-of-mouth marketing	0.290	0.292	0.396	0.990	0.345	0.288	-0.003	-0.219	-0.218
Tontines	0.032	0.042	0.000	0.833	0.216	0.156	-0.049	0.287	0.290
Any marketing activity	0.387	0.396	0.479	0.939	0.428	0.416	-0.018	-0.184	-0.167

*Note: ***, **, and * indicate significance at the 1, 5, and 10 percent critical level. [1] stratification criterion*

C3. Producer characteristics

Table C. 3: Socio-economic producer characteristics, at baseline

	Mean	Standard deviation
Age (#)	37.77	10.04
Literacy (d)	0.39	
Active since (year, #)	2002.10	9.84
Employees (#)	2.65	2.62
Business value (USD)	9090.56	23822.11
Bank account (d)	0.43	
Loan last 12 months (d)	0.29	
Member producer association (d)	0.66	
Monthly sales of charcoal *Jambar* (#)	44.61	91.00
Monthly sales of firewood *Jambar* (#)	10.29	30.17
Monthly sales of pots and kitchenware (#)	12.74	170.61
Monthly sales of censer (#)	7.33	29.47
Expansion to new markets (d)	0.73	
Observations	215	

Note:. d = "dummy"; # = "number of." Literacy = "received more education than Koranic school"; Expansion to new markets = "has sold to clients in new markets over last 12 months".

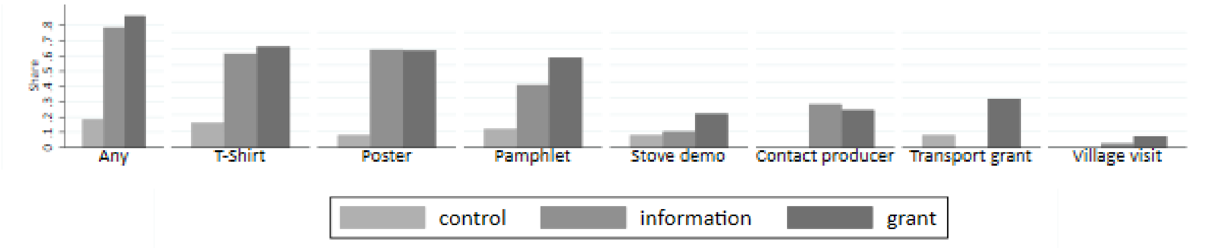
C4. Details on take-up of the randomized intervention

Figure C. 1 display the use of treatment elements for stove business by experimental group.[38] 19 percent of control vendors also report using at least one of the elements, even though they had not received any training or marketing materials over the course of the intervention, pointing to cross-vendor spillovers. These may result from interactions between vendors, fuelled by their high mobility and the generally small number of kitchenware vendors in any given location. The use of these treatment elements also increased vendors' overall propensity to conduct marketing activities. Regressing an indicator of whether any marketing activities had been performed on the treatments demonstrates that the share of vendors who engaged in any marketing activity increased by roughly 30 to 40 percentage points, a statistically

[38] Reasons for not using the distributed elements vary widely across elements and vendors. In open questions they specify that for the marketing materials, the main reasons are that the material had been broken or lost or was perceived as uninteresting for marketing by vendors. The demand sheets were difficult to understand (25 percent), lost or damaged (20 percent), villages too distance/high transport cost (20 percent), or vendors did not find the time to use them (19 percent). Producer contacts were not used mostly because vendors already had suppliers (32 percent) or they lost or damaged the sheets (23 percent). The demonstration stoves were not used for the stove business, most importantly when they were used privately or given away as a present (57 percent) or when no demand for them was identified (29 percent). Lastly, transport grants not used for the stove business were mostly used to cover other business costs (57 percent) or for other private needs (40 percent).

significant increase of 75 to 90 percent from the full sample's pre-intervention share of 43 percent (see Table C.).

Figure C. 1: Use of treatment elements for stove business, by experimental group



Note: Transport grant use is only counted if the grant was used for transporting stoves. Our definition of use includes the following activities reported by vendors. Use of t-shirt includes wearing to attract clients. Poster use includes informing clients, making marketing passionate, and hanging in stall. Pamphlet use includes informing clients, informing themselves, making marketing passionate, and hanging in stall. Use of stove demonstration includes conducting consumer presentation, conducting cooking demonstration, and showing in shop as marketing. Contact producer includes calling to get price information and calling to make an order. Transport grant includes renting transport/gas to transport stoves to a village and renting transport/gas to bring stoves to a market. Village visit includes travelling to this or other villages and travelling to find rural market customers.

Table C. 4: Impact of the intervention on vendors' marketing, by experimental group

	(1) Used *Jambar* T-shirt	(2) Displayed *Jambar* poster	(3) Conducted village sales visit(s)	(4) Contacted producers	(5) Conducted stove demon.	(6) Used transport grant for sales visit	(7) Any intervention marketing tool	(8) Any marketing tool	(9) Any marketing tool
Information	0.42***	0.52***	0.024	0.26***	0.021	-0.074	0.60***	0.0087	
	(0.10)	(0.092)	(0.024)	(0.069)	(0.069)	(0.051)	(0.099)	(0.11)	
Grant	0.47***	0.52***	0.068*	0.23***	0.13	0.22**	0.68***	0.092	
	(0.10)	(0.091)	(0.039)	(0.064)	(0.080)	(0.086)	(0.092)	(0.12)	
Post								0.057	0.037
								(0.12)	(0.12)
Information X Post								0.33**	0.39**
								(0.15)	(0.15)
Grant X Post								0.33**	0.37**
								(0.15)	(0.16)
Observations	113	113	113	113	113	113	113	240	226
Control/pre-ntervention mean°	0.15	0.074	0	0	0.074	0.074	0.19	0.43	0.43
Adj. R-sq.	0.13	0.18	0.0047	0.056	0.011	0.14	0.33	0.14	0.19
Vendor FEs	No	No	No	No	No	No	No	No	Yes

*Note: Standard errors (in parentheses) robust to heteroskedasticity; standard errors clustered at the vendor level in columns (8) and (9). Singletons omitted from estimation in column (9). * p < 0.1, ** p < 0.05, *** p < 0.01. The marketing elements in (1)-(5) are specific to the intervention and were only provided to treatment vendors. °Shows control means for columns (1)-(7), and control baseline means for columns (8) and (9).*

C5. Details on impacts of the randomized intervention on Jambar sales

To quantify the impacts of our intervention on vendors' average monthly sales of the firewood *Jambar*, we estimate a generalized difference-in-differences model (see Section 3.4). We find that after the end of the regular tracking period in March 2020, grant vendors sold approximately 0.9 more firewood *Jambars* per post-intervention month relative to the full sample pre-intervention average of 0.08 sales per month (Table C. 5b). For information vendors we do not see any significant increase. In the longer-term tracking, the grant treatment effect turns insignificant since control vendors start selling firewood *Jambars* (Table C. 5b – Columns 4-7). Results are similar if we use a dummy indicating whether a vendor sold any *Jambar* before and after the intervention (Table C. 6): The grant treatment increases the share of vendors selling any *Jambar*, while the information treatment has no significant effect.

Table C. 5a: Impact of the intervention on vendors' monthly firewood *Jambar* sales (regular tracking and longer-term period)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Any treatment X Post	0.51	0.50*	0.49*	0.085	0.26	0.25	-0.038
	(0.102)	(0.0781)	(0.0809)	(0.864)	(0.588)	(0.596)	(0.962)
Observations	2667	2794	2794	2794	2921	2921	2921
Same							
Joint							
Pre-intervention mean	0.078	0.078	0.078	0.078	0.078	0.078	0.078
Adjusted R-sq.	0.013	0.013	0.013	0.0094	0.011	0.011	0.016
Year-month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Vendor FEs	No	No	No	No	No	No	No
Period	RT	3-2021	3-2021	3-2021	9-2021	9-2021	9-2021
Attrition adjustment	n.a.	zero	Mean	Maximum	zero	Mean	Maximum

*Note: P-values in paretheses. Standard errors clustered at the vendor level. * p < 0.1, ** p < 0.05, *** p < 0.01. RT= regular tracking period; 3-2021= including sales until 3-2021; 9-2021= including sales until 9-2021. To adjust for non-balanced attrition at longer-term data collection in 2021, we apply three attrition corrections. "Zero" set sales of attrited vendors to zero; "Mean" set sales of attrited vendors to the group mean; "Maximum" sets them to the vendors' all-time maximum monthly sales. The latter approach is conservative as it assumes the upper-bound of sales for attritors, of which there are most in the control group. In the PAP, we pre-specified setting missings to the mean of the respective treatment group. The "Joint" row reports the p-value for the test for joint significance of the two treatment coefficients. The "Same" row reports the p-value for testing the hypotheses that there is no difference in the treatment coefficients.*

Table C. 5b: Impact of the intervention on vendors' monthly firewood *Jambar* sales (regular tracking and longer-term period; continued)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Information X Post	0.15	0.14	0.13	-0.24	-0.28	-0.29	-0.57
	(0.625)	(0.602)	(0.623)	(0.645)	(0.514)	(0.499)	(0.475)
Grant X Post	0.88*	0.85**	0.85**	0.41	0.79	0.79	0.50
	(0.0675)	(0.0491)	(0.0494)	(0.488)	(0.205)	(0.206)	(0.587)
Observations	2667	2794	2794	2794	2921	2921	2921
Same	0.15	0.12	0.12	0.19	0.043	0.042	0.089
Joint	0.100	0.076	0.078	0.86	0.59	0.59	0.96
Pre-intervention mean	0.078	0.078	0.078	0.078	0.078	0.078	0.078
Adjusted R-sq.	0.019	0.019	0.019	0.012	0.016	0.017	0.019
Year-month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Vendor FEs	No	No	No	No	No	No	No
Period	RT	3-2021	3-2021	3-2021	9-2021	9-2021	9-2021
Attrition adjustment	n.a.	zero	Mean	Maximum	zero	Mean	Maximum

*Note: P-values in paretheses. Standard errors clustered at the vendor level. * p < 0.1, ** p < 0.05, *** p < 0.01. RT= shorter-term (regular tracking period); 3-2021= including sales until 3-2021; 9-2021= including sales until 9-2021. To adjust for non-balanced attrition at longer-term data collection in 2021, we apply three attrition corrections. "Zero" set sales of attrited vendors to zero; "Mean" set sales of attrited vendors to the group mean; "Maximum" sets them to the vendors' all-time maximum monthly sales. The latter approach is conservative as it assumes the upper-bound of sales for attritors, of which there are most in the control group. In the PAP, we pre-specified setting missings to the mean of the respective treatment group. The "Joint" row reports the p-value for the test for joint significance of the two treatment coefficients. The "Same" row reports the p-value for testing the hypotheses that there is no difference in the treatment coefficients.*

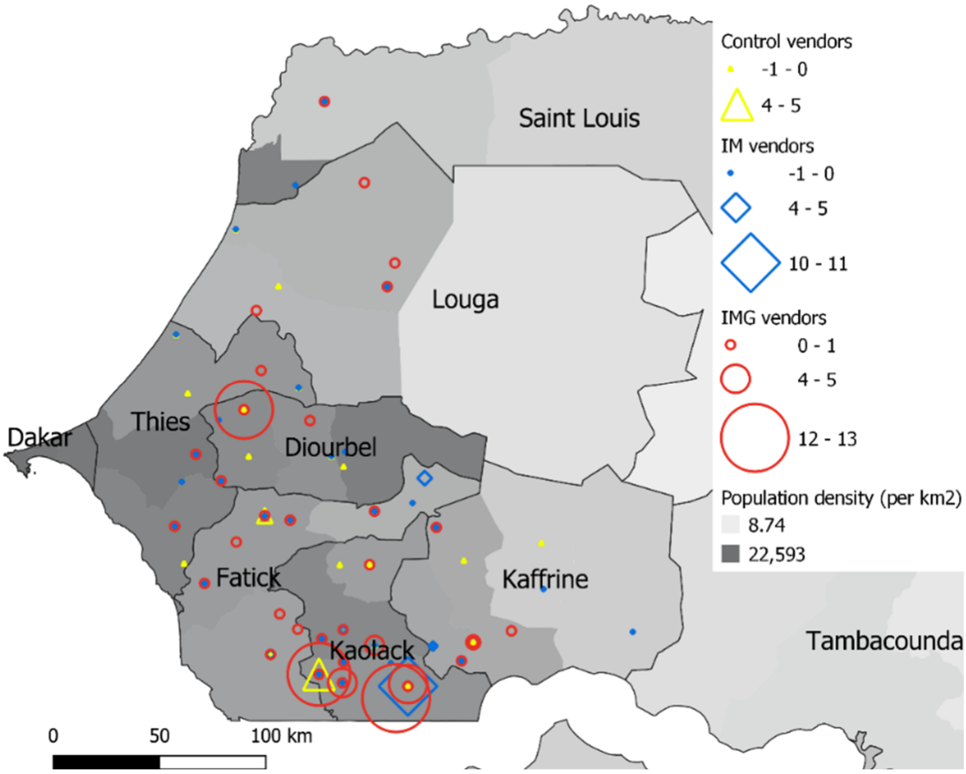
Table C. 6: Impact of the intervention on any sales of firewood *Jambar* (regular tracking)

	(1) Sells any *Jambar* pre- or post-intervention	(2) Sells any *Jambar* pre- or post-intervention
Any treatment X Post	0.17*	
	(0.0638)	
Information X Post		0.14
		(0.153)
Grant X Post		0.20*
		(0.0790)
Observations	2667	2667
same		0.55
joint		0.064
Pre-intervention mean	0.17	0.17
Adjusted R-sq.	0.013	0.027
Year-month FEs	Yes	Yes
Vendor FEs	No	No
Period	ST	ST
Attrition adjustment	n.a.	n.a.

*Note: P-values in paretheses. Standard errors clustered at the vendor level. * p < 0.1, ** p < 0.05, *** p < 0.01. RT= regular tracking period; The "Joint" row reports the p-value for the test for joint significance of the two treatment coefficients. The "Same" row reports the p-value for testing the hypotheses that there is no difference in the treatment coefficients.*

Exploring regional heterogeneities, we observe that six of the seven "super-sellers" were first surveyed in the same region (Kaolack), suggesting that our intervention was particularly impactful in that region. Again, this analysis was not pre-specified but provides valuable insights on drivers and determinants of observed impacts. Figure C. 2 shows the absolute increases from pre- to post-treatment months of all vendors spatially and by experimental group. Overall, sales in Kaolack markets seem to be higher than in other regions, and WTP estimates also show that, on average, demand is higher in Kaolack than in other regions. To better understand differences between regions, we show a set of regional characteristics in Table C. 7. If we interact the region of Kaolack and the treatment, we observe a large, but insignificant interaction effect, likely because of the very small regional sub-samples (results available upon request).[39]

Figure C. 2: Average absolute change in firewood *Jambar* sales between pre- and post-treatment months, by vendor and market location



Source: Own presentation. Population data from the Minnesota Population Center (MPC 2020) based on 2013 census data by the National Agency of Statistics and Demography (ANSD). Areas calculated using QGIS3. Note: The figure displays each vendor in the market s/he was first interviewed in. It sizes vendors by the absolute growth in their average monthly sales between pre-treatment and post-treatment months.

[39] We do find some evidence that the treatment is significantly *less* effective in two regions, namely Fatick and Louga (results available upon request).

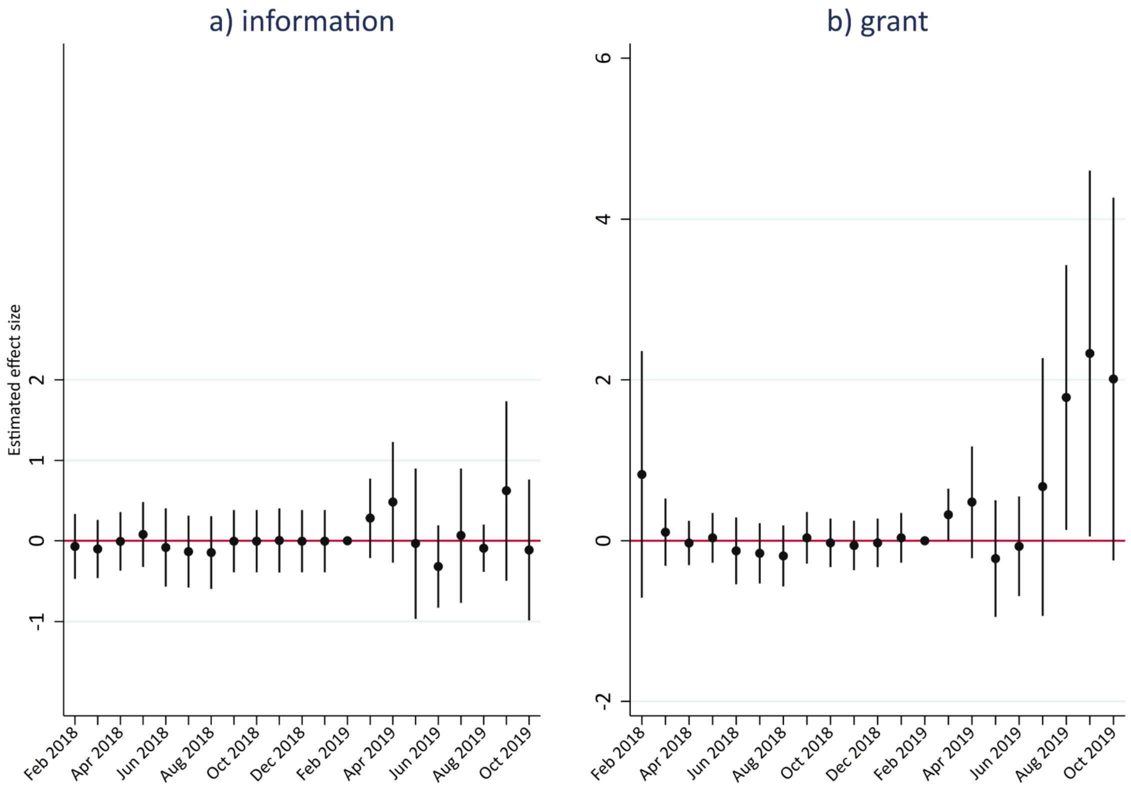
Table C. 7: Characteristics of survey regions

	Diourbel	Fatick	Kaffrine	Kaolack	Louga	Saint Louis	Thiès	Average	Total
# of FASEN producers	36	19	20	58	11	9	23	25.1	176⁺
Producer density (per 1000km²)°	7.5	2.8	1.8	10.8	0.4	0.5	3.4	3.9	
Average WTP (village demand survey; in USD)	4.3	5.0	5.3	6.5	3.7	4.6	4.5	4.8	
Median WTP (village demand survey; in USD)	4.3	4.3	4.3	4.3	3.5	3.9	0.7	3.6	
# of all markets	18	18	22	16	26	16	14	18.6	130
# of surveyed markets*	8	10	8	10	6	1	8	7.3	51
# of all vendors in surveyed markets	37	42	23	51	29	9	30	31.6	221
# of surveyed vendors	19	24	20	31	13	3	17	18.1	127
Population (in thousands)°	1,420	685	544	918	835	871	1,709	997.5	6,982
Population density°	294	100	48	171	34	45	256	135.6	
Rural population share (in %)°	84	84	84	64	78	54	51	71.3	

*Note: ⁺excludes Dakar-based producers; *excludes markets where no new vendors were willing/ available to participate in the survey; °Source: (ANSD, 2014).*

C6. Event study specification

Figure C. 3: Month-by-month impact of the intervention on monthly firewood *Jambar* sales, by treatment group



Note: This figure plots estimated values of $\hat{\beta}_1$ (panel a) and $\hat{\beta}_2$(panel b) from equation (3) with standard errors clustered at the vendor level. Error bars represent 90 percent confidence intervals. Each coefficient represents the differential impact on sales by treatment vendors relative to February 2019, the month immediately prior to the launch of the intervention (omitted to avoid perfect multicollinearity).

Figure C. 3 plots treatment effects by month, obtained from estimating the following event study specification:

$$Y_{i,m} = \beta_1(information_i \times \gamma_m) + \beta_2(grant_i \times \gamma_m) + \gamma_i + \gamma_m + \epsilon_{i,m} \quad (3)$$

where we interact the $information_i$ and $grant_i$ binary variables (representing the two treatment groups) with γ_m (the month–year binary variable) to estimate month–year-specific treatment effects. We find no difference in pre-intervention sales between control and treatment vendors. Starting with the launch of the intervention in March 2019, however, reported sales by the grant vendors increase over time. This pattern is consistent both with the self-reported seasonality in vendors' sales and suggests that the treatment took some months to produce impacts.

C7. Impacts on vendor's rural market-level sales of other stoves

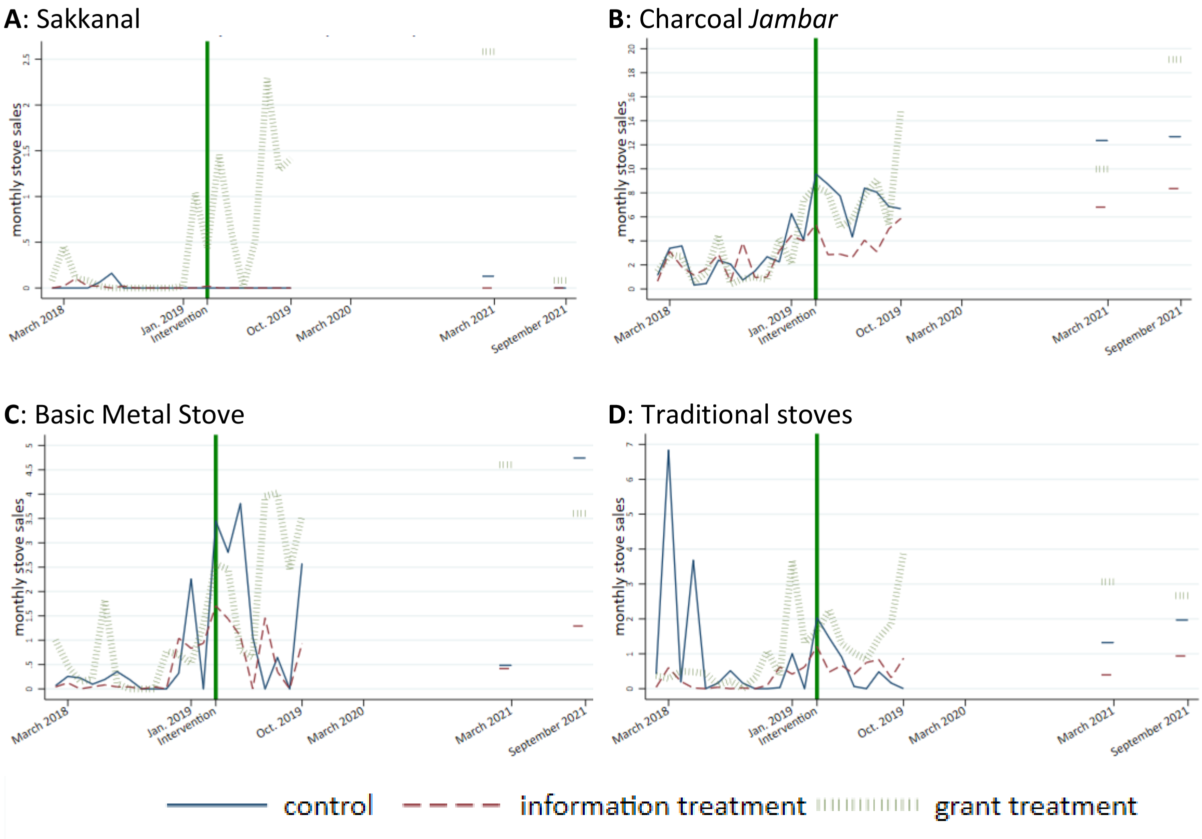
In theory, our treatment could have affected sales of other stove types negatively or positively. However, due to the small absolute increase in firewood *Jambar* sales in response to our treatment and the still much higher sales of traditional stoves, we cannot expect a discernible crowding-out effect on traditional stoves. We also do not expect the intervention to crowd out sales of other EEBC as few vendors were offering other EEBC in the first place. That said, our treatment may have positively affected sales of other stove types as there was some transferability of the treatment to other stove types, constituting cross-product externalities. For instance, some elements may have helped boost sales of the charcoal *Jambar*, which is similar (e.g., marketing materials), or even the *Sakkanal* (e.g., advertising the benefits of EEBCs in general). The producer contacts, the stronger business focus on stoves, the sensitization for new marketing approaches and the grant labelled for stove transport could also have been used to promote sales of other stoves and items.

We test whether the increased sales of the firewood *Jambar* affected vendors' sales of other stove types, both in our regular tracking period and over the longer-term. We descriptively show that *Sakkanal* sales started high and generally trended upwards among grant vendors, even in the low-sales planting season in March 2021. However, in the second longer-term data point in September 2021, *Sakkanal* sales were low, at similar levels as in the control and information group (Figure C. 4). Nevertheless, the increase in average sales after our intervention was positive and significant in the grant group (see Table C. 8). Sales of traditional stoves or charcoal *Jambar* were not significantly affected. Sales of traditional stoves were higher among grant vendors but fluctuated considerably both before and after the intervention and across groups. Charcoal *Jambar* sales had already been trending upwards before the intervention and across all experimental groups.

We derive two important lessons from these results. First, our intervention appears to have generated modest co-benefits via increased sales of another welfare-improving EEBC variant. This is highly plausible as sales of the two EEBC may be co-integrated within vendors' business models, with similar producer, consumer, transport, and pricing structures. This also highlights the advantage of relying on pre-existing market mechanisms for technology diffusion: demand and supply can flexibly react to preferences and, in this case, the reduction in sales barriers due to the grant treatment helps enable responses to latent demand.

At the same time, this finding also points to an important risk inherent in relying on pre-existing market mechanisms. Private sector actors may leverage these reduced barriers for sales of technologies that do not deliver societal or environmental welfare improvements. While this may be profitable and desirable from a vendor perspective, this diminishes the social-welfare motivation for such interventions. We did not observe such unintended negative consequences in this study, but there is descriptive indication of an upward trend in uptake of polluting technology over the longer term (Figure C. 4 - Panel D). Thus, policy engagement of this sort with the private sector must carefully manage incentives and remain cognizant of unintended effects.

Figure C. 4: Vendors' average monthly sales over time, by experimental group and stove type



Note: Y-axes differ; the green line indicates the timing of the intervention.

Table C. 8a: Impact of the intervention on monthly sales of other stove types, by experimental group and time period (regular tracking and longer-term)

Charcoal *Jambar*	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Any treatment X Post	-1.43	-1.70	-1.82	-2.43	-1.42	-1.57	-1.90							
	(0.543)	(0.443)	(0.411)	(0.328)	(0.551)	(0.508)	(0.472)							
Information X Post								-3.46	-3.68*	-3.78*	-4.17*	-3.73	-3.84	-3.82
								(0.138)	(0.0919)	(0.0832)	(0.0918)	(0.114)	(0.102)	(0.147)
Grant X Post								0.59	0.27	0.13	-0.68	0.89	0.70	0.027
								(0.826)	(0.916)	(0.958)	(0.807)	(0.743)	(0.797)	(0.993)
Observations	2667	2794	2794	2794	2921	2921	2921	2667	2794	2794	2794	2921	2921	2921
Same								0.032	0.028	0.028	0.058	0.014	0.015	0.052
Joint								0.54	0.44	0.41	0.33	0.55	0.51	0.47
Pre-intervention mean	2.31	2.31	2.31	2.31	2.31	2.31	2.31	2.31	2.31	2.31	2.31	2.31	2.31	2.31
Adjusted R-sq.	0.032	0.034	0.038	0.053	0.045	0.053	0.075	0.039	0.041	0.045	0.057	0.053	0.061	0.080
Year-month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Vendor FEs	No	No	No	No	No	No	No	No	No	No	No	No	No	No
Period	RT	3-2021	3-2021	3-2021	9-2021	9-2021	9-2021	RT	3-2021	3-2021	3-2021	9-2021	9-2021	9-2021
Attrition adjustment	n.a.	zero	Mean	Maximum	zero	Mean	Maximum	n.a.	zero	Mean	Maximum	zero	Mean	Maximum

*Note: P-values in paretheses. Standard errors clustered at the vendor level. * p < 0.1, ** p < 0.05, *** p < 0.01. RT= regular tracking period; 3-2021= including sales until 3-2021; 9-2021= including sales until 9-2021. To adjust for non-balanced attrition at longer-term data collection in 2021, we apply three attrition corrections. "Zero" set sales of attrited vendors to zero; "Mean" set sales of attrited vendors to the group mean; "Maximum" sets them to the vendors' all-time maximum monthly sales. The latter approach is conservative as it assumes the upper-bound of sales for attritors, of which there are most in the control group. In the PAP, we pre-specified setting missings to the mean of the respective treatment group. The "Joint" row reports the p-value for the test for joint significance of the two treatment coefficients. The "Same" row reports the p-value for testing the hypotheses that there is no difference in the treatment coefficients.*

Table C. 8b: Impact of the intervention on monthly sales of other stove types, by experimental group and time period (regular tracking and longer-term)

Sakkanal	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Any treatment X Post	0.44	0.52*	0.52*	0.52*	0.46*	0.47*	0.46*							
	(0.114)	(0.0899)	(0.0874)	(0.0899)	(0.0882)	(0.0832)	(0.0882)							
Information X Post								0.0056	-0.0091	-0.0094	-0.0091	-0.0079	-0.0083	-0.0079
								(0.727)	(0.676)	(0.664)	(0.676)	(0.704)	(0.689)	(0.704)
Grant X Post								0.88	1.04*	1.05*	1.04*	0.94*	0.95*	0.94*
								(0.111)	(0.0817)	(0.0792)	(0.0817)	(0.0800)	(0.0751)	(0.0800)
Observations	2667	2794	2794	2794	2921	2921	2921	2667	2794	2794	2794	2921	2921	2921
Same								0.11	0.079	0.076	0.079	0.077	0.072	0.077
Joint								0.11	0.085	0.082	0.085	0.083	0.078	0.083
Pre-intervention mean	0.062	0.062	0.062	0.062	0.062	0.062	0.062	0.062	0.062	0.062	0.062	0.062	0.062	0.062
Adjusted R-sq.	-0.00015	0.0020	0.0023	0.0020	0.0020	0.0022	0.0020	0.0072	0.012	0.012	0.012	0.011	0.011	0.011
Year-month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Vendor FEs	No	No	No	No	No	No	No	No	No	No	No	No	No	No
Period	RT	3-2021	3-2021	3-2021	9-2021	9-2021	9-2021	RT	3-2021	3-2021	3-2021	9-2021	9-2021	9-2021
Attrition adjustment	n.a.	zero	Mean	Maximum	zero	Mean	Maximum	n.a.	zero	Mean	Maximum	zero	Mean	Maximum

*Note: P-values in paretheses. Standard errors clustered at the vendor level. * p < 0.1, ** p < 0.05, *** p < 0.01. RT= regular tracking period; 3-2021= including sales until 3-2021; 9-2021= including sales until 9-2021. To adjust for non-balanced attrition at longer-term data collection in 2021, we apply three attrition corrections. “Zero” set sales of attrited vendors to zero; “Mean” set sales of attrited vendors to the group mean; “Maximum” sets them to the vendors’ all-time maximum monthly sales. The latter approach is conservative as it assumes the upper-bound of sales for attritors, of which there are most in the control group. In the PAP, we pre-specified setting missings to the mean of the respective treatment group. The "Joint" row reports the p-value for the test for joint significance of the two treatment coefficients. The "Same" row reports the p-value for testing the hypotheses that there is no difference in the treatment coefficients.*

Table C. 8c: Impact of the intervention on monthly sales of other stove types, by experimental group and time period (regular tracking and longer-term)

Traditional Stove	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Any treatment X Post	1.14	1.12	1.10	0.22	1.05	1.02	-0.32							
	(0.151)	(0.139)	(0.147)	(0.772)	(0.194)	(0.206)	(0.773)							
Information X Post								0.85	0.74	0.71	-0.13	0.64	0.61	-0.72
								(0.284)	(0.324)	(0.342)	(0.868)	(0.413)	(0.438)	(0.513)
Grant X Post								1.44	1.50*	1.48*	0.57	1.45	1.43	0.089
								(0.121)	(0.0861)	(0.0902)	(0.515)	(0.122)	(0.128)	(0.941)
Observations	2667	2794	2794	2794	2921	2921	2921	2667	2794	2794	2794	2921	2921	2921
Same								0.37	0.21	0.20	0.27	0.20	0.20	0.24
Joint								0.15	0.14	0.15	0.77	0.19	0.21	0.77
Pre-intervention mean	0.59	0.59	0.59	0.59	0.59	0.59	0.59	0.59	0.59	0.59	0.59	0.59	0.59	0.59
Adjusted R-sq.	0.0032	0.0036	0.0040	0.0072	0.0040	0.0048	0.011	0.0055	0.0069	0.0074	0.0091	0.0077	0.0086	0.012
Year-month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Vendor FEs	No	No	No	No	No	No	No	No	No	No	No	No	No	No
Period	RT	3-2021	3-2021	3-2021	9-2021	9-2021	9-2021	RT	3-2021	3-2021	3-2021	9-2021	9-2021	9-2021
Attrition adjustment	n.a.	zero	Mean	Maximum	zero	Mean	Maximum	n.a.	zero	Mean	Maximum	zero	Mean	Maximum

*Note: P-values in paretheses. Standard errors clustered at the vendor level. * p < 0.1, ** p < 0.05, *** p < 0.01. RT= regular tracking period; 3-2021= including sales until 3-2021; 9-2021= including sales until 9-2021. To adjust for non-balanced attrition at longer-term data collection in 2021, we apply three attrition corrections. "Zero" set sales of attrited vendors to zero; "Mean" set sales of attrited vendors to the group mean; "Maximum" sets them to the vendors' all-time maximum monthly sales. The latter approach is conservative as it assumes the upper-bound of sales for attritors, of which there are most in the control group. In the PAP, we pre-specified setting missings to the mean of the respective treatment group. The "Joint" row reports the p-value for the test for joint significance of the two treatment coefficients. The "Same" row reports the p-value for testing the hypotheses that there is no difference in the treatment coefficients.*

Table C. 8d: Impact of the intervention on monthly sales of other stove types, by experimental group and time period (regular tracking and longer-term)

Basic Metal Stove	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Any treatment X Post	-0.17	0.061	0.038	-0.27	-0.18	-0.21	-0.30							
	(0.881)	(0.953)	(0.971)	(0.831)	(0.868)	(0.846)	(0.827)							
Information X Post								-0.87	-0.77	-0.80	-1.05	-1.03	-1.07	-1.20
								(0.431)	(0.428)	(0.412)	(0.395)	(0.322)	(0.306)	(0.365)
Grant X Post								0.52	0.89	0.87	0.51	0.67	0.64	0.61
								(0.721)	(0.507)	(0.516)	(0.737)	(0.622)	(0.636)	(0.707)
Observations	2667	2794	2794	2794	2921	2921	2921	2667	2794	2794	2794	2921	2921	2921
Same								0.23	0.13	0.13	0.17	0.091	0.089	0.13
Joint								0.88	0.95	0.97	0.83	0.87	0.85	0.83
Pre-intervention mean	0.37	0.37	0.37	0.37	0.37	0.37	0.37	0.37	0.37	0.37	0.37	0.37	0.37	0.37
Adjusted R-sq.	0.0070	0.0074	0.0080	0.0099	0.0091	0.011	0.015	0.011	0.014	0.014	0.015	0.015	0.017	0.021
Year-month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Vendor FEs	No	No	No	No	No	No	No	No	No	No	No	No	No	No
Period	RT	3-2021	3-2021	3-2021	9-2021	9-2021	9-2021	RT	3-2021	3-2021	3-2021	9-2021	9-2021	9-2021
Attrition adjustment	n.a.	zero	Mean	Maximum	zero	Mean	Maximum	n.a.	zero	Mean	Maximum	zero	Mean	Maximum

*Note: P-values in paretheses. Standard errors clustered at the vendor level. * p < 0.1, ** p < 0.05, *** p < 0.01. RT= regular tracking period; 3-2021= including sales until 3-2021; 9-2021= including sales until 9-2021. To adjust for non-balanced attrition at longer-term data collection in 2021, we apply three attrition corrections. "Zero" set sales of attrited vendors to zero; "Mean" set sales of attrited vendors to the group mean; "Maximum" sets them to the vendors' all-time maximum monthly sales. The latter approach is conservative as it assumes the upper-bound of sales for attritors, of which there are most in the control group. In the PAP, we pre-specified setting missings to the mean of the respective treatment group. The "Joint" row reports the p-value for the test for joint significance of the two treatment coefficients. The "Same" row reports the p-value for testing the hypotheses that there is no difference in the treatment coefficients.*

Appendix D: Attrition and robustness

D1. Attrition

Table D. 1: Vendor reporting regularity, by experimental group

Vendor reported sales …	(1) Control	(2) Information	(3) Grant	(4) All	(5) ANOVA p-value
… in any month (%)	0.97	0.90	0.96	0.94	0.33
… in any pre-intervention month (%)*	0.97	0.88	0.94	0.92	0.29
… in any post-intervention month (%)*	0.94	0.90	0.96	0.93	0.49
… in last calling in 10-2019 (%)	0.61	0.56	0.73	0.64	0.23
… at least until 9-2019 (%)	0.61	0.63	0.73	0.66	0.46
… at least until 8-2019 (%)	0.61	0.67	0.73	0.68	0.55
… in in-person endline survey (%)	0.87	0.88	0.92	0.89	0.76
… mean # of months**	9.10	10.49	10.61	10.18	0.18
… median # of months**,°	10.00	11.00	11.00	10.76	
Observations	31	48	48	127	127

*Note: *includes all calls from the regular tracking period and the last reporting during in-person endline survey. **conditional on any reporting; °ANOVA is a parametric test and thereby inappropriate to test for differences in medians given the almost equal medians we abstain from adding a non-parametric test.*

The overall rate of participation in monthly vendor tracking was high (Table D. 1), but most vendors missed several months and there was considerable variation in participation over time (see Figure D. 1) and across vendors (Figure D. 2). There were no differences in participation rates across experimental groups, either before or after the intervention. Overall, 94 percent of vendors reported their sales during at least one tracking call. Conditional on reporting, the mean and median vendor participated in roughly 10 (or half of all) tracking calls.[40] In the last round of monthly calls in October 2019, we reached only around 64 percent of all vendors. We perform robustness checks to account for this high attrition rate and compare the results from the monthly calls with results from our in-person surveys, which had a substantially lower attrition rate of 11 percent.

In the longer-term follow-up, we surveyed 93 vendors in the first follow-up and 92 in the second follow-up, i.e., slightly more than 70 percent of vendors of our initial sample. In the second follow-up, we did not reach 27 vendors who no longer had functional phone numbers, six who declined to participate, and two who had passed away. Response in this follow-up is

[40] The reasons for not reporting in a given month were failure to reach a vendor (78 percent), e.g., due to turned-off phones; refusal to participate (14 percent); postponement of the interview (six percent), and death (two percent).

unbalanced across groups and is consistently higher in the grant group.[41] We therefore apply three different attrition corrections for the longer-term analyses.

Figure D. 1: Share of vendors reporting sales, by experimental group and month of call

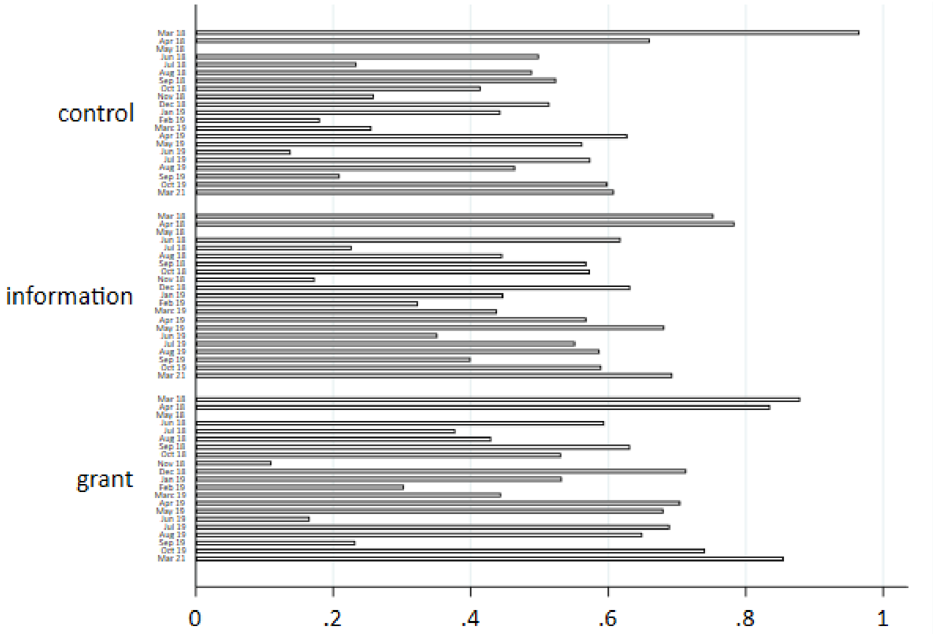
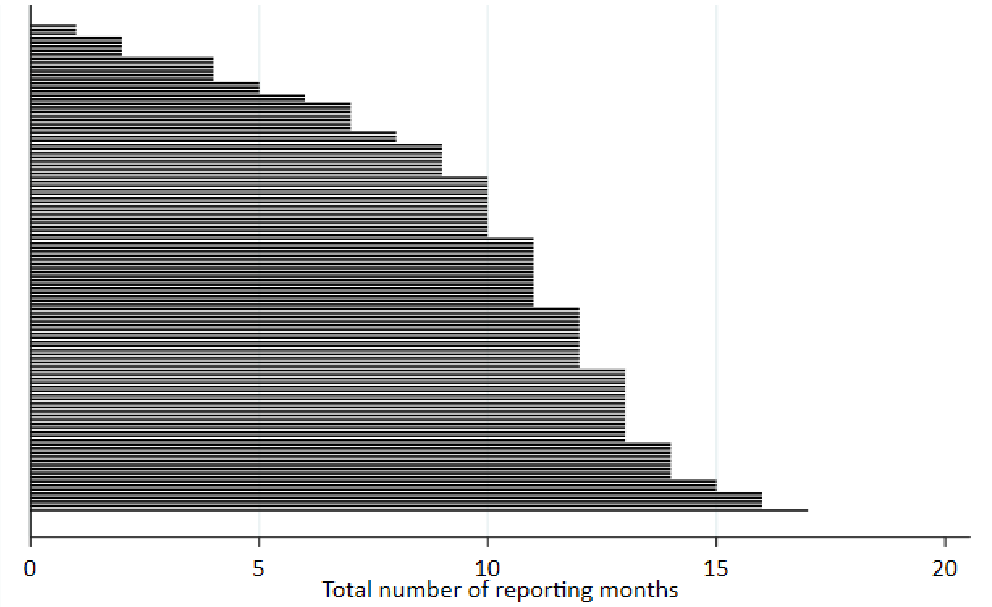


Figure D. 2: Total number of reporting months, by vendor



[41] Response rates in the first (second) follow-up are: 61 (71) percent of control, 69 (60) percent of information, and 85 (85) percent of grant vendors. These differences are significant at the five percent level.

D2. Robustness and external validity

Spillovers

Vendors are mobile, operating on average in three different rural markets; moreover, 36 percent of surveyed vendors reported cooperating with other stove vendors. Spillovers are plausible given that control vendors reported some use of intervention marketing materials, and given the high levels of interaction between vendors in these informal economies.

We observe a slight post-intervention increase in sales of firewood *Jambars* among control vendors that could arise from

(i) direct treatment contamination in the control group, for example because vendors shared treatment information or materials,
(ii) indirect information spillovers, because control vendors might observe and imitate treatment vendors' firewood *Jambar* marketing or sales,
(iii) demand spillovers, as customers may have reached out to control vendors after having learned about the firewood *Jambar* from treatment vendors,
(iv) general equilibrium (GE) effects, if the demand for firewood *Jambars* increased due to increased visibility in rural markets and villages, or increased household experimentation with the stove.

Both direct sharing of elements (i) and learning by observation (ii) are likely and have descriptive support in our data. We also have some indication for increased attention of customers to EEBC (iii). GE effects are less plausible in our setting due to the low absolute number of additional firewood *Jambar* sales.[42]

Sensitivity Analyses

Our impact results are robust to various modifications of our specification. First, they are robust to the inclusion of vendor fixed-effects to control for unobserved vendor-level heterogeneity (Table D. 2 – Column 1 and 2). Second, they are robust to the sequential inclusion

[42] Regarding (i) direct contamination, some control vendors reported having heard about or seen our demand sheets (15 percent), producer contacts (12 percent), and transport grants (23 percent). All these elements can be kept private if desired and must have been shared intentionally by treatment vendors. Our data also provide some evidence for (ii) spillovers on control vendors by observation of treatment vendors. Control vendors reported having seen or heard about firewood *Jambar* T-shirts (60 percent), firewood *Jambar* posters (42 percent), the "Do's and Don'ts" pamphlet (31 percent), and the demonstration stoves (38 percent). These treatment elements would be difficult to keep private.

of control variables into the regression (Figure D. 1). Third, they are robust to estimating an ANCOVA model only with post-intervention observations, controlling for the pre-intervention mean (Table D. 2 – Column 3 and 4). Fourth, we find significant effects also when looking at an alternative measure of the main outcome, namely vendors' sales in a *typical* month as reported during the endline survey (instead of vendors' monthly reported sales from the tracking survey) (Table D. 3). Fifth, we test our results for robustness to outliers using three procedures (see Table D. 4). While the test shows that our results are sensitive to winsorizing or trimming the highest sales, this is in line with the finding that a limited number of "super-sellers" drive the effect on sales observed for the grant treatment group.

Table D. 2: Impact on vendors' monthly firewood *Jambar* sales

	(1) Vendor FE	(2) Vendor FE	(3) ANCOVA	(4) ANCOVA
Any treatment X Post	0.51 (0.102)			
Information X Post		0.15 (0.625)		
Grant X Post		0.88* (0.067)		
Any treatment			0.5 (0.103)	
Information				0.18 (0.540)
Grant				0.81* (0.084)
Pre-intervention mean			2.15*** (0.000)	2.09*** (0.000)
Same		0.15		0.21
Joint		0.1		0.1
Pre-intervention mean	0.078	0.078		
Adjusted R-sq.	0.12	0.12	0.05	0.06
Year-month FEs	Yes	Yes	Yes	Yes
Vendor FEs	Yes	Yes	No	No
Observations	2667	2667	1016	1016

*Note: P-values in parentheses. Standard errors clustered at the vendor level. * p < 0.1, ** p < 0.05, *** p < 0.01. The "Joint" row reports the p-value for the test for joint significance of the two treatment coefficients. The "Same" row reports the p-value for testing the hypotheses that there is no difference in the treatment coefficients.The ANCOVA estimation uses only post-intervention observations used.*

Lastly, we assess the robustness of our results to two forms of attrition in reporting: non-response at endline, and non-response during monthly sales reporting.[43] First, there is balanced but substantial variation in monthly sales reporting. For example, the median

[43] Note that we partly deviate from our pre-specified analyses (see Appendix Table A. 1. for details).

number of reported months is 10-11 out of a total of 20 months (see Table D. 1). We foresaw attrition in interim months because vendors are typically highly mobile and busy. We therefore asked vendors to report sales since their last report, and thereby retrieved sales that interim attritors had not previously reported. Interim attrition in monthly reporting is therefore unlikely to bias effects between experimental groups, but less frequent reporting may lead to noisier sales estimates. We show the robustness of results by controlling for the number of times we reached a vendor and the recall period (Table D. 5a). We also apply inverse probability weights and Lee bounds.

Table D. 3: Impact of the intervention on vendors' typical monthly sales

	(1) Typical monthly sales	(2) Typical monthly sales
Any treatment	1.71*** (0.004)	
Information		0.04 (0.953)
Grant		3.24*** (0.005)
Same		0.02
Joint		0.01
Adjusted R-sq.	0.00	0.04
Observations	110	110

*Note: P-values in parentheses. Standard errors clustered at the market level. * p < 0.1, ** p < 0.05, *** p < 0.01. Only post-intervention observations used.*

For vendors who attritted from monthly sales reporting and never responded again to our calls, we cannot rely on this procedure. The share that we did not reach at the last round of calls is 36 percent (see Table D. 1). In our main analysis, we assume that these vendors did not sell any stoves after leaving our panel. As a robustness test, we restrict our analysis to only vendors who reported in the last round of calls (64 percent of vendors) and apply attrition tests. We again apply inverse probability weights and Lee bounds, which confirm our results (Table D. 5b).

Table D. 4: Robustness of main results to outliers

	(1) Natural log	(2) Natural log	(3) Winsorized 95%	(4) Winsorized 95%	(5) Winsorized 99%	(6) Winsorized 99%	(7) Trimmed 95%	(8) Trimmed 95%	(9) Trimmed 99%	(10) Trimmed 99%
Any treatment X Post	0.38		0.07		0.44		0.06***		0.23	
	(0.179)		(0.424)		(0.132)		(0.001)		(0.341)	
Information X Post		0.19		0.01		0.1		0.05**		0.05
		(0.547)		(0.872)		(0.718)		(0.012)		(0.834)
Grant X Post		0.57*		0.12		0.77*		0.08**		0.41
		(0.092)		(0.247)		(0.074)		(0.018)		(0.204)
Same		0.25		0.21		0.14		0.39		0.28
Joint		0.18		0.42		0.13		0		0.34
Adjusted r-sq.	0.02	0.02	0.04	0.04	0.02	0.03	0.02	0.02	0.01	0.02
Observations	2667	2667	2667	2667	2667	2667	2594	2594	2649	2649

*Note: P-values in parentheses. Standard errors clustered at the vendor level. * p < 0.1, ** p < 0.05, *** p < 0.01. This table tests for robustness of our main results to taking the natural log of sales, winsorizing and trimming the upper 5 and 1 percent of sales. In columns (7) and (8), also the information treatment and the pooled treatment becomes significant when trimming the upper 5 percent, as we drop all control vendors who sell the firewood Jambar, thereby reducing the control mean to zero, making any sales in the treatment groups statistically distinguishable from the control mean of zero.*

Table D. 5a: Impact of the intervention on vendors' monthly firewood *Jambar* sales - sensitivity to attrition

	(1)	(2)	(3) Inverse Probability weights	(4) Lee lower bound		(5) Lee upper bound	
Information X Post	0.15	0.17	0.12	0.15		0.26	
	(-0.625)	(-0.616)	(-0.558)	(-0.714)		(-0.363)	
Grant X Post	0.88*	0.89*	1.06*		0.92		1.34**
	(-0.068)	(-0.075)	(-0.053)		(-0.122)		(-0.01)
Times reached in monthly callings	0.03***		0.02***	0.02*	0.04*	0.01*	0.03
	(-0.008)		(-0.004)	(-0.086)	(-0.066)	(-0.094)	(-0.132)
Number of days between sales and reporting		-0.00***					
		(-0.003)					
Same	0.15	0.18	0.07				
Joint	0.1	0.11	0.06				
Adjusted R-sq.	0.02	0.02	0.03	0.01	0.03	0.01	0.03
Observations	2667	2432	2583	1426	1421	1426	1421

*Note: P-values in parentheses. Standard errors clustered at the vendor level. * p < 0.1, ** p < 0.05, *** p < 0.01. The "Joint" row reports the p-value for the test for joint significance of the two treatment coefficients. The "Same" row reports the p-value for testing the hypotheses that there is no difference in the treatment coefficients. Column 1 uses all observations and controls for the number of times the vendor has been reached in monthly callings. For vendors who dropped out, sales are set zero after dropout. Column 2 uses all observations and controls for the number of days between sales and reporting. For vendors who dropped out, sales are set zero after dropout. Column 3 applies attrition correction weights. Weights are calculated as the inverse probability of being reached in the corresponding month of calling. Column 4 and 5 display results using Lee bounds, trimming the experimental group with fewer attrition at the same percentage of vendors reached in the more strongly affected group.*

Table D. 5b: Impact of the intervention on vendors' monthly firewood *Jambar* sales - sensitivity to attrition- only vendors reached in last round of calling

	(1)	(2)	(3)		(4)	
Only vendors reached in last round of calling						
	No attrition correction	Inverse Probability weights	Lee lower bound		Lee upper bound	
Information X Post	0.29	0.64	0.26		0.56	
	(-0.57)	(-0.296)	(-0.631)		(-0.202)	
Grant X Post	1.23*	1.25*		1.54**		-0.26
	(-0.066)	(-0.061)		(-0.048)		(-0.362)
Times reached in monthly callings	0.03	0.01	0.01	0.05	-0.01	0.02
	(-0.39)	(-0.59)	(-0.597)	(-0.247)	(-0.199)	(-0.247)
Same	0.2	0.47				
Joint	0.1	0.05				
Adjusted R-sq.	0.03	0.02	0	0.04	0.01	0.02
Observations	1701	1701	924	1008	924	1008

*Note: P-values in parentheses. Standard errors clustered at the vendor level. * p < 0.1, ** p < 0.05, *** p < 0.01. The "Joint" row reports the p-value for the test for joint significance of the two treatment coefficients. The "Same" row reports the p-value for testing the hypotheses that there is no difference in the treatment coefficients. All columns display results if only vendors who were reached in the last round of calling are used. Column 2 applies attrition correction weights. Weights are calculated as the inverse probability of being reached in the last round of calling. Column 3 and 4 display results using Lee bounds, trimming the experimental group with fewer attrition at the same percentage of vendors reached in the last round of callings in the more strongly affected group.*

Table D. 6: Impact of the intervention on vendors' monthly firewood *Jambar* sales – longer-term results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Any treatment X Post	0.51	0.50*	0.085	0.26	-0.038					
	(0.102)	(0.0781)	(0.864)	(0.588)	(0.962)					
Information X Post						0.15	0.14	-0.24	-0.28	-0.57
						(0.625)	(0.602)	(0.645)	(0.514)	(0.475)
Grant X Post						0.88*	0.85**	0.41	0.79	0.50
						(0.0675)	(0.0491)	(0.488)	(0.205)	(0.587)
Observations	2667	2794	2794	2921	2921	2667	2794	2794	2921	2921
Same						0.15	0.12	0.19	0.043	0.089
Joint						0.100	0.076	0.86	0.59	0.96
Pre-intervention mean	0.078	0.078	0.078	0.078	0.078	0.078	0.078	0.078	0.078	0.078
Adjusted R-sq.	0.013	0.013	0.0094	0.011	0.016	0.019	0.019	0.012	0.016	0.019
Year-month FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Vendor FEs	No	No	No	No	No	No	No	No	No	No
Period	RT	3-2021	3-2021	9-2021	9-2021	RT	3-2021	3-2021	9-2021	9-2021
Attrition adjustment	n.a.	zero	Maximum	zero	Maximum	n.a.	zero	Maximum	zero	Maximum

*Note: P-values in paretheses. Standard errors clustered at the vendor level. * p < 0.1, ** p < 0.05, *** p < 0.01. RT= regular tracking period; 3-2021= including sales until 3-2021; 9-2021= including sales until 9-2021. To adjust for non-balanced attrition at longer-term data collection in 2021, we apply two attrition corrections. "Zero" set sales of attrited vendors to zero; "Maximum" sets them to the vendors' all-time maximum monthly sales. The latter approach is conservative as it assumes the upper-bound of sales for attritors, of which there are most in the control group.*

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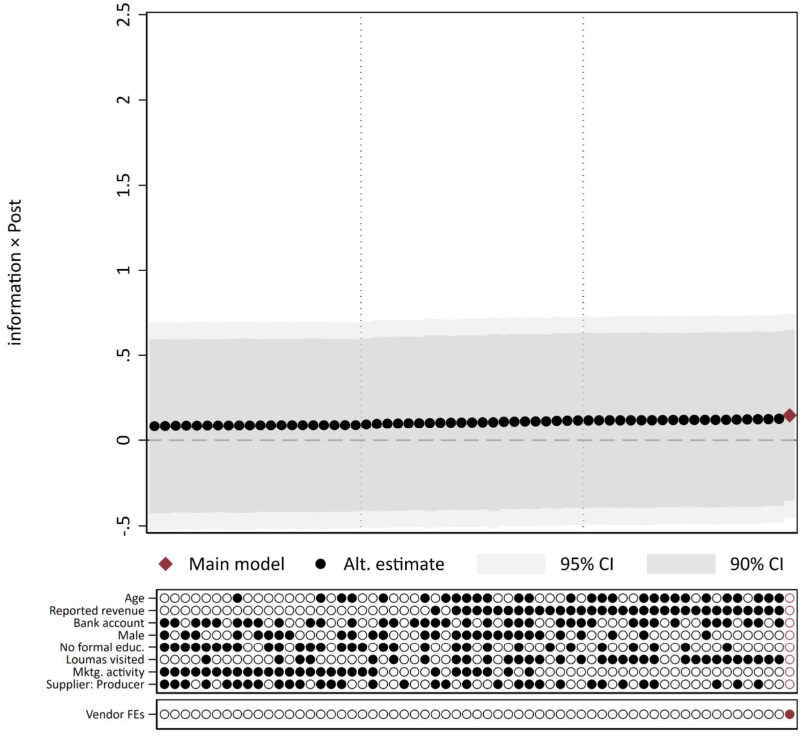
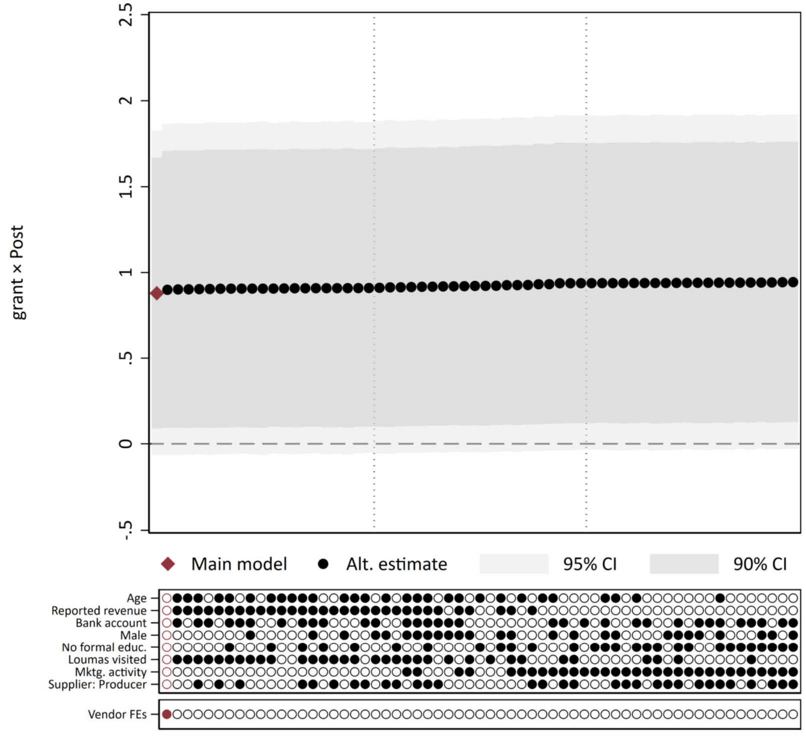
Figure D. 3: Robustness of the main results to sequential inclusion of control variables

A: Information treatment



B: Grant treatment



Note: The figures show that sequential inclusion of control variables to our main regression does not alter our results. Panel A and B plot the estimated coefficients for informationxPOST and grantxPOST based on around 250 regressions with different permutations/combinations of a set of control variables (which are also included in our balance test in Section 3.3). The panels below the figures show the controls included in each specification. For a better overview, the figures show a subsample of 60 estimates (20 of the lowest ones on the left, 20 of the highest ones on the right, and 20 randomly selected ones from the middle).

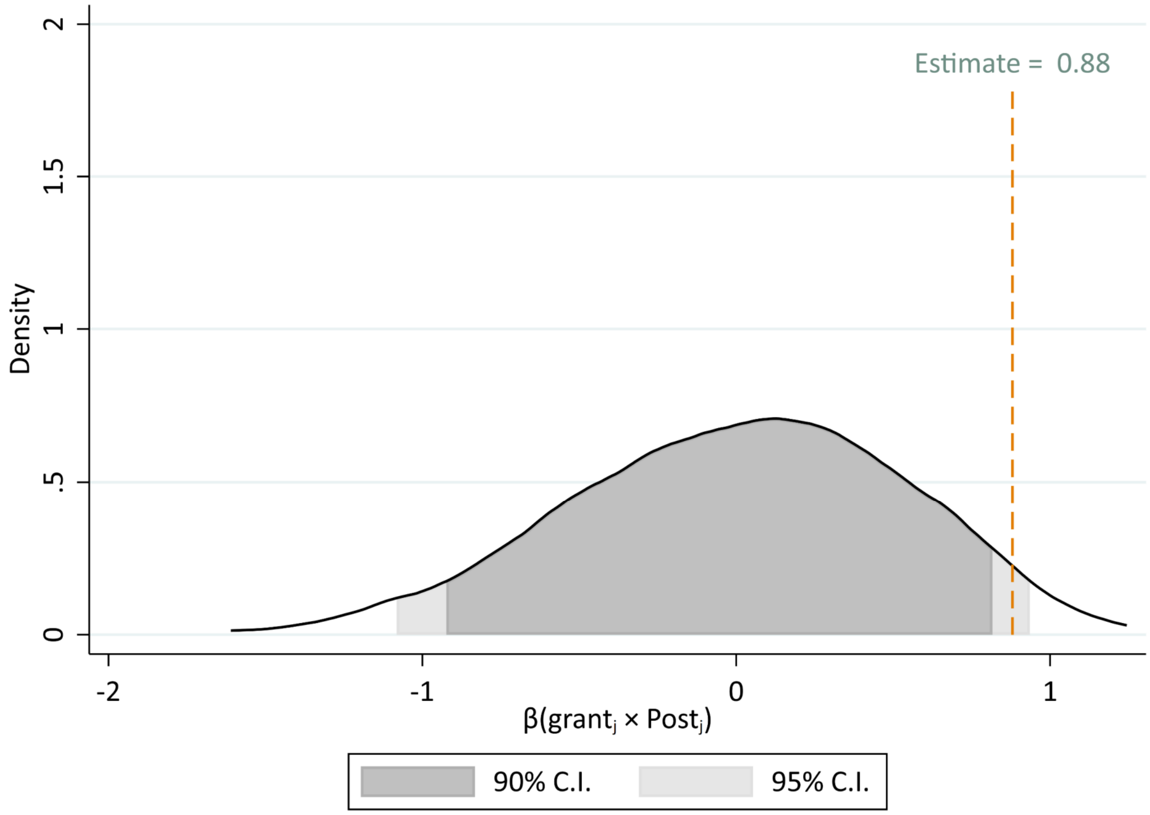
Randomization Inference

The fact that our positive treatment effect for the grant treatment is driven by very few "super-sellers" raises suspicion that it might be due to chance and driven by a coincidental grouping of high-performers in the grant group. In Figure D. 4, we present an approach inspired by randomization-based inferential procedures (Athey & Imbens, 2017) to address this concern. Specifically, we randomly assign vendors to a placebo grant treatment, a placebo information treatment, and control groups and re-estimate Equation 1(b); this process is repeated 1,000 times to obtain a distribution of placebo treatment effect estimates. If the effect we observe was due to the chance assignment of vendors to the grant treatment, we would expect to observe our actual estimate located near the middle of this distribution. Instead, we find that only three percent of these placebo estimates are greater than our actual grant treatment estimate.

External validity

Our study faces three main threats to external validity. First, our intervention targets one part of the market and is hence prone to general equilibrium effects that would be dependent on scale. For example, treated vendors might benefit from a first-mover advantage and acquire the most profitable rural consumers, which makes selling stoves in the future more difficult for other vendors. Alternatively, the first stoves sold in villages might pave the way for higher future sales as early adopters spread the word about stove-use benefits among their networks. Second, it is unclear whether a scaled version of the program would be as effectively implemented as done by our well-trained field team (Usmani et al., 2022). Third, as in any study, our insights are drawn from a specific context. For example, we build on prior FASEN engagement in a West African context that has a Sahelian climate, i.e., in which firewood is scarce and the reliance on solid fuels and traditional stoves is high. The extent to which these insights generalize to other settings, seasons or technologies remains an open question.

Figure D. 4: Randomization-based inferential procedure applied to vendor treatment allocation



Note: This figure plots the distribution of 1,000 estimated β4 coefficients from a randomization inference procedure (Athey and Imbens, 2017) applied to vendor treament allocation to estimate Equation (1b). We randomly assign vendors in the sample to a placebo information group, a placebo grant group, and a control group, and estimate Equation (1b) to obtain a placebo grant effect estimate. This procedure is repeated 1,000 times to obtain a distribution of placebo effects. The dark and light grey shaded regions indicate the 90 and 95 percent confidence intervals, respectively, of the placebo distribution. The vertical line indicates the magnitude of our actual estimated “grant treatment effect” reported in Table 1.