

Impact of Market Information on Cashew Producers in Guinea-Bissau

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Abstract

Does lack of information reduce the ability of producers to find the right time to sell their products? To answer this question, we ran a two-level cluster randomized controlled trial among 1988 cashew producers in 290 villages in Guinea-Bissau. Treated producers received free weekly messages to their mobiles during the trading seasons in 2020 and 2021. The messages provided up-to-date market news, farmgate prices, and gave commercialisation advice. We found that treated producers earned higher prices, mostly during the 2021 season. Treated producers sold their cashews more frequently relative to the other producers, who tend to sell their cashews in a single transaction. We explore several mechanisms to understand our results. We find evidence consistent with information increasing the bargaining power of treated producers, who negotiate better deals. Moreover, we find that between-village spillovers may lead us to underestimate treatment effects on prices. We found no evidence of intertemporal arbitrage, changes in preferences, or increased salience of transactions as a result of the information sent to treated producers. Given the low cost of our intervention, market information can be a cost-effective tool to increase producers' revenues.

[[PRE-ANALYSIS PLAN](#)]

We randomised author order using the AEA tool, confirmation code: IdIRV40J4H3d. Corresponding author: giulio.schinaia@economics.ox.ac.uk Affiliations: Álvarez Pereira: Nova School of Business and Economics, Universidade Nova de Lisboa and NOVAFRICA; Schinaia: Department of Economics and Centre for the Study of African Economies, University of Oxford; Schäber: European Commission; Raiss Laval Tavares: Universidade Lusófona de Bissau and Ministry of Economy and Finance of Guinea-Bissau; Mendonça: Ministry of Economy and Finance of Guinea-Bissau.

1 Introduction

International prices in agricultural commodity markets fluctuate considerably (Deaton and Laroque, 1992). Yet, for export crops, pass-through rates from international to farmgate prices are low. Low pass-through rates could be due to high transport costs and competitive forces along the value chain (Casaburi et al., 2013; Casaburi and Reed, 2022). Another reason for low pass-through could be informational asymmetries along the value chain. It is possible that producers of export crops may not be as well informed about price fluctuations as the intermediary buyers that source their products. These intermediary buyers are more integrated in global value chains and so may be better informed about prices and market news (Fafchamps and Hill, 2008). Price uncertainty could weaken producers' bargaining power and lower the prices they can negotiate.

This paper studies whether and how information technologies affect producers' outcomes by reducing price uncertainty. Our study takes place in Guinea-Bissau among producers of cashew nuts, an export crop with high price volatility. Cashew nuts account for 90% of the Guinea-Bissau's exports and are a key source of income for producers. To help producers in their sales decisions, we introduced a new market information system in the country, which combined market information from international markets with local market news and prices.¹ Our market information system provided selected producers with weekly voice and text messages to their mobile phones during the trading season, which runs between March and July. The messages contained information on market news, a range of farmgate prices across different regions in the country, and sales advice based on international market conditions. These messages were tailored to be easy to understand and to provide producers with up-to-date market information.

We evaluate the effect of reducing price uncertainty by implementing a two-level cluster randomised control trial. We randomised treatment assignment both across and within villages. In treated villages, a randomly selected group of cashew producers received free messages from our market information system during the trading season across two years, in 2020 and 2021. Our research design enabled us to estimate within-village spillover effects due to the presence of both treated and untreated producers in treated villages. We assessed the intervention's impact on various outcomes through

¹ We introduced to Guinea-Bissau the *n'kalô* service. *n'kalô* is a market information system designed by the French NGO Nitidae, which already operates in several cashew-producing countries and is a globally trusted source of information for this commodity market. The intervention is described further in Section 4.1.

in-person survey and a second phone-based follow-up conducted about 17 months and 24 months after the baseline, respectively. Analysis plans were registered for each follow-up survey before their completion.² During the first year of the intervention, only the treated group had direct access to these messages. In the second year, after the in-person survey, we informed untreated producers about the service and told them how they could subscribe to it, for a monthly fee.

The cashew nut market in Guinea-Bissau is a useful setting to understand whether reducing price uncertainty can help producers negotiate higher prices. First, producers make a high-stakes decision when deciding at what price to sell their cashews. Most producers earn their annual revenue from selling their production in one or two transactions. Second, every year, producers have several months during the trading seasons to make their transactions. While producers do not store their stock across different years, raw cashew nuts can be stored for several months after harvest as they are not highly perishable. Third, Guinea-Bissau is a price taker in the global cashew nut market. Cashew prices fluctuate across and within the trading seasons because of exogenous fluctuations in international prices. Prior to our intervention, these price fluctuations were not well communicated to producers. Fourth, there are few opportunities for spatial arbitrage for producers. Most producers sell their cashews at the farmgate to intermediary buyers, who live or visit the village during the trading season, with very few producers selling in centralised wholesale markets.

We have three main results showing that access to market information can help producers in their sales decisions. First, we find that the information system increased farmgate prices. We find a 2% intent-to-treat effect on average prices comparing treated producers with control producers in 2021. While in 2020, we find a small and not statistically significant treatment effect, *between*-village spillovers can explain why did not find a larger increase in average prices. Our intent-to-treat estimates could be an underestimate of the effect of the intervention, which could have reached some control producers in more intensely-treated areas. Indeed, we find evidence that each extra treated producer within a 5km radius increases the average farmgate price by 1%, affecting also producers in control villages closer to treated villages.³ After accounting for between-cluster spillovers we find a positive and direct treatment effect

² See <https://www.socialsciregistry.org/trials/4740> for the trial registration and analysis plan.

³ Although we acknowledge that we had not pre-specified our between-cluster analysis, our estimates of between-cluster spillovers are robust to endogenous exposure to our exogenous cluster-level randomisation, once we follow the methodology recently proposed by [Borusyak and Hull \(2021\)](#).

of the intervention comparing treated producers with control producers without any treated producers in their vicinity. Treated producers are marginally more likely to share information about prices, which may explain how spillovers occurred. Second, we show that treated producers sold their cashews more frequently relative to the other producers across both trading seasons. This result is consistent with the advice that treated producers received through our messages, which advised them to not sell all their stock in one transaction. Treated producers report that they preferred to sell more than once as a result of the advice received and because they wanted to smooth their income. We interpret the changes in the frequency of sales as evidence that reducing price uncertainty makes producers attempt less risky sales strategies. Third, treated producers earned 20-23% higher revenue from all sales and barter, relative to the control group mean. The increase in revenue is consistent with the positive price effect and is also due to a larger amount of cashews sold in market transactions, instead of barter, own consumption, or towards repayment of loans. We are cautious in not over-interpreting the magnitude of this effect, as the estimates on revenue are noisy, and we cannot fully account for the increase in the quantity sold.

The main mechanisms that explain our results is that our intervention increased bargaining power of producers. First, we find that producers in 2021 are 7% more likely to report having negotiated a higher price than the one that they were initially offered. Second, we see that treated producers are 6% more likely to sell to itinerant traders relative to other producers. Itinerant buyers may have more room to negotiate prices, as opposed to local buyers that usually work for a fixed fee for an exporter. Third, treated producers are less likely to rely on their buyers as a source of information for prices. Instead, treated producers are more likely to report having used the messages as a source of information to guide their sales decisions. Exploiting interviews conducted with buyers across a sub-sample of study villages, we find that treated villages had higher pass-through than control villages, as buyers earned lower margins but bought higher quantities. While these differences are not statistically significant, they are consistent in direction and magnitude with those obtained using the producers' data. Fourth, we find that our effects are more pronounced for producers with larger plots, consistent with idea those with larger production to sell can have more room for negotiation.

Our findings are not consistent with other behavioural explanations.

First, we do not find evidence that producers changed the timing of their sales to increase prices. Instead, we find that treated producers start selling some of their stock

earlier, despite prices being lower at the beginning of the trading season. These earlier sales are consistent with the treatment reducing price uncertainty leading producers to revise downwards their reservation prices.⁴ With lower reservation prices, treated producers are more likely to accept offers that they would have otherwise rejected. These earlier sales also help producers smooth their income during the trading season and may partly explain why treated producers did not earn higher prices, at least on average. By shifting some transactions earlier, treated producers are less likely to barter their cashew for rice later in the trading season, when the terms of barter are worse than market transactions. Second, we do not find evidence consistent with our effects being the result of better record-keeping induced by our intervention or by changes in preference parameters, such as risk aversion. Third, we also do not find evidence consistent with the timing of the messages changing the salience of transactions. Receiving regular messages could have induced producers to sell more frequently just by making transaction more salient. We do not observe producers being more likely to report a transactions within two days of having received a message, leveraging transaction diaries that a subset of producers filled with the exact date of their transaction.

We contribute to the literature on agricultural commodity markets by showing that imperfect information can affect the timing of commercialisation decisions. Information frictions could be both spatial and temporal. Producers may lack information on both *where* and *when* to sell their output to maximize profits. With the introduction of new information technologies producers have been better able to decide *where* to sell (Jensen 2007; Aker 2010; Casaburi et al. 2013; Allen 2014; Parker et al. 2016). Instead, our study shows that producers may also lack information on when it is better to sell and how much to sell at different points in time. Credit or storage constraints may prevent producers from selling at the optimal time (Aggarwal et al. 2018; Kadjo et al. 2018; Burke et al. 2019; Mukherjee et al. 2021). We are the first to causally document how information frictions affect market timing in commodity markets. Earlier studies in this literature relied on non-experimental data to look at the effects of information on market performance over time (Osborne 2004; Fafchamps and Hill 2008).

Our study adds to the literature on the use of information communication technologies

⁴ Consistent with this interpretation, we find some weak evidence that treated producers had a more realistic outlook on prices for the upcoming trading season, suggesting that the information revised downward producers' reservation prices.

in agriculture.⁵ Previous evaluations of market information systems found mixed results on the benefits of these services for farmers.⁶ Our study has three novel differences compared to previous evaluations: First, we are the first to evaluate a market information system that disseminated price information via audio messages (through robocalls and an interactive voice-response system) as opposed to text-based messages. This novel means of communication was intended to address the barriers that users with low levels of literacy face in interpreting the information they received. Previous studies identified low levels of literacy as a potential explanation for the lack of positive effects of similar text-based interventions (Fafchamps and Minten, 2012).⁷ Second, our messages provided not only a point estimate for the wholesale market price (Mitra et al., 2018) but also a range of farmgate prices for all regions in Guinea-Bissau. The messages also included market information on expected price developments and concrete commercialisation advice on when and at which price to sell production. Third, we focus on an export commodity, whose local price fluctuations are mostly driven by exogenous international prices changes. Previous studies have not focused on export commodities but rather on internally consumed commodities.

Finally, our study contributes to the literature on information exchanges in agricultural markets. A large literature has documented the role that communication networks within rural communities play in the adoption of new technologies and sharing of agricultural practices, with a strong focus on within-village spillovers (Foster and Rosenzweig, 1995; Conley and Udry, 2010; Magruder, 2018; Beaman et al., 2021). Similarly to Fabregas et al. (2019); Nakasone (2013), who respectively evaluated an SMS-based agricultural advice service and a price information system, our study was

⁵ See Nakasone et al. (2014) or Aker et al. (2016) for reviews of this literature.

⁶ Randomised evaluations of MIS in Colombia (Camacho and Conover, 2019) and India (Fafchamps and Minten, 2012; Mitra et al., 2018) have failed to find a significant average treatment effect on producer prices. In contrast, Svensson and Yanagizawa (2009) and Soldani et al. (2023) find that a MIS in Uganda and Ghana, respectively, increased producer prices by 7-10% for specific crops. In a related intervention, Goyal (2010) studies the expansion of information kiosks in district markets in Andhra Pradesh, and finds that the kiosks increased producer prices by about 1-3%.

⁷ Cole and Fernando (2021) and Gupta et al. (2021) study an agricultural extension service that informed Indian farmers of different production practices via audio-based messages. But, they did not provide commercialisation advice or price information.

explicitly designed to estimate potential *within*-cluster spillovers of our intervention.⁸ More recent work has attempted to also measure the extent to which spillovers of market information systems may occur *between*-clusters, especially within groups of villages that had pre-existing strong networks (Soldani et al., 2023; Falcao Bergquist et al., 2021). Because our study sampled villages across a large geographic area that covered almost all of the country, we are able to test for *between*-village spillovers using an identification strategy that exploits the geographic proximity of clusters and the random exposure induced by the cluster-level randomisation, in the style of Egger et al. (2022) and Miguel and Kremer (2004).

This article is structured as follows. We begin by illustrating the main features of the market for cashew nuts in Guinea-Bissau, briefly describing its supply chain (producers, intermediaries, and exporters), and the characteristics of the sample of producers we work with. To motivate the intervention, in Section 3 we lay out a simple conceptual framework to illustrate how the intervention may affect the frequency, timing, and prices of sales. In Section 4, we describe our intervention, our sampling and randomization protocol, and our estimation and inference strategy. Section 5 presents results on our main outcomes of interest. Before concluding, in Section 6, we analyze a set of potential competing explanations for the underlying mechanisms of our main results.

2 Context of the study

2.1 International market outlook and the cashew value chain

Guinea-Bissau relies heavily on cashews as its main export product, constituting over 90% of its official exports revenue. The majority of production is exported unprocessed to countries like India, Vietnam, and China, with only 10% serving the domestic market. The country's cashew market is greatly influenced by international supply and demand dynamics, similar to other raw agricultural commodities traded internationally (e.g. coffee, cocoa). However, as shown in Figure 1, the difference between export

⁸ A few papers study the presence of spillovers without relying on randomised saturation design. For example, (Cole and Fernando, 2021) and (Camacho and Conover, 2019) use variation induced by the experiment to also estimate spillovers *within*-clusters, such as the share of producers treated within a cluster or the share of producers' network that was randomly assigned to treatment. The design of Fafchamps and Minten (2012)'s study also allows for the estimation of *within*-village spillovers, but the authors do not report them, presumably since they did not find any direct effect of the SMS-based intervention evaluated.

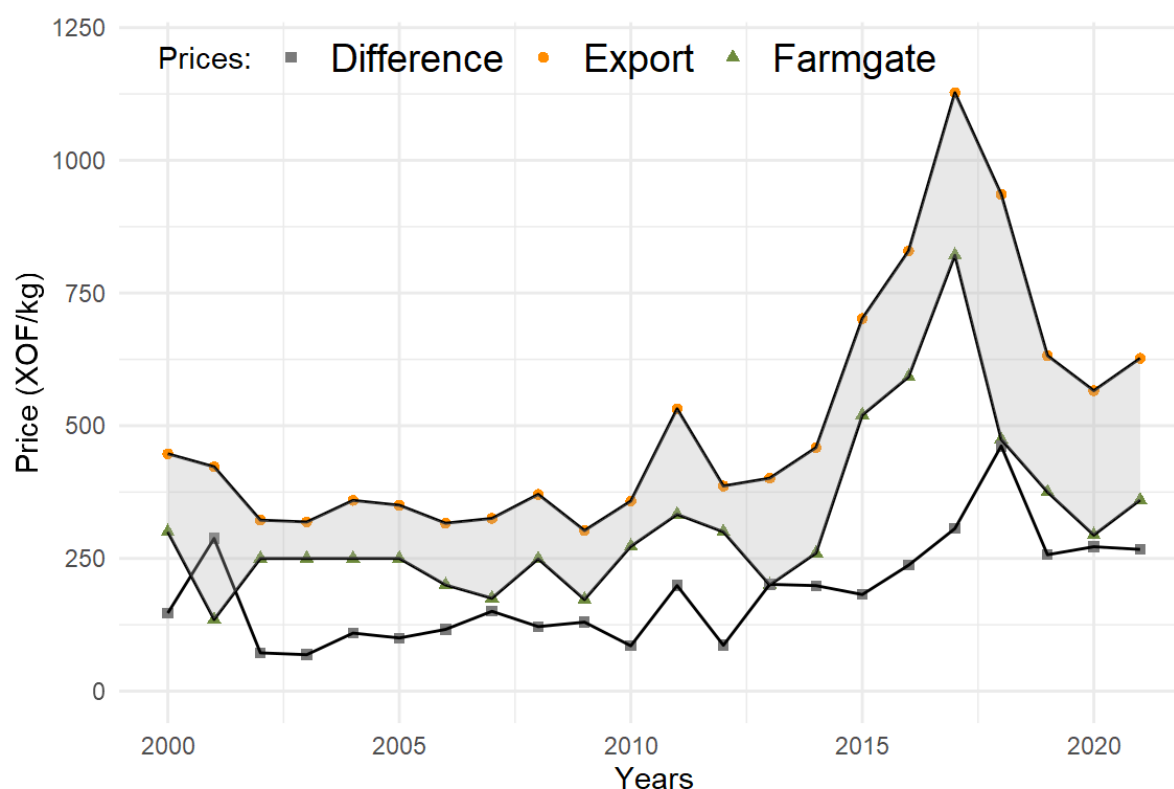


Figure 1: Margin between export and farmgate prices across years.^a

^a Source: Directorate for Macroeconomic Forecasting, Ministry of Economy and Finance of Guinea-Bissau. Raw cashew nut prices per kg reported in nominal West African CFA francs (XOF). Export prices are free-on-board annual average prices from the Bissau port collected by the Ministry of Commerce, and farmgate prices are national annual averages collected by the National Statistics Institute.

and farmgate prices vary significantly across years, which suggests that market imperfections may be reducing pass-through rates to producers.⁹

The cashew nut value chain in this market involves producers, intermediaries, wholesalers, and exporters. Intermediaries purchase raw cashew nuts from producers and either sell them to wholesalers or exporters. The number of intermediaries is uncertain, with licensed intermediaries estimated to be around 2,000-3,000 and informal traders potentially reaching up to 10,000. Local intermediaries that reside in the village usually rely on exporters or wholesalers for pre-financing and earn a fixed commission. Itinerant intermediaries are more likely to self-finance their cashew purchases

⁹ All monetary values reported are in nominal West African CFA francs (XOF), which is pegged to the euro at an exchange rate of 1 EU for 656 XOF.

through other businesses or informal credit. Wholesalers, numbering around 50, collect cashews from smaller intermediaries and handle transportation and storage until selling to exporters. The export segment is relatively competitive, with 69 registered RCN export companies in 2019, primarily Indian-owned. Exporters finance most of the operations in the value chain, fix contracts with processing plants, and bear the costs from farmgate to shipping, including insurance, banking, transport, and storage.

Margins between export and farmgate prices can vary also within the same year. Figure 2 reports the month-by-month average prices between 2019 and 2021 during the trading season (the years of our study). In the last three years margins between export and producer prices moved by almost 50 XOF (17% of the farmgate prices) from one month to the other. In general, farmgate prices tend to increase towards the end of the trading season, but there is still volatility in the level at which they will start from one year to the other and when the peak will be reached within the trading season. For example, overall prices dropped from 2020 to 2019 in part as a result of the COVID-19 pandemic shock, and increased between 2020 and 2021, in part due to the resumption of normal operation of processing factories. Appendix Section D.3 provides more details on how the trading seasons were affected by the COVID-19 pandemic.

2.2 Descriptive statistics on cashew producers

At least 40% of households in the country are involved in cashew production. The majority (85%) of raw cashew nuts producers are smallholders. In the rest of this section, we characterize producers in this market using data from our baseline survey. Our sample comprises 1988 producers across 290 villages in the country and is fairly representative of the majority of smallholder cashew producers in Guinea-Bissau.¹⁰

Villages in our study are all in relatively remote rural areas. The median village has 24 cashew producers and three buyers that live in the village. They are relatively distant from markets. The median village is 22 km away from the closest sector capital, 46 km from the closest regional capital, and 155 km away from the capital, Bissau, where most of the raw cashew nuts are exported from.¹¹

¹⁰ Appendix A provides details of our sampling strategy.

¹¹ Guinea-Bissau is administratively divided into 9 regions, including a semi-urban region for the capital, Bissau. Regions are sub-divided into sectors, which are a smaller administrative unit. There are on average four sectors per region.

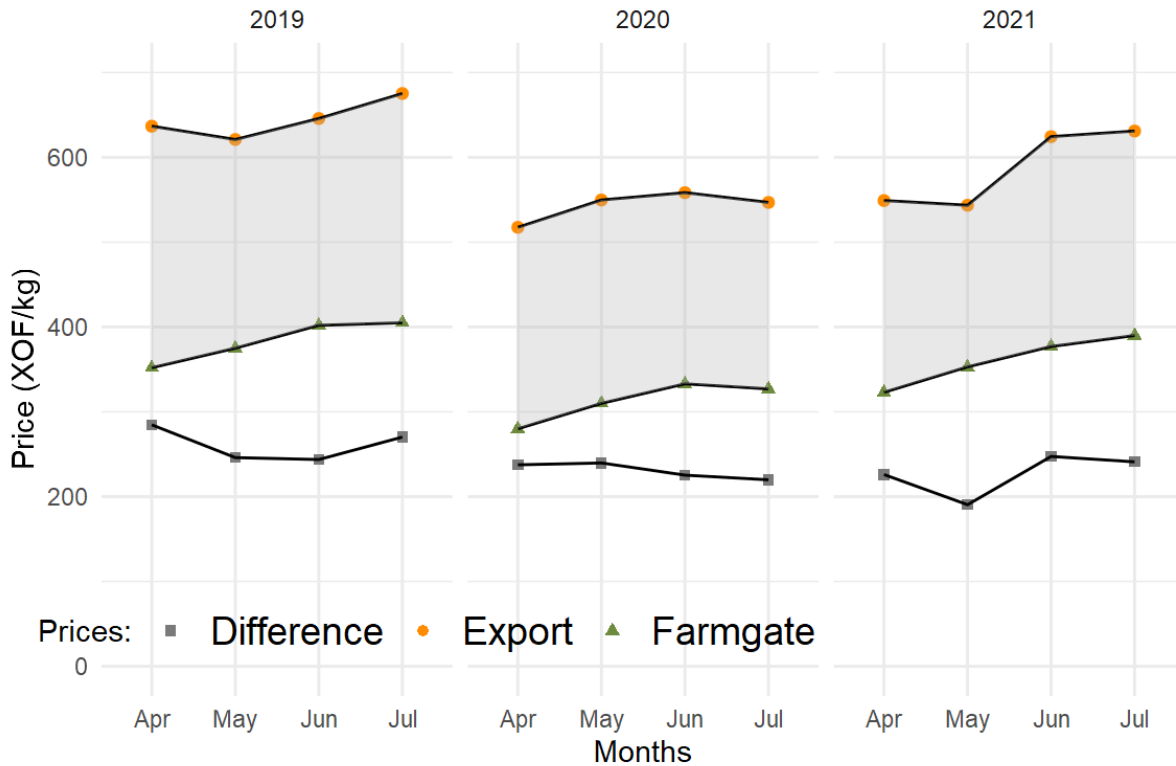


Figure 2: Margin between export and farmgate prices *within* years.^a

^a Source: Directorate for Macroeconomic Forecasting, Ministry of Economy and Finance of Guinea-Bissau. Raw cashew nut prices per kg reported in nominal West African CFA francs (XOF). Export prices are free-on-board monthly average prices from the Bissau port collected by the Ministry of Commerce, and farmgate prices are monthly averages collected from producers in our study.

Producers in our study own relatively small plots and rely heavily on cashew production as a source of income. Our descriptive statistics, in Table 1, show that cashew sales constitute the primary source of income for 80% of our sample. The size of the median cashew plantation was around 3.4 hectares. In 2019, the average producer had reported producing about 1,500 kg of cashews. In terms of demographic characteristics, almost all producers in our sample are men (94%) with an average age of 43-years old. Household size is large: the median household had twelve individuals. The median producer completed three years of education and had been working in the cashew sector for the previous nine years. About 30% of respondents were unable to read a basic sentence, hence developing a service that takes into account low literacy skills is important for most producers.

Table 1: Sample characteristics at baseline

	Mean	St. Dev.	25th percentile	Median	75th percentile
<i>Individual-level characteristics:</i>					
1 if cashews are the main source of income	0.80	0.40	1.00	1.00	1.00
Area plantation (hectares)	3.45	2.58	2.00	3.00	4.00
Total quantity produced (kg)	1526.59	1627.36	669.50	1125.00	1935.00
1 if a woman	0.06	0.23	0.00	0.00	0.00
Age	42.91	14.53	32.00	41.00	52.00
Household size	13.66	8.16	8.00	12.00	17.00
Years of education	3.95	3.85	0.00	3.00	6.00
Years of experience in cashew sector	10.41	7.11	5.00	9.00	15.00
1 if can read	0.29	0.45	0.00	0.00	1.00
1 if faces storage limitations	0.31	0.46	0.00	0.00	1.00
Number of sales	1.64	0.79	0.00	1.00	2.00
Number of potential buyers that made offers	2.82	1.77	1.00	2.00	4.00
Price per sale per kg (XOF)	390.92	100.58	332.18	370.94	450.00
1 if thinks that the reference price is important	0.38	0.48	0.00	0.00	1.00
1 if sold cashews in their own village	0.97	0.17	1.00	1.00	1.00
1 if sold most to local buyer	0.39	0.49	0.00	0.00	1.00
1 if sold most to itinerant buyer	0.54	0.50	0.00	1.00	1.00
1 if sourced market information from family	0.57	0.50	0.00	1.00	1.00
1 if sourced information on best time to sell	0.54	0.50	0.00	1.00	1.00
1 if heard of mobile MIS	0.06	0.24	0.00	0.00	0.00
<i>Village-level characteristics:</i>					
Number of cashew producers in the village	32.66	24.26	17.00	24.00	41.00
Number of cashew buyers in the village	3.57	3.41	1.00	3.00	5.00
Road distance in km to nearest sector capital	27.86	22.84	10.88	21.67	37.39
Road distance in km to nearest region capital	51.51	33.28	26.43	46.45	67.19
Road distance in km to the capital	160.63	68.37	102.08	155.56	215.86
Observations	1988				

In general, producers sell their entire annual harvest during the same trading season between March and June. Most producers do not store cashews across seasons because their quality would deteriorate significantly during the rainy season, which starts in July. About two thirds of our respondents reported having no difficulties storing their cashews within the same trading season. Most producers reported that they concentrate their sales into a single trade, as shown in Table 1. The median number of potential buyers is two, but the median number of sales is just one. This finding implies that deciding when to sell has high stakes for the majority of producers, as it will determine the largest share of their annual income. The reported farmgate price across all sales averages 390 XOF per kg. This is significantly below the reference price of 500 XOF per kg that the government had originally proposed at the beginning of the 2019 trading season. Indeed, only 38% of the respondents stated they believed the

government reference price to be an important factor in their sales decisions.

Spatial arbitrage is rare in this market, as 97% of sales occur by the producers' house or somewhere else in the producer's village. Producers rarely travel with their output to other markets, as doing so poses more risks and costs for them. Most producers do not have a network of potential buyers beyond their villages.¹² 54% of buyers are traders that temporarily visit the villages, whilst 39% are intermediaries that live regularly in the same village as the producers and that usually act as agents for an exporter.

More than half of respondents rely mostly on family and friends for commercialisation advice, with only a few producers relying on more formal channels such as producer associations. Out of those that seek advice, the timing of the sale is one of the most sought type of information by producers, alongside the right price. Only 6% of producers in our sample had heard of the previous market information system (MIS) that had been launched by a previous World Bank-financed project, but was later discontinued.

3 Conceptual framework

To motivate our intervention, we outline a simple framework to see how increased access to information can affect the timing, frequency of sales, and the average price. We borrow the model from [Mitra et al. \(2018\)](#) and add our own discussion of how this framework can explain features of the market we study in our experiment.

3.1 Setup of the model

A producer, denoted by F , engages in a negotiation with a village trader, VT . We can think of the village trader as a single trader or a group colluding. Because we are interested in the inter-temporal aspect of the decision, we assume that there are two periods, denoted by t . The first period, $t = 1$, is the beginning of the trading season, and $t = 2$ is the second-half. The model can be generalised to have more than two periods without altering the main intuition.

Traders resell the cashews at the given export price, x , which they observe. Producers cannot sell directly to exporters. Besides village traders, there are markets in larger urban areas that producers can visit to sell their stock. These markets represent the producer outside option if they fail to agree on a price with the village trader. In

¹² According to qualitative interviews implemented in some of the villages in our sample.

each period, producers choosing to sell directly to a market obtain a reservation price denoted by $M(x_t)$, net of transport costs.

Producers have to decide how much to sell of their stock in either period to the village trader. We normalise the total quantity for sale to be equal to 1, and denote with q_1 the proportion of quantity sold in the first period and $q_2 = (1 - q_1)$ as the proportion of quantity sold in the second period.

Producers do not observe x but believe it follows a prior distribution, G , with support $[\bar{x}, x]$. Producers choose what proportion to sell in either period by maximising $W(y_1) + \delta W(y_2)$, where y_t denotes the revenue at t , $W(\cdot)$ is a strictly concave and strictly increasing function satisfying $W'(0) = \infty$, and $\delta \in (0, 1)$ is a discount rate. Producers are credit constrained, so cannot borrow across periods. Traders are risk neutral and can borrow or lend at an interest i .

We can solve the producers' problem by backward induction. In period 2, F takes q_1 and p_1 as given. Then, without any information, the equilibrium is fully non-revealing and characterised by the farmgate price offered $p_2^* = E(M(x_2)|p_1)$ and a quantity sold $(1 - q_1)$. In other words, the trader will offer the producers' reservation price, who will accept it.¹³

We then turn our attention to the first period. Focusing on the non-fully revealing equilibrium offer, as in [Mitra et al. \(2018\)](#), a price offer p_1 will only be accepted if $p_1 \geq E[M(x_1)]$. F will choose to sell q_1^* to maximise $W(p_1 q_1) + \delta W(p_2(1 - q_1))$, with its associated first order condition:

$$p_1 W'(p_1 q_1) = \delta p_2^* W'(p_2(1 - q_1)) \quad (1)$$

Equation (1) generates the supply function $q_1 = q_1^*(p_1, p_2^*)$ for the first period over the range $p_1 \geq E[M(x_1)]$ and $q_1 = 0$ if $p \leq p_1$.

The relationship between the price in the first period and the amount sold in that period depends on the interplay between wealth and substitution effects. The wealth effect is captured by the concavity of $W(\cdot)$. An increase in p_1 will reduce $W'(p_1, q_1^*)$ for any q_1^* . In other words, as the price in the first period increases, the marginal value of revenue in that period falls. Substitution effects are represented by the p_1 that is pre-multiplying the left-hand side of Equation (1). In order to satisfy Equation (1), as

¹³ The price offer being fully non-revealing implies that the producer gains no information about the state of the export prices. In other words, this price offer would be a function of a constant weighted average of the possible realisations of the export prices across different states of the world.

the price increases, $W'(\cdot)$ has to fall, which means that q_1^* will have to increase. The net effect will depend on the curvature of $W(\cdot)$. To illustrate this point, suppose that $W(y) = \frac{y^{1-\theta}}{1-\theta}$, where $\theta \neq 1$ and $\theta > 0$. If θ is smaller than 1, then q_1 increases in p_1 and the substitution effect dominates—a price increase in the first period will make producer want to sell more in that period. Whereas, if θ is greater than 1, the wealth effects dominates and q_1 decreases in p_1 —a price increase in the first period increases overall opportunities to sell in both periods in order to equalise the marginal value of sales across periods. In other words, the supply function will be backward-bending in the latter scenario.

3.2 How would an information intervention change sales and prices?

The intervention is modeled, for simplicity, as a binary signal σ_t in each period, which updates producer's beliefs about the price distribution. In particular, if $\sigma_t = L$ and the signal is of a low price, then the producer expects that $x_t \in [\underline{x}, \hat{x}]$; or if σ_t and the signal is of a high price, then the producer expects that $x_t \in [\underline{x}, \bar{x}]$.

The farmgate price at each date depends on the signal, p_t^k such that $p_t^L < p_t^* < p_t^H$, where $k \in \{L, H\}$ and p_t^* is the pre-intervention price.

The proportion of output sold at $t = 1$ satisfies the following first order condition:

$$p_1^k W'(p_1^k q_1) = \delta [\alpha_k^H p_2^H W'(p_2^H (1 - q_1)) + (1 - \alpha_k^H) (p_2^L W'(p_2^L (1 - q_1)))] \quad (2)$$

where after observing the signal $k \in \{L, H\}$ the producer believes that the price will be high with probability α_k^H .

In the both the 2020 and 2021 trading seasons, the information provided to producers indicated that export price shocks were negatively correlated across the two periods, such that $\alpha_H^H \leq \alpha_L^H$. In this scenario, the producers that received a low signal in the first period, could also infer a higher probability of increased prices in the second period.

$$p_1^L W'(p_1^L q_1) = \delta [\alpha_L^H p_2^H W'(p_2^H (1 - q_1)) + (1 - \alpha_L^H) (p_2^L W'(p_2^L (1 - q_1)))] \quad (3)$$

Given the concavity of $W(\cdot)$, if the producer observes a low signal in the first period, this would induce them to sell more in that period, than if they observed a high signal. This follows from the motive to smooth revenue across periods. Treated producers might earn lower prices in the first period, but higher prices in the second period, if

wealth effects dominate.¹⁴

The model illustrates some non-intuitive features of how the intervention can change producers' sales decisions and prices. First, receiving a low price signal in the first period, may actually reduce the average price earned by informed producers in that period. This is in part due to the fact that these producers may be more likely to accept price offers that uninformed producers will not accept because uninformed producers' reservation price may be higher, as shown also in the theoretical framework proposed by Courtois and Subervie (2015). Second, since the signal sent at the beginning of the trading season were informing producers of higher prices in the latter half of the trading season, we may still expect that treated producers are able to extract more of the trade surplus in the latter half of the trading season relative to uninformed producers. These two predictions on the prices earned by treated producers may outweigh each other, resulting in no changes to the overall average price as a result of the intervention, but an increase in the frequency of sales.

4 Study design, data, and empirical strategy

4.1 Intervention: a Mobile Market Information System

Our intervention provided market information to cashew producers in Guinea-Bissau during the trading season, via weekly short text messages or robocalls.

The weekly market information messages included three components: (i) current cashew nut prices in different regions of Guinea-Bissau, (ii) important market news, and (iii) sales advice based on expected market trends. These messages were developed on a weekly basis by a network of market analysts known as the *n'kalô* service, a Market Information System, already operating in other West African countries, that we introduced to Guinea-Bissau in collaboration with the Ministry of Finance. The content was generated by combining analysis from local market analysts who gather farmgate prices and local market updates, with insights from neighboring or international markets collected and summarised by an international analyst. Appendix Table 24 provides the complete content of the messages sent during both trading seasons.

As a part of the intervention, treated producers received a one-hour training on the

¹⁴ A similar result is true if we assume the export prices to be independent across the two periods, i.e., $\alpha_H^H = \alpha_L^H$. In this case, the right-hand side of Equation (2) is independent of the signal, but a low signal in the first period would still induce more sales by concavity of $W(\cdot)$.

main factors determining the farmgate price and how receiving our weekly messages could improve their sales decisions. The research team provided the training on the same day of the baseline survey, shortly after the baseline interview took place.

The messages were delivered during the trading seasons through a combination of text messages, robocalls, and through an Interactive Voice Response (IVR) service that replayed the latest robocall sent. Specifically, in 2020 our weekly messages were delivered between April and August 2020, whereas in 2021 we delivered messages between March and July 2021 as we detail below. Short text messages were sent to those randomly selected producers between the 1st of April and the 8th of May 2020. We also delivered a short text message as our first message of the 2021 trading season. From the 16th of May 2020, the weekly messages were sent via robocalls. Finally, since the 26th of June 2020, the information was also available on demand through an IVR service. The IVR service was active for only two months prior to the end of the 2020 season, due to delays associated with COVID-19 and the implementing mobile operator, MTN. Our original plan was to develop the IVR service before the start of the 2020 season. According to our original plan, we would have had two separate treatment arms, with the first one receiving the service for free and the other one only after subscribing to it, paying a small monthly price. Given the delay in setting up the IVR service, we decided to merge these two treatment arms into one, providing the service for free to every treated producer. In 2021, while we also experienced some delays with our mobile operator in the sending of robocalls, almost all of the messages were sent as a robocall and the IVR service was active from the 23rd of April 2021.

4.2 Study design

We implemented a two-stage sampling and randomisation to conduct our study. In Appendix Section A, we provide the details of how we conducted the sampling and randomisation, which we briefly summarise here. In the first sampling stage, we selected a random sample of 290 villages with the aim of picking villages that were as far from each other as possible. Second, upon visiting the village, the research team randomly sampled seven producers to be interviewed, with the support of the local village authority, to obtain an overall sample of 1988 producers.

In the first stage of randomisation, we randomly allocated two thirds of villages to receive our intervention, and one third to act as our pure control group. We stratified random assignment by creating triplets of villages that were most similar along a set

of village-level characteristics. In the second stage, in treated villages, the research team randomly allocated the intervention only to four out of the seven interviewed producers through a public lottery. Figure 3 shows how we allocated participants across treatment, spillover and control groups.

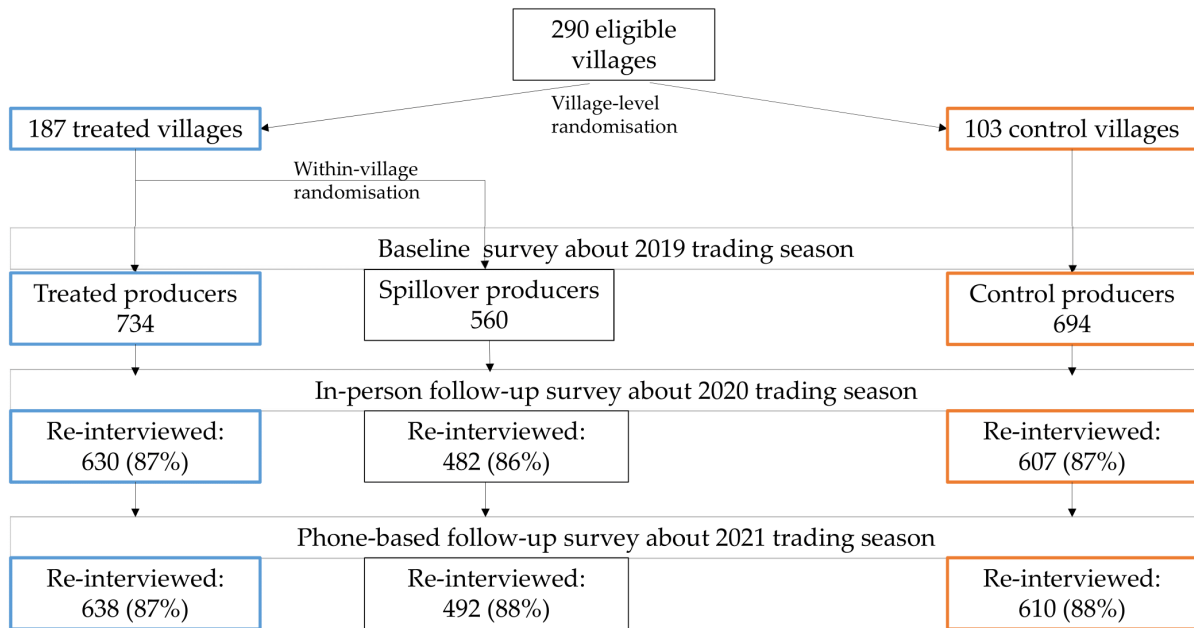


Figure 3: Flowchart of participants through the study.

We collected three rounds of interviews with cashew producers to evaluate the effects of our market information system. First, we conducted a baseline survey an in-person baseline survey between October and December 2019. Second, once it was safe to resume face-to-face contact with sanitary precautions, between April and May 2021 we conducted an in-person follow-up survey to ask about the trading season in 2020. During this survey we interviewed 86% of producers that had taken part in the baseline in 2019. We also recovered a transaction diary that we had left with producers during the baseline survey for them to fill with details of each cashew-nut transaction during the 2020 trading season. After completing the in-person follow-up survey, producers in both the treatment and control villages were provided with information on the *n'kalô* service and were shown how to subscribe as fee-paying users to receive information during the remainder of the 2021 trading season. Treated producers continued to receive free messages in 2021. Third, to assess the effects of our information in 2021, we conducted a phone-based follow-up on a key set of outcome variables between

December 2021 and March 2022, during which we reached almost 88% of the baseline sample.¹⁵

4.3 Empirical strategy

We estimate models of the form:

$$y_{iv} = treatment_{iv} \cdot \beta + spillover_{iv} \cdot \delta + y_{0iv} \cdot \gamma + \alpha_v + \epsilon_{iv} \quad (4)$$

where: i and v index individuals, and villages, respectively; y_{iv} denotes the outcome of interest measured in the follow-up; y_{0iv} denotes the outcome of interest measured in the baseline; $treatment_{iv}$ denotes individual-level assignment to the treatment group; $spillover_{iv}$ denotes individual-level assignment to the spillover group in treated villages; α_v denotes the randomisation triplet fixed effect (as described in section A.3); ϵ_{iv} is the unobserved variation in the outcome. We present estimates from Equation 4 separately for the 2020 (Year 1) trading seasons and 2021 (Year 2).

We cluster standard errors at the village-level, the unit of the first randomisation. Our coefficient of interest is β , the intent-to-treat (ITT) effect. In Appendix Section C we also describe alternative empirical specifications (e.g. top-winsorizing continuous variables, including additional controls to account for baseline imbalances, clustering at the triplet-level) to test the robustness of our main specification.

4.4 Inference and multiple-hypothesis testing adjustments

For each of our pre-specified outcomes, we test the following hypothesis:

- (i). $H_0 : \beta = 0$: The intervention had no effect;
- (ii). $H_0 : \delta = 0$: The intervention had no spillover effect;
- (iii). $H_0 : \beta = \delta$: The intervention had no effect relative to the spillover group;

¹⁵ Between July and October 2020 we had also conducted a shorter phone-based follow-up to check how our study participants were coping with the COVID-19 pandemic. We decided not to use the data from this interim phone-based follow-up to assess the impact of the service for two reasons. First, we had mistakenly contacted treated producers and later control producers making the timing of the call correlated with treatment status. Second, we only reached 66% of the baseline sample during this survey, partly because we detected fraud by one member of the research team which meant we had to discard about 10% of the interviews. Results from this interim survey are available upon request and are consistent with the data we collected in person in 2021.

For each of these hypothesis tests, we report the False Discovery Rate (FDR) adjusted q-values, taken across the family of outcomes (Benjamini et al., 2006). For each type of test, we construct a q-value for that test across outcomes. That is, we construct a set of q-values using all p-values for the null hypothesis ‘The intervention had no effect’; we construct a set of q-values using all p-values for the null hypothesis ‘The intervention had no spillover effect’, and a set of q-values using all p-values for the null hypothesis ‘The intervention had no effect relative to the spillover group’.

For clarity, we link the statistical hypothesis above with the underlying economic predictions. A rejection of the null hypothesis (i) implies that the random allocation to receive market information did affect producers’ outcomes, such as prices and revenues. Whereas a rejection of the null hypothesis (ii) implies that the intervention has affected the outcomes of untreated producers living *within* treated villages. These spillovers may be due, for example, to communication between treated producers and untreated producers living within the same village. Rejecting null hypothesis (iii) implies that producers that were allocated to the intervention (and also participated in the training) are differently affected by the treatment relative to untreated producers living in the same village.

4.5 Experimental integrity

Appendix Table 6 show that our baseline sample is relatively balanced across treatment arms for our main outcome variables of interest, except for two of the nine variables. The differences between the mean of the quantity (and value) of cashews exchanged for rice at baseline is larger in the treatment group compared to the spillover and control groups, but these differences are not very large, accounting for about 4% of the quantity sold in monetary transactions. Our empirical strategy includes the baseline value of the outcomes as control variables, which would allay concerns that imbalance in the outcome variables of interest would affect our estimates of the treatment effects.

Tables 7 and 8 show that our baseline sample is also relatively balanced across treatment arms across producer-level and village-level baseline characteristics. The differences between the mean of 9 of the 25 producer-level characteristics are statistically significant across treatment groups. However, these differences are not large, as none of the pairwise standardised differences in means are larger than 0.21 standard deviations. Out of the eight village characteristics considered, we find that treated villages have one more village trader relative to control group villages on average. As a robustness

check for our main outcomes of interest, we control for the producer and village characteristics unbalanced at baseline, and report these results in Appendix Section C.2.

Overall attrition in our follow-up surveys was small and uncorrelated with treatment assignment. In the in-person follow-up survey attrition was 14% and in the subsequent phone-based survey attrition was less than 13% overall, as shown in Appendix Table 9. We have at least one follow-up with about 78% of our baseline sample, though we focus our analysis on the sample present in either round.

To measure take-up of the intervention we rely on both self-reported and administrative data. We report both measures of compliance in Appendix Table 10 across treatment arms. Our self-reported measure is based on the answer to the question "Did you use the service in [2020/2021]?". Based on the survey responses, 25% of treated producers reported having used the service in 2020 (Year 1), with 2% and 3% of control and spillover producers, respectively, also reporting having used the service. Since after the in-person survey, all study participants had been informed of the existence of the service and shown how to subscribe to it, we find that 43% of the control group producers reported having used the service in 2021 (Year 2). We still find that treated producers were 18 percentage points more likely to have reported using the service, since this group kept receiving messages at no cost during our study.

Reassuringly, our administrative data, collected by our mobile partner operator, is consistent with the patterns observed in the self-reported data. According to the administrative data, 53% of treated producers received at least a third of messages (8 messages) sent in 2020, with no messages sent to other producers, and 28% of treated producers still received at least a third of messages (5 messages) sent in 2021, with 8% of other producers also receiving a similar amount. These statistics make us confident that the information was successfully delivered to the group of producers that had been allocated to receive it.

We prefer not to estimate treatment-on-the-treated (TOT) effects but focus our attention to the intention-to-treat estimates. Given the potential presence of spillovers within the cluster, assuming that non-compliers might have had no effect of treatment is too strong an assumption to entertain. A naive estimate of the treatment-on-the-treated would over-estimate the effect of the treatment on compliers, inflating the intention-to-treat estimates by the inverse probability of take-up.

5 Results

In this section, we present the main effects of the intervention, estimated using the data collected in the follow-up surveys over the 2020 and 2021 trading seasons. We first focus on the primary outcomes of interest (prices and revenue), before turning our analysis to quantities of raw cashew nuts devoted to different transactions (sold for cash or exchanged for rice). The analysis follows our pre-analysis plan, and we note where we may have deviated from it.

5.1 Prices and revenue

Producers can earn higher prices once they are better informed about market conditions. Table 2 shows our results from the two follow-up surveys on prices and revenue. We find a positive treatment effect on prices that corresponds to about 7 XOF/kg, equivalent to 2% of the control group mean (q-value: 0.06). This effect comes from the 2021 trading season (column 6), whereas in 2020 (column 2) the effect is also positive but smaller (less than 1% of the control mean) and not statistically significant. For 2021, we also find a difference between the treatment group and the spillover group in the average prices though not robust to multiple hypothesis testing. There is no difference between treatment and spillover group in 2020.¹⁶

We also find noisy increase in revenue from cashews overall. The second row in Table 2 shows that the total value of cashews that were either sold or exchanged (directly for rice) increased by about 21-23% of the control group mean in both years for the treatment group, by 61,650 XOF (q-value: 0.05) in 2020 and by 105,533 XOF (q-value: 0.00) in 2021.¹⁷ Relative to the spillover group, producers in the treatment group earn a marginally higher revenue from sales and exchanges, on average, but this difference is not statistically significant in 2020 and only significant at the 10% level in 2021 (q-value: 0.10). The spillover effect corresponds to around 13-15% of the control group mean,

¹⁶ The estimation sample includes all the producers that we reached in the respective follow-up survey. In order to not condition our outcome on the decision to sell, we recode the average price across sales to be equal to zero for producers that did not conduct any sales and add as a control an indicator equal to one for observations for which we impute this zero price. Our results are not affected by this imputation strategy once we control for it in the regression model.

¹⁷ While the average price of sales is only computed for monetary transactions, the total value of all sales and exchanges includes also the total revenue from cashew trades that were repaid in rice. We value the rice received in exchange of cashews using elicited hypothetical valuations from producers. We obtained these valuations through a specific module in our questionnaire designed to ask how much producers think the rice individuals in their village receive in exchange for cashews is worth in monetary terms.

but is only statistically significant in 2021 according to naive p -values. The coefficient decreases and loses statistical significance in our robustness checks, but it remains always positive and of a similar magnitude.

In the bottom rows of Table 2, we separately analyse the effects of the intervention on the value of monetary sales and the value of cashews exchanged for rice. The results in both years show that an increase in the value of monetary sales accounts for the treatment effects on the sum of these two components. In 2021, treated producers obtain on average 95,366 XOF (q-value: 0.00) more earnings from sales of cashew nuts relative to the control group, a 26% increase relative to control group mean, also significant at the 1% level. In 2020, treated producers obtain on average 51,336 XOF (q-value: 0.05) more earnings from sales of cashew nuts relative to the control group, a 22% increase relative to control group mean, also significant at the 5% level.

We find that producers in the spillover group report higher value of cashews exchanged for rice relative to the control group across both years, though this effect is not statistically significant once we account for multiple hypothesis testing. We do not find significant differences between the treatment and the spillover group, which are small in magnitude and negative.

Overall, we find that price increase for the treated producers in 2021 and that the intervention increased the value of sales and exchanges, mostly through an increase in the value of sales across both years. These results remain broadly robust to a number of alternative specifications. The effect on prices are not driven by large outliers, as our conclusions do not change once we top-winsorize the data at the 99th or 95th percentile (Appendix Table 11 and 12). The inclusion of imbalanced controls reduces the magnitude of the effect on prices by 1-3 XOF/kg in either year, which remain positive but not statistically significant (Appendix Table 13). Re-clustering reduces the statistical significance of the 2021 difference between the treatment and the spillover group. We still find a positive and significant difference in prices between treatment and control group once we pool across years (Appendix Table 16).

We note that our effects on revenue are noisy, judging from the standard error of the coefficients, though we find a similar pattern in our robustness checks. When we winsorize the total value of sales and exchanges at the 99th and 95th percentiles, respectively in Appendix Table 11 and 12, the treatment effect remain positive but smaller and not statistically significant in 2020, corresponding to about 8-10% of the control group mean, and of a similar magnitude in 2021, but not statistically different from the spillover effect. Our estimates of the effect on revenue remain broadly similar

Table 2: Results — Prices and revenue

	Year 1 (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treatment-Spillover	Year 2 (5) Mean (SD) Total obs.	(6) Treatment	(7) Spillover	(8) Treatment-Spillover
Price per sale per kg	295.71 (104.36) 1587	2.07 (3.46) [0.55]	2.33 (3.35) [0.49]	-0.26 (3.30) [0.94]	355.77 (83.13) 1704	6.89** (3.41) [0.06]*	1.32 (3.37) [0.69]	5.57* (3.05) [0.10]
Value of all sales and exchanges	287786 (315353) 1520	61650** (25296) [0.05]**	44747 (26607) [0.19]	16904 (22543) [0.61]	440724 (461069) 1686	105533*** (30721) [0.00]**	58183** (28816) [0.13]	47351* (26518) [0.10]
Value of all sales	233495 (292722) 1591	51336** (22678) [0.05]**	24129 (23274) [0.40]	27207 (19951) [0.61]	364477 (428369) 1705	95366*** (26633) [0.00]**	41746* (25166) [0.13]	53620** (24217) [0.10]
Value of exchanges	52326 (88038) 1635	10969 (8146) [0.24]	17252* (9739) [0.19]	-6282 (7314) [0.61]	75662 (111393) 1710	9775 (8464) [0.25]	15941* (8616) [0.13]	-6165 (7953) [0.44]

Notes: Producer-level intention-to-treat (ITT) estimates reported in columns 2 and 3 across year 1 (2020) and year 2 (2021). Column 4 and column 8 test for differences in parameters obtained in previous two columns. Outcome variables are listed on the left and described in detail in the pre-analysis plan. The unit of observation is the individual producer. All models control for the randomisation triplet fixed-effects and the baseline value of the outcome. Standard errors are in parentheses and are clustered at the village-level. Stars on the coefficient estimates reflect unadjusted *p*-values. Sharpened *q*-values controlling the false discovery rate across outcomes within each family are shown in brackets. * denotes significance at 10 pct.; ** at 5 pct.; and *** at 1 pct. level. Column 1 and column 5 display the control mean; standard deviation; and total number of observations across all groups in the estimation sample.

across other robustness checks, though not always robust to multiple hypothesis testing in 2020.

To better understand the impact of treatment on average producer prices, we plot the distribution of prices across treatment arms in Figure 4 for 2020 and 2021. We can see that those price effects are hard to detect in 2020. However, in 2021, our effects seem to be driven by the middle of the distribution of prices in particular treated producers are more likely to sell at the medium price of 350 relative to other treatment groups. There are no differences across groups in the likelihood of report no sales at all, as shown by the left-most bars in the figure.

5.2 Quantity sold or exchanged

To unpack our results on price and revenues, we next analyse the producers' quantities of cashews sold or exchanged, on the extensive and intensive margins. Changes in the amounts of cashews sold or exchanged can help us understand our effects on revenue, given that we found only relatively smaller increases on average prices. Indeed, in Table 3 we see that treatment group report selling on average 142 kg (*q*-value: 0.05) in 2020 and 211 kg (*q*-value: 0.00) in 2021 more than those in the control group, about a

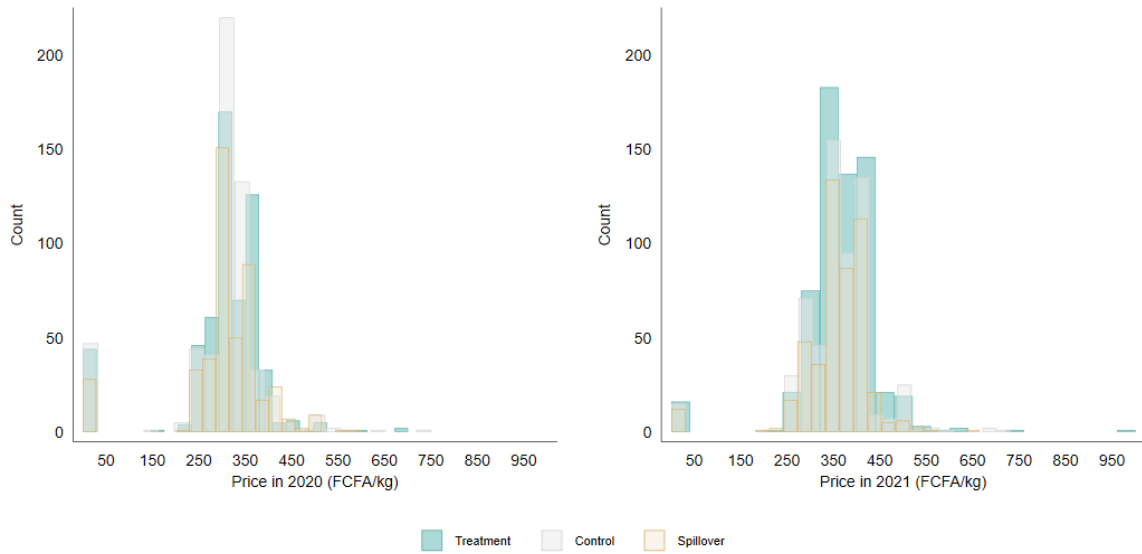


Figure 4: Distribution of average producer prices by treatment arm.^a

^a Source: In-person (2020) and phone-based follow-up (2021) surveys. Prices (average across all sales for each producer) are reported in nominal West African CFA francs (XOF). We recode the price to be equal to zero for producers that did not conduct any sales. Count refers to the number of producers in the sample.

20% increase relative to the control group mean.¹⁸ We also find statistically significant differences between the control and spillover producers in the amount of cashews sold in 2021, but not in 2020. The spillover effects are positive and lower than our treatment effects.

Our intervention induced producers to sell more frequently their cashews relative to the control and spillover groups, consistently across both years. Treated producers sell their cashews more times during the season, with the average number of sales being 0.20 higher (q-value: 0.00) relative to the control group, which represents 12-14% of the control group mean. This treatment effect is statistically significant at the 1% level is robust to our alternative specifications in both size and statistical significance. The difference between the treatment and the spillover group is also robust to our alternative specifications and remains statistically significant (q-value: 0.08-0.10). This behaviour is consistent with the advice provided by our intervention, which advised

¹⁸ In our robustness specifications the coefficients on quantity sold remain smaller but broadly robust in 2021, whereas in 2020 they also decrease and remain statistically significant at the 10% according to naive *p*-values.

Table 3: Results — Quantities sold and exchanged

	Year 1 (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treatment-Spillover	Year 2 (5) Mean (SD) Total obs.	(6) Treatment	(7) Spillover	(8) Treatment-Spillover
Total quantity sold	708.21 (855.14) 1622	141.75** (61.11) [0.05]*	63.53 (62.11) [0.45]	78.21 (54.72) [0.38]	981.13 (1163.13) 1709	211.49*** (64.24) [0.00]***	93.73 (61.88) [0.22]	117.77** (54.62) [0.08]*
Number of sales	1.49 (1.01) 1693	0.21*** (0.06) [0.00]***	0.07 (0.06) [0.45]	0.14** (0.06) [0.10]*	1.66 (0.92) 1730	0.20*** (0.05) [0.00]***	0.08* (0.05) [0.21]	0.11** (0.05) [0.08]*
1 if exchanged cashew for rice	0.56 (0.50) 1706	-0.01 (0.03) [0.85]	-0.03 (0.03) [0.45]	0.03 (0.03) [0.59]	0.56 (0.50) 1724	-0.01 (0.03) [0.60]	-0.00 (0.03) [0.99]	-0.01 (0.03) [0.58]
Total quantity exchanged	217.70 (323.48) 1681	19.62 (24.38) [0.53]	20.91 (25.70) [0.45]	-1.30 (23.10) [0.96]	231.72 (346.31) 1707	44.46 (27.70) [0.18]	70.75** (28.41) [0.07]*	-26.28 (26.38) [0.53]
Share of quantity sold over quantity exchanged and sold	0.73 (0.32) 1604	0.02 (0.02) [0.53]	0.02 (0.02) [0.45]	0.00 (0.02) [0.96]	0.79 (0.25) 1691	0.01 (0.02) [0.60]	-0.00 (0.02) [0.99]	0.01 (0.01) [0.57]

Notes: Producer-level intention-to-treat (ITT) estimates reported in columns 2 and 3 across year 1 (2020) and year 2 (2021). Column 4 and column 8 test for differences in parameters obtained in previous two columns. Outcome variables are listed on the left and described in detail in the pre-analysis plan. The unit of observation is the individual producer. All models control for the randomisation triplet fixed-effects and the baseline value of the outcome. Standard errors are in parentheses and are clustered at the village-level. Stars on the coefficient estimates reflect unadjusted *p*-values. Sharpened *q*-values controlling the false discovery rate across outcomes within each family are shown in brackets. * denotes significance at 10 pct.; ** at 5 pct.; and *** at 1 pct. level. Column 1 and column 5 display the control mean; standard deviation; and total number of observations across all groups in the estimation sample.

producers to sell cashews in multiple sales, given an expected increase in prices in the latter half of the trading season. We only find an increase in the number of sales among producers in the spillover group in 2021. The estimated spillover effect is positive, smaller than for the treatment group but not statistically significant after controlling for multiple hypothesis testing.

Treated producers do not exchange a higher quantity of cashew for rice relative to control producers, and the same is true for producers in the spillover group. The total quantity of cashew exchanged for rice is larger for both treated and spillover producers, more so for the latter, but the difference relative to the control group is only statistically significant in 2021. We find no treatment or spillover effects of the intervention on the share of the quantity sold over the quantity exchanged and sold.

6 Mechanisms

In this section, we shed some light on which mechanisms may be driving our results. First, we show some evidence consistent with our intervention having increased bargaining power of producers. Second, we test for *between*-spillovers to see whether the

our effects on prices may have been attenuated by the information affecting control producers, although we had not pre-specified this analysis. Third, we try to understand whether producers changed their market timing or producers' knowledge about other information related to the cashew market as a result of our messages. Finally, we explore what could account for the increase in the quantity sold and try to rule out whether our results may be explained by changes in outcomes where we would not expect to see any results *a priori* — a set of placebo outcomes.

6.1 Bargaining power

We find evidence that an increase in bargaining power can explain why prices increased. Four findings support this mechanism.

First, we find that producers, especially in 2021, report having been able to negotiate a higher price than the one they were originally offered (top panel, Table 4). We see that treatment producers are 7% more likely to report having gained a higher price than the one they originally were offered, though this effect is only significant at the 10% using naive *p*-values and we do not find differences between treatment and spillover producers in this variable. We find no evidence that the treatment induced producers to act as intermediary buyers. In our control group, 7% of producers report having bought cashews from other producers for resale. However, we find that producers in the spillover group reported receiving offers from more buyers relative to the control group in 2020. The difference between the treatment and spillover group is negative but not statistically significant after accounting for multiple-hypothesis testing. A possible explanation for the latter pattern may be that the service induced some traders to purchase cashew nuts from other producers in the same village, that were not as informed about market developments as the treated ones. On average the control group reported receiving offers from about 3 potential buyers. The spillover producers received offers from an extra 0.3 buyers (*q*-value: 0.01) relative to the control group, an increase of 10% of the control group mean.

Second, treated producers change the allocation of their sales to different buyers (middle panel, Table 4). Treated producers sell less to local buyers and more to itinerant buyers, especially in 2021. Treated producers sell, on average, 6 percentage points more of their output to itinerant buyers. Those type of buyers are more likely to self-finance their operations and have more room to negotiate with producers over a price. In 2020, we also see that treated producers sold 4 percentage points more of their output to

itinerant buyers, but this effect is not statistically significant.¹⁹

Third, we find that producers that receive our information change the source of information about market conditions. They move away from intermediaries as a source of information and they start to rely more on the messages that we have sent them through their mobile phones (bottom panel, Table 4). In 2020, treated producers report being 4-6 percentage points (q-value: 0.00) less likely to rely on intermediaries as a source of market advice. We interpret this switch in source of information alongside the change in the type of buyer as evidence that the messages changed producers' negotiation strategy. We had not pre-specified this family of outcomes and only recorded this data in the in-person follow-up, so we can only speculate that the change in this behaviour might have persisted also in 2021.

Fourth, we find that our positive effects are larger for producers with more bargaining power to begin with. In Figure 5 we plot heterogeneous treatment effects on our two focal outcomes, average price and quantity sold across the two years. We find suggestive evidence that our effects on prices are larger for producers with cashew plots that are in the top tercile the distribution (row 2, column 3, Figure 5). Producer with larger plots are those with larger amounts to sell, which can intuitively negotiate harder for deals than those with fewer bags of cashew to sell. While there is no clear difference in treatment effects or consistent pattern based on whether producers were able to read, their level of assets, or the distance with the closest sector market, or the number of other producers in the village.

¹⁹ We deviate slightly from of the analysis plan in reporting the shares sold to different buyers, as we opposed to the pre-specified indicator variables equal to one if the producer sold the majority of the stock to a specific type of buyer. In Appendix Table 17 we present our pre-specified outcomes within this family of variables, our conclusions remain qualitatively similar, though not robust to multiple hypothesis testing.

Table 4: Mechanisms - Increased bargaining power

	Year 1 (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treatment-Spillover	Year 2 (5) Mean (SD) Total obs.	(6) Treatment	(7) Spillover	(8) Treatment-Spillover
<i>Panel A: Bargaining power</i>								
Number of succesful bargains	0.14 (0.37) 1690	-0.00 (0.02) [0.89]	-0.02 (0.02) [0.44]	0.01 (0.02) [0.57]	0.46 (0.71) 1729	0.07* (0.04) [0.29]	0.07 (0.05) [0.41]	0.00 (0.05) [0.95]
1 if acted as intermediary	0.07 (0.26) 1713	0.01 (0.01) [0.89]	0.01 (0.02) [0.44]	-0.01 (0.02) [0.57]	0.06 (0.23) 1725	0.01 (0.01) [0.89]	0.02 (0.02) [0.41]	-0.01 (0.02) [0.95]
Number of potential buyers that made offers	2.91 (1.82) 1671	0.08 (0.09) [0.89]	0.30*** (0.10) [0.01]***	-0.21** (0.10) [0.12]	3.18 (2.02) 1703	0.01 (0.10) [0.89]	0.05 (0.11) [0.63]	-0.04 (0.12) [0.95]
<i>Panel B: Share of cashews sold to...</i>								
...a local buyer	0.47 (0.49) 1494	-0.02 (0.03) [0.47]	0.02 (0.03) [0.69]	-0.05 (0.03) [0.25]	0.57 (0.48) 1651	-0.05* (0.03) [0.08]*	-0.03 (0.03) [0.56]	-0.02 (0.03) [0.54]
...an itinerant buyer	0.47 (0.49) 1494	0.04 (0.03) [0.37]	-0.01 (0.03) [0.83]	0.04 (0.03) [0.25]	0.41 (0.48) 1651	0.06** (0.03) [0.08]*	0.03 (0.03) [0.56]	0.03 (0.03) [0.43]
...the wholesale market	0.05 (0.20) 1494	-0.03** (0.01) [0.06]*	-0.02 (0.01) [0.55]	-0.01 (0.01) [0.31]	0.01 (0.11) 1651	-0.01 (0.01) [0.42]	0.00 (0.01) [0.62]	-0.01 (0.01) [0.43]
<i>Panel C: Received market advice from...</i>								
...family and friends	0.49 (0.50) 1713	0.02 (0.03) [0.46]	-0.02 (0.03) [0.57]	0.04 (0.03) [0.20]				
...intermediaries	0.17 (0.37) 1713	-0.06*** (0.02) [0.00]***	-0.02 (0.02) [0.57]	-0.04** (0.02) [0.05]**				
...mobile messages	0.04 (0.19) 1713	0.15*** (0.02) [0.00]***	0.01 (0.01) [0.57]	0.14*** (0.02) [0.00]***				
...other sources	0.12 (0.33) 1713	-0.04*** (0.02) [0.01]***	-0.02 (0.02) [0.57]	-0.02 (0.01) [0.20]				

Notes: Producer-level intention-to-treat (ITT) estimates reported in columns 2 and 3 across year 1 (2020) and year 2 (2021). Column 4 and column 8 test for differences in parameters obtained in previous two columns. Outcome variables are listed on the left and described in detail in the pre-analysis plan. The unit of observation is the individual producer. All models control for the randomisation triplet fixed-effects and the baseline value of the outcome when it is available. Standard errors are in parentheses and are clustered at the village-level. Stars on the coefficient estimates reflect unadjusted *p*-values. Sharpened *q*-values controlling the false discovery rate across outcomes within each family are shown in brackets. * denotes significance at 10 pct.; ** at 5 pct.; and *** at 1 pct. level. Column 1 and column 5 display the control mean; standard deviation; and total number of observations across all groups in the estimation sample.

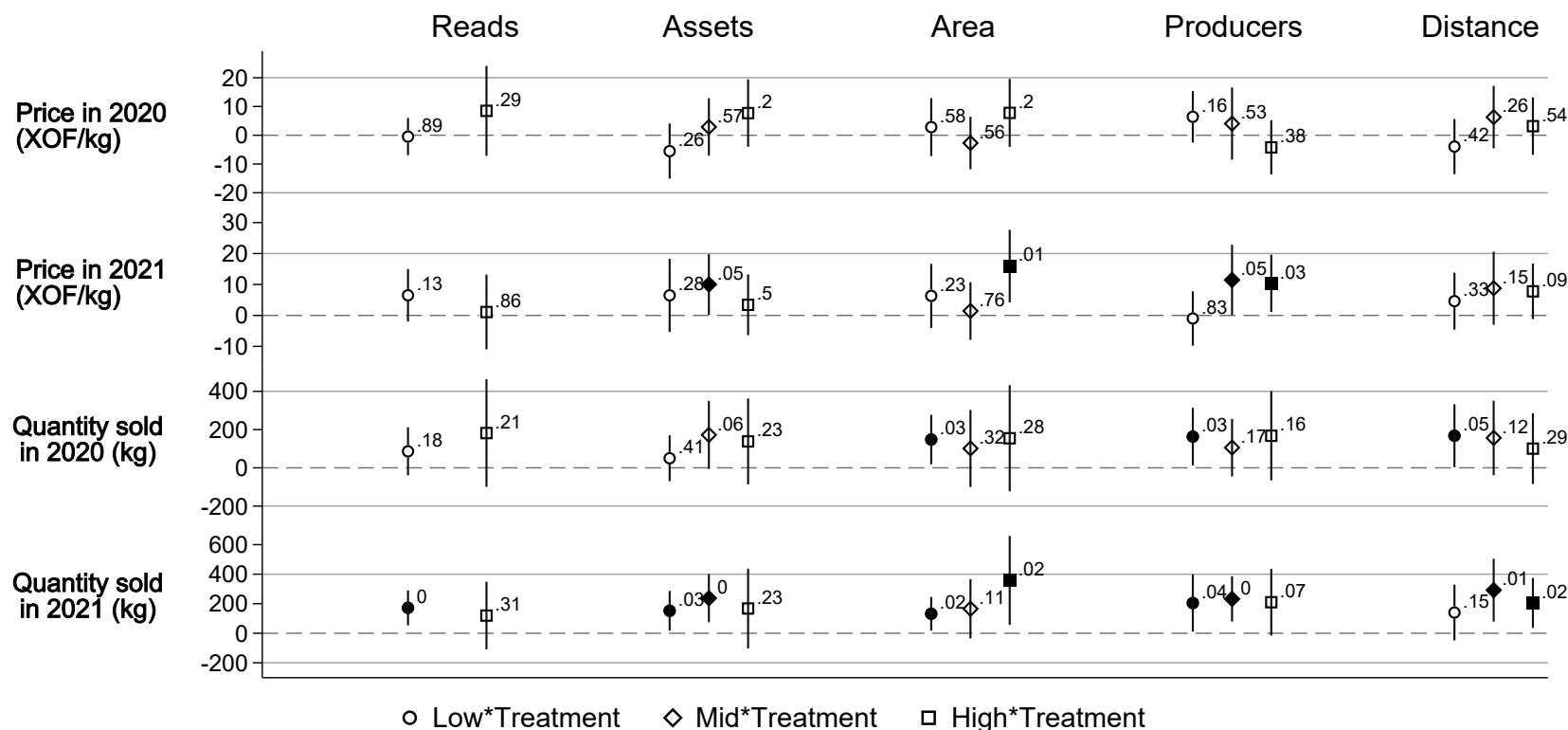


Figure 5: Treatment heterogeneity in price and quantity sold.^a

^a Source: In-person (2020) and phone-based follow-up (2021) surveys. Each cell represents coefficients from a separate OLS regression. For continuous dimensions of heterogeneity, we trichotomize the variable — interacting in each case our treatment indicator with (i) an indicator equal to 1 if the baseline value lies at or below the lower tercile, (ii) an indicator equal to 1 if the baseline value lies strictly between the two terciles, and (iii) an indicator equal to 1 if the baseline values lies at or above the upper tercile, following [Bari et al., Faisal and Malik et al., Kashif and Meki et al., Muhammad and Quinn et al., Simon \(2021\)](#). For the first column (Reads), we interact treatment with an indicator variable equal to one if the respondent could read a sentence in Bissau-Guinean Kriol. Assets heterogeneity is based on the respondent's principal component of assets ([Filmer and Pritchett, 2001](#)). Area heterogeneity is based on the respondent's size of the cashew tree plot (in hectares). Producers heterogeneity is based on the number of producers in the respondent's village. Distance heterogeneity is based on the road distance to the nearest sector capital (in km). All regressions control for randomisation triplet fixed effects and indicators for the middle and upper tercile of the heterogeneity dimension. Standard errors are clustered at the village-level. Bars represent 95% confidence interval based on standard p -values. Filled markers imply statistical significance at the 5% level relative to the control group in the bottom tercile of the heterogeneity dimension, the omitted category.

6.2 Between-cluster spillovers

Could spillovers have occurred across villages? Our sampling design had explicitly tried to minimise the possibility that spillovers may occur across villages, by maximising the minimum distance across sampled villages. However, as suggested by [Soldani et al. \(2023\)](#) and [Falcao Bergquist et al. \(2021\)](#), it is plausible that market information may flow across closely connected villages and also affect how itinerant traders negotiate their prices as they move across proximate villages. We test for the presence of *between-village* spillovers by testing whether the number of treated individuals within a radius of 5km, conditional on the number of villages in our sample within that radius, significantly changes our outcomes of interest beyond the producer-level treatment assignment. We describe this specification in Appendix Section C.6, which is similar to the specification used in [Miguel and Kremer \(2004\)](#). We had not pre-specified this analysis because we had designed our intervention to minimise *between-village* spillovers, but we decide to explore it as a potential mechanism to understand our results.

Identification relies on our village-level randomisation, since the number of treated producers within a 5km radius should be plausibly exogenous after controlling for the number of villages in our study within that radius. To illustrate how treatment intensity varies across our sample, Appendix Figure 8 shows the spatial distribution of villages and plots a 5km buffer around each village in our sample: 40% of villages have another sampled village within 5km, 9% of villages have two sampled villages, 1% had three sampled villages, and the remaining 50% had none. 27% of villages have another treated village within a 5km distance, 8% of villages have two treated villages, and the remaining 65% had none.

In Figure 6 we report estimates of our test for *between-village* spillovers. We again focus on our focal outcomes: average price earned by producers and the total quantity sold for cash. Prices in more intensely treated areas are higher than less intensely treated ones. In 2020, our estimate of the effect of an additional treated producer within a 5km is very close to our direct estimate of treatment effects, and is statistically significant at the 10% level. The magnitude of these spillovers on prices corresponds to a 0.7% increase for every treated producer within a radius of 5km. These spillovers are positively affecting producers in control villages. When we estimate the interaction between the number of treated producers within 5km and the indicators for whether producers are in either the treatment or spillover group are negative, we find a positive effect of about 1% of the control mean find that is statistically significant and correspond. Instead, the direct

effects in treated villages that are not close to other treated villages are positive and statistically significant at the 10% level. Producers in control villages that neighbour treatment villages report an increase of about 4 XOF (1.3% of the control group mean). Assuming a linear relationship between the number of treated individuals within 5km and the price, these results imply an increase of around 4-8% in control villages that were more intensely treated. These *between-village* could plausibly explain why we failed to detect larger increases in average prices in treated villages, especially in 2020. Our estimates of *between-village* effects in 2021 are smaller than in 2020 and not statistically significant, but going in the same direction as the previous year. It is plausible that since in 2021 all producers in the study had heard about the service, and some of the control group producers had also subscribed, would attenuate these indirect effects. Finally, in the bottom half of Figure 6, we do not find effects of treatment intensity on the quantity sold.

Our findings are consistent with buyers not being able to distinguish between treated and untreated producers and hence adapting their behavior towards all producers, for instance by making better price offers across villages that are close to each other.²⁰ Fafchamps and Minten (2012) described this mechanism potentially having a role in interventions that reduce asymmetric information between buyers and sellers of agricultural commodities.

²⁰ In line with this hypothesis, in Appendix Table 27, we found that control villages reported receiving more itinerant traders than treated villages.

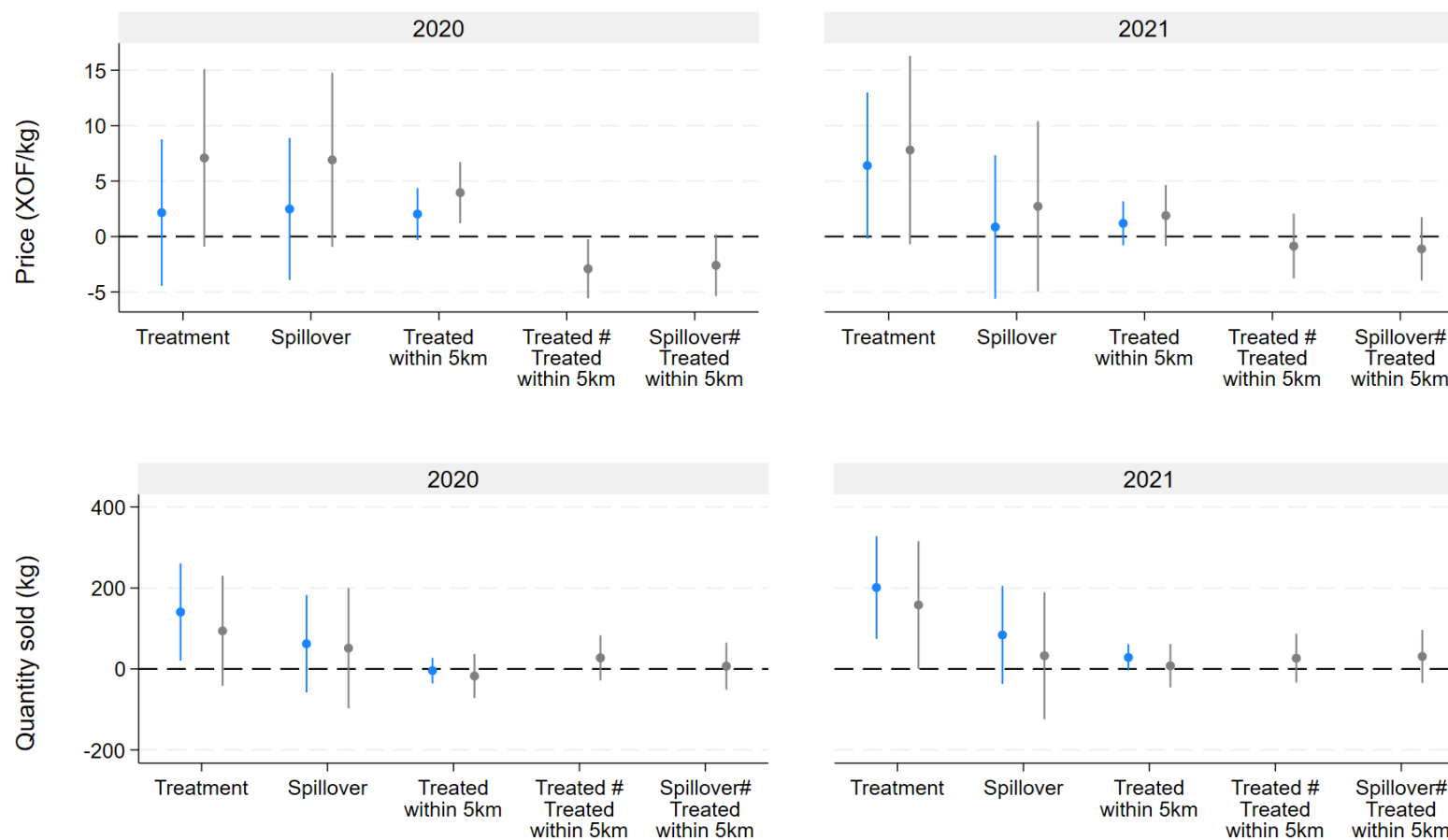


Figure 6: Spatial spillovers on prices and quantity sold.^a

^a Source: In-person (2020) and phone-based follow-up (2021) surveys. Each sub-plot represents coefficients from two separate OLS regression where the unit of observation is the producer. Regressions control for randomisation tripled fixed effects, the number of villages within a 5km radius that are in our sample, and the baseline value of the outcomes. The radius of 5km was selected after running a series of nested models as in Egger et al. (2022), selecting the model that minimised the Bayesian Information Criterion across all models. Bars represent 95% confidence interval based on Conley (1999) standard errors, accounting for spatial correlation within a 5km radius.

6.3 Market timing

A mechanism that could explain our results is that producers may have tried to change the timing of their sales in order to sell when prices were highest. Instead, we find that treated producers were more likely to sell during the first half of the trading season (in or before April/May) relative to the control group (Table 5). This period coincided with the time when we initially started sending messages to treated producers, though the messages advised producers to wait to sell, as prices were expected to increase. In 2020, treated producers are 4 percentage points more likely to sell in April relative to the control group, though this effect is not robust to multiple hypothesis testing. Whereas in 2021, they increased the probability of recording a sale in April or May by 7 percentage points (q-value: 0.02-0.03). In 2020, treated producers were 3 percentage points less likely to report exchanging cashew for rice towards at beginning of trading season and more likely to do so in July or later (when the terms of exchange had improved). Qualitatively, we see a similar pattern in 2021, though it is not statistically significant and effect sizes are smaller. The difference in timing of sales is consistent with treated producers selling more than once during the trading season.

Producers do not sell more frequently in high price periods, but they do make higher prices in those periods in 2021. To convey this finding descriptively, Figure 7 plots the interaction between our treatment indicator and an indicator for the month of sales, running the regression at the transaction-level. While there is no difference in the average effect by month of sales in 2020, we see that prices for treated producers relative to control producers are almost 20 XOF/kg higher in June 2021, which was the month when prices peaked. In bottom right quadrant, we also see that treated producers sold more on the intensive margin in June 2021 relative to control producers, though this effect is not statistically significant.

To understand what motivated producers to sell more than once, we used an open-ended question to elicit their reasons for adopting this sale strategy. In the first three rows of Appendix Table 18, we report the producers' responses by treatment status. We had not pre-specified the analysis of the outcomes in this table, which we only collected during the in-person survey. Treated producers were 3 percentage points more likely to directly refer to the market information system as a reason for selling more than once. Treated producers were also 7 percentage points more likely to say they sold more than once because they thought that prices would increase in the future and because they thought this strategy would reduce their risk of price uncertainty.

This reasoning is consistent with the information delivered by the market information system, which suggested that prices would increase. Moreover, we see that treated producers are also 5 percentage points more likely to sell more than once because they report that this enabled them to smooth consumption (i.e. to avoid spending all the revenue at once or because they lacked liquidity). In the second panel of the same table, we also see that treated producers report being 5-8 percentage points more likely to have sought advice on market timing, significantly so relative to the spillover group.

6.4 Alternative behavioural mechanisms

We do not find evidence consistent with several other behavioral mechanisms. Firstly, we find no evidence that treated producers changed their beliefs about the market (Appendix Table 19). Producers, on average, expect high prices for the upcoming trading season. Treated producers had lower and more realistic expectations about their sale price. However, these differences were not statistically significant or large. Secondly, we did not observe an increase in information sharing among producers as a result of the intervention. Specifically, we did not find that treated producers were more likely to share or receive offers. Thirdly, we investigated various outcomes where we did not anticipate any effects (Appendix Table 20). In 2020, we found no effects on those particular outcomes, such as producers being more likely to record their sales using a transaction diary. However, we did observe a slight increase in the number of treated producers reporting greater trust, which may be a secondary effect of the intervention for the producers who benefited. Fourthly, we provide evidence from transaction diaries and interviews conducted with a subset of intermediaries (Appendix Section F and G), which aligns qualitatively with the self-reported data.

Additionally, we explored alternative uses of cashews to determine whether producers used fewer cashews for other purposes. While we found lower amounts of cashews used by treated producers to repay loans (Appendix Table 21) or for small exchanges, these reductions were not significant enough to fully explain the increases in quantity sold (Appendix Table 22). Furthermore, in our follow-up survey conducted via phone, we examined whether the quantity increases were due to changes in production decisions made by producers but found no effects on the behavioral measures we collected. We remain cautious in interpreting our effects on the quantity sold, but we did observe a positive correlation over time between prices and quantity sold in the market (Appendix Table 23).

Table 5: Mechanisms — Changes in timing of sales

	Year 1				Year 2			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Mean	Treatment	Spillover	Treatment-Spillover	Mean	Treatment	Spillover	Treatment-Spillover
	(SD)				(SD)			
	Total obs.				Total obs.			
1 if sold cashews in April or before	0.15 (0.35) 1361	0.04** (0.02) [0.17]	0.02 (0.02) [0.80]	0.02 (0.03) [0.99]	0.17 (0.38) 1477	0.07*** (0.02) [0.02]**	0.05* (0.03) [0.25]	0.03 (0.03) [0.89]
1 if sold cashews in May	0.51 (0.50) 1361	0.02 (0.03) [0.71]	-0.01 (0.04) [0.88]	0.03 (0.03) [0.99]	0.49 (0.50) 1477	0.07*** (0.03) [0.03]**	0.06* (0.03) [0.25]	0.02 (0.03) [0.89]
1 if sold cashews in June	0.49 (0.50) 1361	0.05 (0.03) [0.29]	0.05 (0.03) [0.33]	-0.00 (0.03) [0.99]	0.64 (0.48) 1477	-0.02 (0.03) [0.84]	-0.03 (0.03) [0.84]	0.01 (0.03) [0.89]
1 if sold cashews in July or later	0.07 (0.25) 1361	0.01 (0.02) [0.71]	0.01 (0.02) [0.81]	0.00 (0.02) [0.99]	0.12 (0.33) 1477	0.02 (0.02) [0.74]	0.00 (0.02) [0.84]	0.02 (0.02) [0.89]
1 if exchanged rice in April or before	0.09 (0.28) 1591	-0.03* (0.01) [0.17]	-0.03* (0.02) [0.21]	0.00 (0.02) [0.99]	0.10 (0.29) 1662	-0.01 (0.02) [0.84]	-0.00 (0.02) [0.84]	-0.00 (0.02) [0.93]
1 if exchanged rice in May	0.23 (0.42) 1591	-0.01 (0.02) [0.81]	-0.00 (0.03) [0.90]	-0.00 (0.02) [0.99]	0.22 (0.42) 1662	0.00 (0.03) [0.95]	0.02 (0.03) [0.84]	-0.01 (0.03) [0.89]
1 if exchanged rice in June	0.26 (0.44) 1591	-0.01 (0.03) [0.76]	-0.02 (0.03) [0.80]	0.01 (0.02) [0.99]	0.27 (0.44) 1662	-0.01 (0.02) [0.84]	-0.02 (0.03) [0.84]	0.01 (0.03) [0.89]
1 if exchanged rice in July or later	0.01 (0.11) 1591	0.03*** (0.01) [0.01]**	0.03*** (0.01) [0.05]*	0.00 (0.01) [0.99]	0.05 (0.21) 1662	0.01 (0.01) [0.84]	0.01 (0.01) [0.84]	0.00 (0.01) [0.89]

Notes: Producer-level intention-to-treat (ITT) estimates reported in columns 2 and 3 across year 1 (2020) and year 2 (2021). Columns 4 tests for differences in parameters obtained in previous two columns. Outcome variables are listed on the left and described in detail in the pre-analysis plan. The unit of observation is the individual producer. All models control for the randomisation triplet fixed-effects and the baseline value of the outcome when it was available. Standard errors are in parentheses and are clustered at the village-level. Stars on the coefficient estimates reflect unadjusted p -values. Sharpened q -values controlling the false discovery rate across outcomes within each family are shown in brackets. * denotes significance at 10 pct.; ** at 5 pct.; and *** at 1 pct. level. Column 1 and column 5 display the control mean; standard deviation; and total number of observations across all groups in the estimation sample.

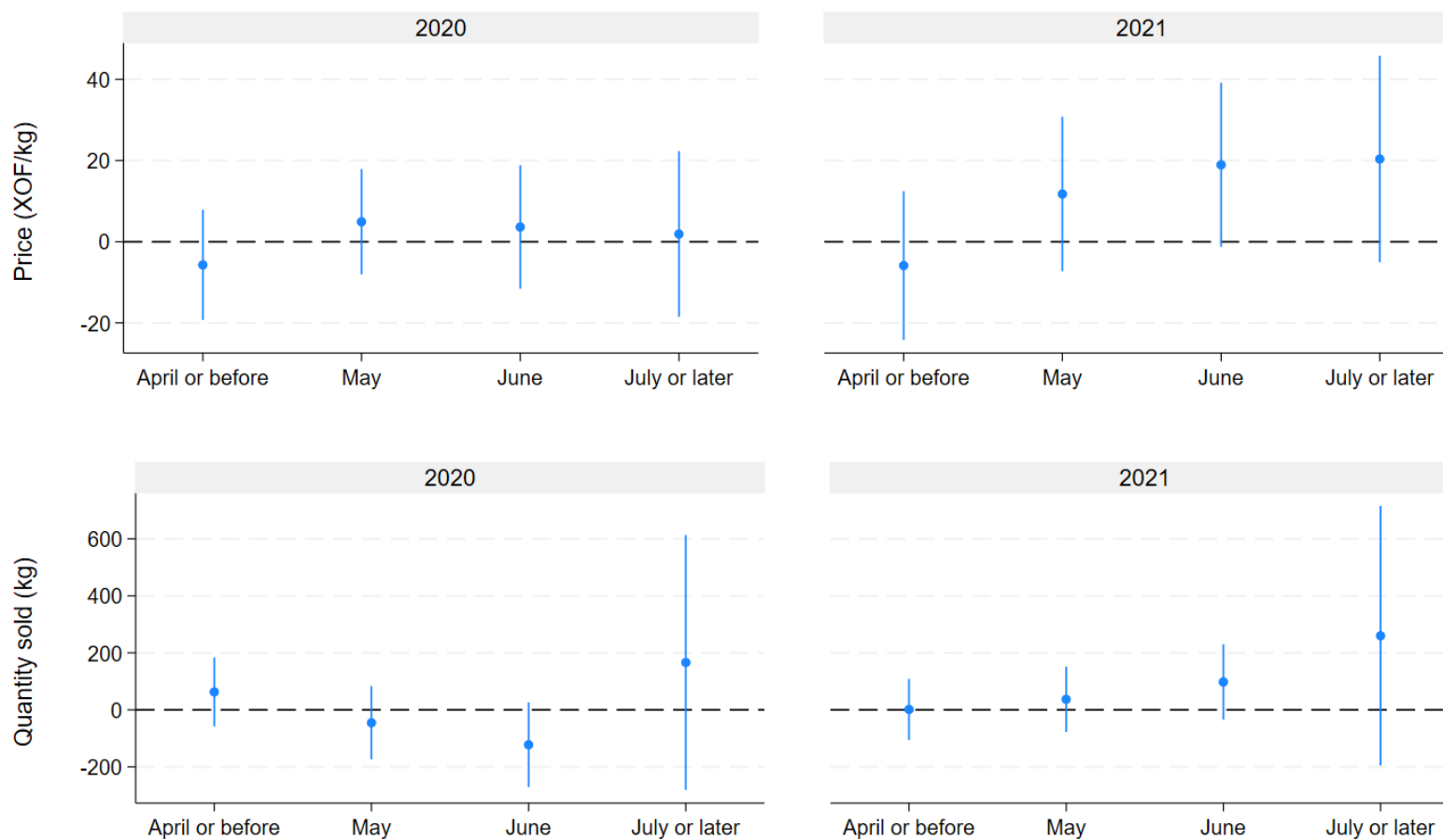


Figure 7: Treatment heterogeneity in price and quantity sold.^a

^a Source: In-person (2020) and phone-based follow-up (2021) surveys. Each sub-plot represents coefficients from a separate OLS regression where the unit of observation is a transaction. All regressions control for randomisation triplet fixed effects. Bars represent 95% confidence intervals.

7 Conclusion

This paper estimated the effects of introducing a new market information system among cashew producers in Guinea-Bissau. The market information system provided free weekly text and voice-messages during the 2020 and 2021 trading season to treated producers. The information sent to producers contained up-to-date farmgate prices, market news, and marketing advice on when to sell. On the whole, we see that treated producers benefited from these messages and were able to negotiate higher prices on average. These affects are larger for producers that had more output to sell. We find that *between*-village spillovers of the intervention could attenuate the intention-to-treat effects on average prices among treated producers relative to control producers. We speculate that these spillovers may occur through itinerant traders changing their negotiation strategy, as in (Soldani et al., 2023).

Our findings yield several policy implications. First, we found that the increased revenue among treated producers is substantial in magnitude. Providing up-to-date and reliable information on market conditions seems to improve the market outcomes of producers in this context. Subscribers to the service could quickly repay the user-fee if they earned similar additional returns as those we observed among treated producers. Second, the baseline government policy of providing a reference price only at the beginning of the trading season can be improved upon, through the communication of more frequent price updates.

There are several open questions that remain unanswered. As the service continues to be rolled-out and its user-base grows, the effects we estimated may change, as more producers become better informed about the market dynamics. We plan to explore some of these issues in future research.

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Appendix

This appendix has nine sections. In Section [A](#) we provide details of the sampling and randomisation procedures we implemented. In Section [B](#) we provide balance tables, measures of compliance and attrition. Section [C](#) shows robustness of our main results to alternative econometric specifications. It also illustrates effects of the intervention on additional outcomes not reported in the main text. In Section [D](#), we provide additional details on the cashew nut production and commercialisation. In Section [F](#) and Section [G](#) we show complementary results based on a sub-sample of producers that kept written records of their transactions and from intermediaries based in a sub-sample of villages.

A Sampling and randomisation

A.1 Village-level sampling

Our sampling strategy has two main goals: (i) making the treatment and control groups statistically balanced across a set of baseline characteristics, and (ii) minimising spillovers of the treatment to untreated villages. There is a trade-off between these two goals: minimizing spillovers requires that treatment and control groups be sufficiently far apart geographically; while statistical balance requires that treatment and control groups be similar to each other, which in turn often requires geographical proximity (Duflo et al., 2008). We address this trade-off as follows.

In order to select villages that would be part of our study, we created a grid containing $2.7km^2$ cells, covering the entirety of the country. We sample 290 villages from about 1,800 villages, using geo-coded census data, such that one village is chosen from each of those cells.²¹ We compute for each village the closest distance to another sampled village. We store the value of the minimum distance in the sample of villages drawn. We iterate these steps 999 times and choose the sample that has the largest minimum distance.

We excluded from our sampling frame villages that have the following characteristics:

- with less than 10 households according to the 2009 census (to allow a sufficiently large sample of cashew producers in every village),
- located on the islands (due to budget constraints),
- located in a circle of 15km in radius in the region of Tombali, which are closest to the river estuary (due to budget constraints, as reaching those villages is logistically difficult),
- located in the sector of Boé, in the East of the country (due to budget constraints and poor phone signal in the area),
- for which we did not have reliable GPS and population data (so those unmatched from the fuzzy merging of the census and the GPS data),
- located in Biombo, the region to the West of the capital Bissau (as it was used for the pilot).

These exclusions account for 40% of producers in the country, mostly driven by exclusion of the smaller villages. We posit that since most of the villages that were not included in the sampling frame are more remote and smaller, producers living in those excluded villages are likely to be most affected by lack of information and could benefit more from the intervention. Once the final sample of villages was selected, we sought the contact details of the village leaders of our final sample of villages. Whenever possible, we contacted the village leaders ahead of the data-collection baseline visit to inform them of the study and seek their collaboration.

A.2 Producer-level sampling

After the data-collection team reached the sampled village and the village leaders granted them permission to work, they asked the village leader for a list of all producers living in the village owning a cashew plantation. In eliciting this list, the data-collection team stressed that every producer with a cashew plantation should be included, including small ones. On the day of the visit, the data-collection team used a random number generator to sample 7 producers from this list.

²¹ We constructed our sampling frame by merging the 2009 census data, the latest census available to-date, with GPS coordinates obtained from a geocoded administrative dataset maintained by the United Nations Office for the Coordination of Humanitarian Affairs.

A.3 Village-level randomisation

We follow the recommendations of [Bruhn and McKenzie \(2009\)](#), stratifying our randomisation to increase efficiency. Randomisation of treatment across villages was implemented constructing, in each region, triplets of villages that are as similar as possible along a number of dimensions that are likely to affect the impact of treatment, and that were available to use. We combine the population data from the 2009 census and other geo-coded databases to construct the following variables:

- (i). road distance to the nearest sectoral capital (to proxy the closest location where small intermediaries are based)
- (ii). road distance to the nearest regional capital (to proxy the closest location where wholesale intermediaries are based)
- (iii). road distance to the port in Bissau (to proxy for distance from the main export route, and inversely from distance to the closest borders)
- (iv). linear distance to the nearest (MTN) mobile network tower, and
- (v). number of households for each village, according to the 2009 census.

Because these characteristics are likely to be correlated, we use the Mahalanobis distance as a metric of similarity across villages that takes into account of the correlation across these characteristics. The Mahalanobis distance between them is then defined as:

$$||z_l - z_j|| = ((z_l - z_j)'S^{-1}(z_l - z_j))^{1/2} \quad (5)$$

where z_l and z_j denote the vector of relevant characteristics from villages l and j , respectively, and S is the covariance matrix of characteristics z . Pairs of villages with a smaller Mahalanobis distance are more similar along these dimensions. Since these characteristics are weighted by the inverse of the covariance matrix S , correlation between characteristics — e.g. between our various distance measures — is given less weight.

We select, within each region, the allocation of villages into triplets that minimizes the sum, over all triplets, of the Mahalanobis distances within each triplet. The search is conducted using an algorithm that randomly tries different combinations of villages into triplets. Within each triplet, one village was then randomly assigned to control and two villages to treatment. This ratio of treated to control villages yields a similar number of individual producers that are either treated, spillover, or pure control, in each triplet.

A.4 Within-village randomisation

Once the 7 randomly sampled producers in treatment villages completed the baseline interview, an on-the-spot within-village lottery determined producer-level assignment to either the treatment or spillover groups. The supervisor of the data-collection team administered the lottery. Each producer drew one of two kinds of goodies from a bag. Using this lottery, 4 producers were assigned to the treatment group and 3 to the spillover one. After the producer-level randomisation, the treated producers took part in a training session about the intervention and the determinants of farmgate raw cashew-nut prices.

B Experimental integrity

B.1 Balance

Table 6: Baseline balance — Primary outcome variables

	Control (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover	(5) Max pairwise st. diff.
Price per sale per kg	395.89 (96.24) 1968	-3.66 (5.51) [0.74]	1.35 (5.60) [0.81]	-5.01 (4.75) [0.50]	0.05 1968
Value of all sales and exchanges	527100.44 (921775.27) 1978	35669.54 (44414.13) [0.74]	24404.24 (43399.62) [0.80]	11265.30 (29831.03) [0.88]	0.03 1978
Value of all sales	486696.38 (912168.16) 1984	14815.85 (43992.64) [0.74]	15806.13 (43245.56) [0.80]	-990.28 (28614.91) [0.97]	0.01 1984
Value of exchanges	39662.45 (85677.82) 1982	21556.26*** (6797.60) [0.01]**	10321.69* (6024.35) [0.46]	11234.56** (5681.82) [0.22]	0.22 1982
Total quantity sold	1206.35 (1855.65) 1984	54.39 (96.73) [0.74]	36.52 (94.05) [0.80]	17.87 (66.13) [0.88]	0.01 1984
Number of sales	1.62 (0.81) 1988	0.01 (0.04) [0.74]	0.07 (0.05) [0.46]	-0.05 (0.04) [0.50]	0.08 1988
1 if exchanged cashew for rice	0.30 (0.46) 1985	0.04 (0.03) [0.30]	0.02 (0.03) [0.80]	0.02 (0.02) [0.50]	0.15 1985
Total quantity exchanged	113.66 (247.43) 1982	57.26*** (19.08) [0.01]**	25.75 (17.06) [0.46]	31.51** (15.94) [0.22]	0.21 1982
Share of quantity sold over quantity exchanged and sold	0.90 (0.20) 1979	-0.02 (0.01) [0.30]	-0.01 (0.01) [0.80]	-0.02 (0.01) [0.44]	0.13 1979

Notes: Coefficient of treatment and spillover at baseline (columns 2-3). Column 4 tests for differences in parameters obtained in previous two columns. Column 5 reports the standardised pairwise maximum difference between mean across all study groups. Outcome variables are listed on the left and described in detail in the pre-analysis plan. The unit of observation is the individual producer. All models control for the randomisation triplet fixed-effects. Standard errors are in parentheses and are clustered at the village-level. Stars on the coefficient estimates reflect unadjusted p -values. q -values reported in brackets. * denotes significance at 10 pct.; ** at 5 pct.; and *** at 1 pct. level. Column 1 displays the control mean; standard deviation; and total number of observations.

Table 7: Baseline balance — Producer characteristics

	Control (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover	(5) Max pairwise st. diff.
1 if female	0.05 (0.23) 1985	0.00 (0.01) [0.95]	-0.00 (0.01) [1.00]	0.00 (0.01) [0.97]	0.02 1985
Age	43.47 (14.96) 1988	-1.06 (0.79) [0.37]	-1.11 (0.77) [0.60]	0.05 (0.75) [0.97]	0.06 1988
Household size	13.40 (7.88) 1988	0.57 (0.54) [0.45]	1.02* (0.57) [0.38]	-0.45 (0.41) [0.92]	0.08 1988
Years of education	3.89 (3.77) 1988	0.01 (0.23) [0.95]	-0.10 (0.23) [1.00]	0.11 (0.18) [0.92]	0.04 1988
1 if Kriol is the most spoken language at home	0.21 (0.41) 1988	-0.01 (0.02) [0.87]	-0.01 (0.02) [1.00]	0.00 (0.02) [0.97]	0.05 1988
1 if cashew is the main source of income	0.79 (0.41) 1988	0.02 (0.02) [0.45]	0.00 (0.02) [1.00]	0.02 (0.02) [0.92]	0.06 1988
1 if faces storage limitations	0.27 (0.45) 1988	0.06** (0.03) [0.08]*	0.02 (0.02) [0.99]	0.04* (0.02) [0.92]	0.15 1988
Minimum age of trees (years)	3.32 (3.43) 1966	-0.06 (0.17) [0.87]	0.08 (0.20) [1.00]	-0.14 (0.20) [0.92]	0.06 1966
Max age of trees (years)	17.23 (8.41) 1868	-0.20 (0.47) [0.87]	0.04 (0.48) [1.00]	-0.25 (0.48) [0.93]	0.04 1868
1 if trees were plagued	0.69 (0.46) 1980	-0.03 (0.03) [0.37]	-0.01 (0.03) [1.00]	-0.03 (0.03) [0.92]	0.08 1980
1 if sells other crops	0.65 (0.48) 1988	0.09*** (0.02) [0.00]***	0.10*** (0.02) [0.00]***	-0.01 (0.02) [0.97]	0.21 1988
Index of trust (Anderson, 2008)	0.00 (1.00) 1988	0.07 (0.05) [0.29]	0.07 (0.05) [0.60]	0.00 (0.05) [0.97]	0.06 1988
Index of food security(Anderson, 2008)	0.00 (1.00) 1983	-0.02 (0.05) [0.87]	0.01 (0.06) [1.00]	-0.03 (0.05) [0.92]	0.03 1983
1 if has a fixed buyer	0.30 (0.46) 1987	0.05* (0.03) [0.23]	0.03 (0.03) [0.83]	0.02 (0.03) [0.92]	0.10 1987
Area plantation (hectares)	3.26 (2.29) 1988	0.28** (0.13) [0.19]	0.27* (0.14) [0.38]	0.01 (0.15) [0.97]	0.11 1988
Years of experience in cashew sector	10.17 (7.05) 1988	0.65* (0.39) [0.23]	0.25 (0.38) [1.00]	0.41 (0.38) [0.92]	0.08 1988
Standardised principal component of wealth (Filmer and Pritchett, 2001)	-0.19 (1.63) 1979	0.30*** (0.10) [0.03]**	0.37*** (0.10) [0.00]***	-0.07 (0.09) [0.92]	0.20 1979
Index of numeracy (Anderson, 2008)	0.00 (1.00) 1988	0.10* (0.06) [0.23]	0.02 (0.06) [1.00]	0.08 (0.05) [0.92]	0.11 1988
1 if can read	0.29 (0.45) 1988	0.00 (0.03) [0.95]	0.00 (0.03) [1.00]	-0.00 (0.02) [0.97]	0.02 1988
1 if present-biased	0.17 (0.37) 1985	0.03* (0.02) [0.23]	0.00 (0.02) [1.00]	0.03 (0.02) [0.92]	0.07 1985

Notes: Coefficient of treatment and spillover at baseline (columns 2-3). Column 4 tests for differences in parameters obtained in previous two columns. Column 5 reports the standardised pairwise maximum difference between mean across all study groups. Outcome variables are listed on the left and described in detail in the pre-analysis plan. The unit of observation is the individual producer. All models control for the randomisation triplet fixed-effects. Standard errors are in parentheses and are clustered at the village-level. Stars on the coefficient estimates reflect unadjusted p -values. q -values reported in brackets. * denotes significance at 10 pct.; ** at 5 pct.; and *** at 1 pct. level. Column 1 displays the control mean; standard deviation; and total number of observations.

Table 8: Balance — Village characteristics

	Control (1) Mean (SD) Total obs.	(2) Treatment
Number of cashew producers in the village	33.87 (23.49) 288	-0.67 (2.83) [0.81]
Number of cashew buyers in the village	2.90 (2.58) 287	1.14*** (0.41) [0.05]*
Road distance in km to nearest sector capital	25.59 (23.70) 290	1.02 (2.65) [0.81]
Road distance in km to nearest region capital	51.48 (33.51) 290	-0.85 (2.96) [0.81]
Road distance in km to the capital	162.30 (69.26) 290	4.15 (4.60) [0.59]
Distance in km to the nearest primary road	20.16 (17.17) 290	2.35 (1.60) [0.38]
Distance in km to the nearest secondary road	3.19 (2.85) 290	0.53 (0.32) [0.38]
Distance in km to the nearest road	2.74 (2.40) 290	0.30 (0.28) [0.58]

Notes: Coefficient of village-level treatment at baseline (columns 2). Outcome variables are listed on the left and described in detail in the pre-analysis plan. The unit of observation is the village. All models control for the randomisation triplet fixed-effects. Standard errors are in parentheses and are robust. Stars on the coefficient estimates reflect unadjusted p -values. q -values reported in brackets. * denotes significance at 10 pct.; ** at 5 pct.; and *** at 1 pct. level. Column 1 displays the control mean; standard deviation; and total number of observations.

B.2 Attrition

Table 9: Attrition rates at either follow-up

	(1) Control Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover	(5) Max pairwise st. diff.
<i>1 if attrited at...</i>					
...the in-person follow-up	0.13 (0.33) 1988	0.01 (0.02) [0.81]	0.00 (0.02) [0.88]	0.00 (0.02) [0.92]	0.05 1988
...the phone-based follow-up	0.12 (0.33) 1988	0.00 (0.02) [0.81]	-0.01 (0.02) [0.88]	0.01 (0.02) [0.92]	0.03 1988
...either follow-up	0.22 (0.42) 1988	0.01 (0.02) [0.81]	0.00 (0.02) [0.88]	0.00 (0.02) [0.92]	0.04 1988

Notes: Coefficient of treatment and spillover at baseline (columns 2-3). Column 3 tests for differences in parameters obtained in previous two columns. Column 4 reports the standardised pairwise maximum difference between mean across all study groups. Outcome variables are listed on the left and described in detail in the pre-analysis plan. The unit of observation is the individual producer. All models control for the randomisation triplet fixed-effects. Standard errors are in parentheses and are clustered at the village-level. Stars on the coefficient estimates reflect unadjusted p -values. q -values reported in brackets. * denotes significance at 10 pct.; ** at 5 pct.; and *** at 1 pct. level. Column 1 displays the control mean; standard deviation; and total number of observations.

B.3 Compliance

Table 10: Compliance — Take-up of the intervention

	Year 1 (1) Mean (SD) Total obs.	(2)	(3)	(4)	Year 2 (5) Mean (SD) Total obs.	(6)	(7)	(8)
		Treatment	Spillover	Treatment-Spillover		Treatment	Spillover	Treatment-Spillover
<i>Self-reported data:</i>								
1 if heard of mobile MIS	0.14 (0.35) 1714	0.30*** (0.02) [0.00]***	0.10*** (0.03) [0.00]***	0.20*** (0.03) [0.00]***	0.68 (0.47) 1724	0.12*** (0.03) [0.00]***	0.04 (0.03) [0.13]	0.08*** (0.02) [0.00]***
1 if used mobile MIS	0.02 (0.16) 1714	0.25*** (0.02) [0.00]***	0.03 (0.02) [0.10]	0.22*** (0.02) [0.00]***	0.43 (0.49) 1724	0.18*** (0.03) [0.00]***	0.05* (0.03) [0.13]	0.13*** (0.03) [0.00]***
<i>Administrative data:</i>								
Take-up rate	0.00 (0.00) 1988	0.53*** (0.02) [0.00]***	0.00 (0.01) [0.86]	0.53*** (0.02) [0.00]***	0.08 (0.27) 1988	0.28*** (0.02) [0.00]***	0.01 (0.02) [0.52]	0.27*** (0.02) [0.00]***

Notes: Producer-level intention-to-treat (ITT) estimates reported in columns 2 and 3 across year 1 (2020) and year 2 (2021). Columns 4 and 8 test for differences in parameters obtained in previous two columns. The top panel presents self-reported measures of engagement and take-up of the mobile market information system. The bottom panel presents a measure of take-up derived from administrative data from the mobile partner operator, MTN, that managed the technology of the service. The measure of take-up is equal to one if at least a third of the messages sent reached the respondent, where we define "reached" as an SMS being received or the robocall being listened for at least 45 seconds. In 2020, we sent 24 messages, so we codetake-up equal to 1 if respondents were reached by eight messages. In 2021, we sent 15 messages, so we code take-up equal to one if the respondents were reached by five messages. The unit of observation is the individual producer. All models control for the randomisation triplet fixed-effects and the baseline value of the outcome when it was available. Standard errors are in parentheses and are clustered at the village-level. Stars on the coefficient estimates reflect unadjusted p -values. Sharpened q -values controlling the false discovery rate across outcomes within each family are shown in brackets. * denotes significance at 10 pct.; ** at 5 pct.; and *** at 1 pct. level. Column 1 and column 5 display the control mean; standard deviation; and total number of observations across all groups in the estimation sample.

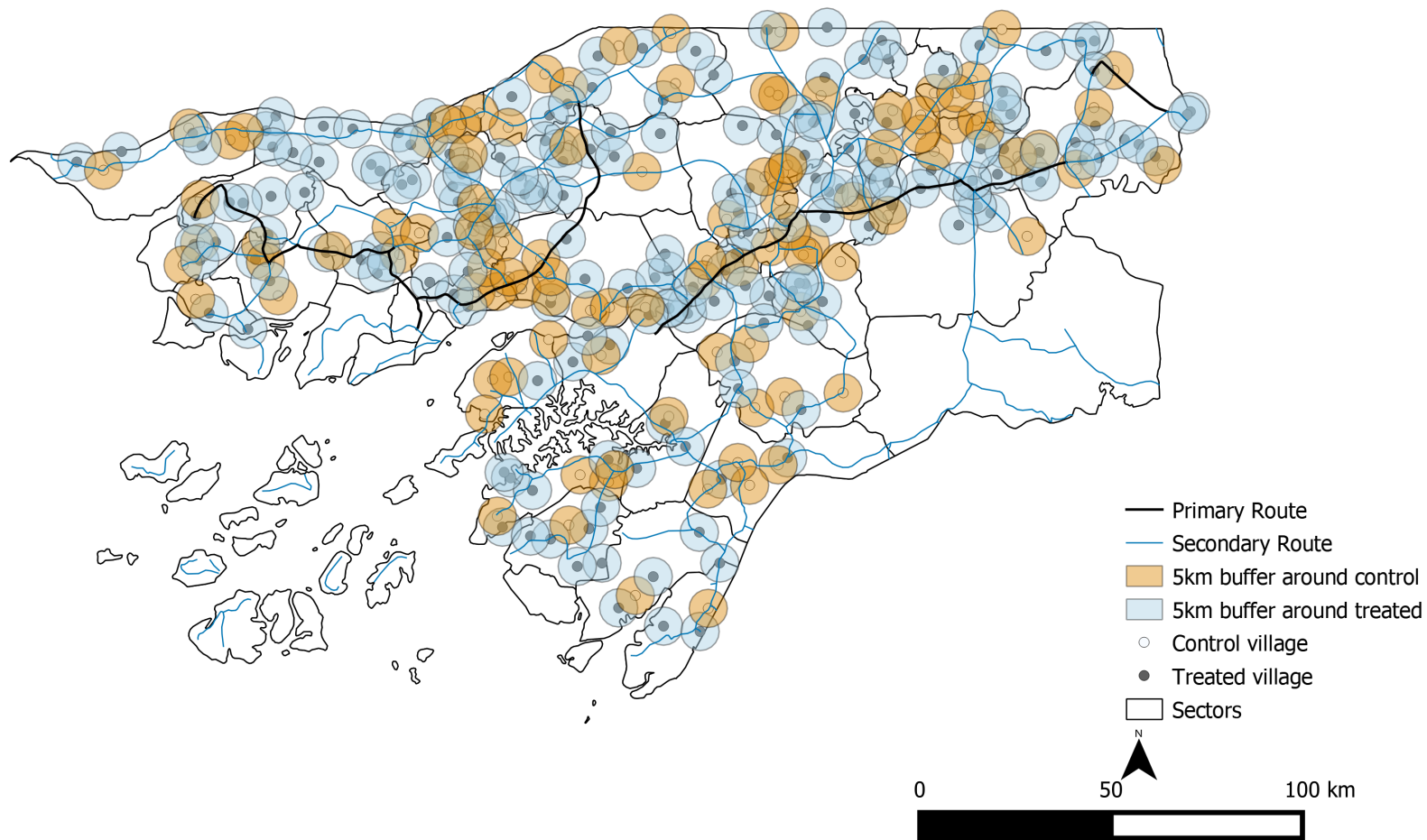


Figure 8: Geographic distribution of treatment with 0-5km buffer around villages

C Robustness checks

C.1 Winsorized outcomes

We winsorized at the 1% level our continuous main outcomes as a robustness check, as we had pre-specified. This affects the significance of our results on the quantity and revenue from sales, though the coefficients remain positive and relatively large. We also look at outcomes winsorized at the 5% level, which we had not pre-specified, which shows that increase in quantity sold and value from sales remains marginally significant using naive p -values. The increase in value of sales corresponds to 8% of the control group mean, when looking at the results winsorized at the 5% level.

C.2 Controlling for unbalanced producer and village characteristics

We ran equation (4) including as controls any producer or village characteristic that was found to be substantially unbalanced at baseline, in tables 7 and 8. We include the following covariates:

- Household size
- 1 if faces difficulties storing
- 1 if sells other agricultural products
- Area of plantation
- Years of experience in the sector
- Standardized principal component of assets ([Filmer and Pritchett, 2001](#))
- Numeracy index ([Anderson, 2008](#))
- 1 if extremely present-biased ([Ashraf et al., 2006](#))
- Number of traders in the village

When the baseline value of one of these covariates was missing, we impute to be equal to the sample mean and control for the imputations with an indicator variable.

C.3 Estimation using the post-double least absolute shrinkage and selection operator (PDSLASSO)

We estimated our intention-to-treat effects using the post-double least absolute shrinkage and selection operator (PDSLASSO), as described by [Belloni et al. \(2014\)](#). Instead of equation (4), we increase precision of our estimates by letting this algorithm pick additional control variables from baseline values of the producer and village characteristics, and values of the main outcome variables.

C.4 Clustering standard errors at the randomisation triplet-level

We estimated our intention-to-treat effects clustering standard errors at the randomisation triplet-level following the recommendation of [de Chaisemartin and Ramirez-Cuellar \(2023\)](#).

C.5 Pooling across trading seasons

We estimated our intention-to-treat effects pooling data from the 2020 and the 2021 trading seasons.

C.6 Between-village spillover specifications

We estimate the effects of treatment intensity on our main outcomes using the following specification:

$$y_{iv} = \text{treatment}_{iv} \cdot \beta_1 + \text{spillover}_{iv} \cdot \delta_1 + y_{0iv} \cdot \gamma_1 + V_{iv-v}^{5km} \cdot \pi_1^{5km} + TP_{iv-v}^{5km} \cdot \pi_2^{5km} + \alpha_v + \epsilon_{iv} \quad (6)$$

where variables are defined as in Equation 4, TP_{iv-v}^{5km} is the total amount of producers assigned to treatment within 5km of producer i (excluding treated producers in village v), and V_{iv-v}^{5km} is the number of villages within a 5km radius (excluding village v). Conditional on the number of villages within a given radius, the total amount of treated individuals that are within this radius is exogenous.²² This equation allows us to estimate the following effects: β_1 gives us the (direct) intent-to-treat effect of the intervention on producers in treated villages. δ_1 gives us the spillover effect of the intervention on producers within treated villages. π_2^{5km} gives us the (indirect) between-village effect of the total amount of treated individuals within a 5km radius.

For analyses using spatial treatment intensity, we account for spatial dependence using Conley standard errors with a uniform kernel up to the boundary of the largest doughnut (Conley, 1999).

In order to pick the radius distance of 5km, we estimate the effect of treatment intensity within a series of non-overlapping doughnuts, $d = 1, \dots, D$, each with inner radius r and outer radius $r + c$ kilometres, as in Egger et al. (2022), with $c \in \{1, 2, 3, 4, 5\}$. We estimate a series of nested models: with a single doughnut ($d = 1$) with $r = 0$; with two doughnuts ($d = 1, 2$) with $r \in \{0, 1\}; \dots$; with ten doughnuts ($d = 1, 2, \dots, 10$) with $r \in \{0, 1, \dots, 9\}$, and then iterate this procedure for doughnuts with a larger outer radius for each c . For each specification and outcome, we then select the model which minimises the Bayesian Information Criterion (BIC).²³ Across all the fifty specifications we estimated for price, the specification with the lowest BIC for which we observed positive variation in treatment intensity in at least 10% of the sample is the doughnut with $r = 0$ and $r + c = 5$.²⁴ We therefore focus on presenting results from this specification.

²² Conditional exogeneity of the number of producers treated within a doughnut comes from the fact that the number of treated producers is a fixed discrete number depending on the random assignment at the village-level. That is, treated villages had 4 treated producers, whereas the pure control villages had none.

²³ These specifications implicitly assume a linear relationship between the intensity of treatment within a given radius and the outcome variable of interest, but allows this effect to vary between the different ‘doughnuts’ specified in the regression.

²⁴ For example, the specification with doughnuts with $r = 0$ and $r + c = 2$ had a marginally lower BIC, but only 5% of the sample had any non-zero variation in this measure of spatial treatment intensity. The lack of variation in treatment intensity within smaller radii is due to our sampling strategy, which aimed to maximise the minimum distance between any village in our sample.

Table 11: Robustness — Main outcomes winsorized at 99th percentile

	Year 1 (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treatment-Spillover	Year 2 (5) Mean (SD) Total obs.	(6) Treatment	(7) Spillover	(8) Treatment-Spillover
<i>Price and revenue:</i>								
Price per sale per kg	294.76 (101.66) 1587	1.73 (2.96) [0.56]	2.76 (3.16) [0.52]	-1.03 (2.98) [0.73]	354.64 (79.70) 1704	5.66* (2.98) [0.08]*	1.22 (3.23) [0.71]	4.44 (2.79) [0.17]
Value of all sales and exchanges	284193.51 (289006.62) 1520	30127.41 (18509.29) [0.26]	18143.43 (21135.80) [0.52]	11983.98 (18573.33) [0.73]	434464.65 (423634.36) 1686	77702.81*** (23248.72) [0.00]***	43159.75* (25442.04) [0.18]	34543.06 (22831.16) [0.17]
Value of all sales	230293.14 (269143.16) 1591	25365.33 (16662.85) [0.26]	2140.42 (18341.82) [0.91]	23224.92 (15886.19) [0.58]	356704.84 (385129.20) 1705	73674.76*** (19610.92) [0.00]***	29882.23 (21728.01) [0.23]	43792.53** (19610.05) [0.10]
Value of exchanges	52020.20 (86004.73) 1635	9291.95 (7644.53) [0.30]	12687.02 (8269.69) [0.50]	-3395.07 (5759.07) [0.73]	75155.05 (108324.67) 1710	8983.78 (8204.34) [0.27]	14674.83* (8239.38) [0.18]	-5691.05 (7541.22) [0.45]
<i>Quantity sold and exchanged:</i>								
Total quantity sold	695.97 (764.74) 1622	82.83* (48.32) [0.22]	22.28 (52.72) [0.67]	60.55 (45.51) [0.46]	956.20 (1013.12) 1709	172.65*** (50.53) [0.00]***	80.48 (54.31) [0.23]	92.17* (48.30) [0.14]
Number of sales	1.48 (0.94) 1693	0.21*** (0.06) [0.00]***	0.08 (0.06) [0.64]	0.13** (0.06) [0.10]*	1.63 (0.81) 1730	0.21*** (0.04) [0.00]***	0.10** (0.04) [0.07]*	0.11** (0.05) [0.09]*
1 if exchanged cashew for rice	0.56 (0.50) 1706	-0.01 (0.03) [0.85]	-0.03 (0.03) [0.64]	0.03 (0.03) [0.59]	0.56 (0.50) 1724	-0.01 (0.03) [0.60]	-0.00 (0.03) [0.99]	-0.01 (0.03) [0.58]
Total quantity exchanged	217.70 (323.48) 1681	13.48 (23.44) [0.71]	16.16 (24.58) [0.64]	-2.68 (21.08) [0.96]	230.05 (335.19) 1707	42.29 (26.95) [0.20]	64.57** (26.63) [0.07]*	-22.27 (24.70) [0.57]
Share of quantity sold over quantity exchanged and sold	0.73 (0.32) 1604	0.02 (0.02) [0.70]	0.02 (0.02) [0.64]	0.00 (0.02) [0.96]	0.79 (0.25) 1691	0.01 (0.02) [0.60]	-0.00 (0.02) [0.99]	0.01 (0.01) [0.57]

Notes: Producer-level intention-to-treat (ITT) estimates reported in columns 2 and 3 across year 1 (2020) and year 2 (2021). Columns 4 tests for differences in parameters obtained in previous two columns. Outcome variables are listed on the left and described in detail in the pre-analysis plan. The unit of observation is the individual producer. All models control for the randomisation triplet fixed-effects and the baseline value of the outcome when it was available. Standard errors are in parentheses and are clustered at the village-level. Stars on the coefficient estimates reflect unadjusted *p*-values. Sharpened *q*-values controlling the false discovery rate across outcomes within each family are shown in brackets. * denotes significance at 10 pct.; ** at 5 pct.; and *** at 1 pct. level. Column 1 and column 5 display the control mean; standard deviation; and total number of observations across all groups in the estimation sample.

Table 12: Robustness — Main outcomes winsorized at 95th percentile

	Year 1 (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treatment-Spillover	Year 2 (5) Mean (SD) Total obs.	(6) Treatment	(7) Spillover	(8) Treatment-Spillover
<i>Price and revenue:</i>								
Price per sale per kg	291.62 (96.87) 1587	2.12 (2.49) [0.40]	2.28 (2.63) [0.69]	-0.16 (2.37) [0.95]	352.17 (75.51) 1704	5.95** (2.71) [0.04]**	2.28 (2.93) [0.44]	3.67 (2.57) [0.31]
Value of all sales and exchanges	268381.83 (227328.24) 1520	22670.69 (13876.99) [0.21]	10018.81 (15519.40) [0.69]	12651.88 (12974.91) [0.66]	411529.74 (344012.16) 1686	67367.14*** (19570.91) [0.00]***	45968.37** (21354.65) [0.07]*	21398.77 (18289.62) [0.32]
Value of all sales	212156.79 (198563.75) 1591	19263.07* (11401.24) [0.21]	870.66 (12720.95) [0.94]	18392.41* (10984.52) [0.38]	334113.48 (308129.59) 1705	68252.59*** (16615.62) [0.00]***	38016.15** (18100.38) [0.07]*	30236.44* (16243.81) [0.25]
Value of exchanges	47953.99 (69132.58) 1635	5369.29 (5582.65) [0.40]	6359.93 (6042.44) [0.69]	-990.63 (4439.21) [0.95]	70998.43 (93578.96) 1710	7404.42 (6835.91) [0.28]	11033.59 (6711.52) [0.14]	-3629.16 (5762.07) [0.53]
<i>Quantity sold and exchanged:</i>								
Total quantity sold	644.94 (572.80) 1622	59.06* (33.74) [0.20]	5.31 (37.73) [0.89]	53.75* (32.42) [0.25]	887.45 (778.79) 1709	173.22*** (42.32) [0.00]***	107.88** (45.85) [0.05]**	65.33 (42.01) [0.30]
Number of sales	1.43 (0.81) 1693	0.19*** (0.05) [0.00]***	0.08 (0.05) [0.61]	0.11** (0.05) [0.10]*	1.61 (0.76) 1730	0.20*** (0.04) [0.00]***	0.10** (0.04) [0.05]**	0.11** (0.05) [0.10]*
1 if exchanged cashew for rice	0.56 (0.50) 1706	-0.01 (0.03) [0.85]	-0.03 (0.03) [0.61]	0.03 (0.03) [0.59]	0.56 (0.50) 1724	-0.01 (0.03) [0.60]	-0.00 (0.03) [0.99]	-0.01 (0.03) [0.58]
Total quantity exchanged	204.12 (270.63) 1681	4.86 (20.20) [0.85]	14.63 (21.21) [0.61]	-9.77 (16.49) [0.69]	215.94 (282.80) 1707	31.11 (21.15) [0.24]	42.41** (20.43) [0.06]*	-11.31 (17.81) [0.58]
Share of quantity sold over quantity exchanged and sold	0.73 (0.32) 1604	0.02 (0.02) [0.70]	0.02 (0.02) [0.61]	0.00 (0.02) [0.96]	0.79 (0.25) 1691	0.01 (0.02) [0.60]	-0.00 (0.02) [0.99]	0.01 (0.01) [0.58]

Notes: Producer-level intention-to-treat (ITT) estimates reported in columns 2 and 3 across year 1 (2020) and year 2 (2021). Columns 4 tests for differences in parameters obtained in previous two columns. Outcome variables are listed on the left and described in detail in the pre-analysis plan. The unit of observation is the individual producer. All models control for the randomisation triplet fixed-effects and the baseline value of the outcome when it was available. Standard errors are in parentheses and are clustered at the village-level. Stars on the coefficient estimates reflect unadjusted *p*-values. Sharpened *q*-values controlling the false discovery rate across outcomes within each family are shown in brackets. * denotes significance at 10 pct.; ** at 5 pct.; and *** at 1 pct. level. Column 1 and column 5 display the control mean; standard deviation; and total number of observations across all groups in the estimation sample.

Table 13: Robustness — Main outcomes controlling for unbalanced characteristics

	Year 1 (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treatment-Spillover	Year 2 (5) Mean (SD) Total obs.	(6) Treatment	(7) Spillover	(8) Treatment-Spillover
<i>Price and revenue:</i>								
Price per sale per kg	295.71 (104.36) 1587	0.43 (3.46) [0.90]	0.65 (3.40) [0.85]	-0.22 (3.31) [0.95]	355.77 (83.13) 1704	3.92 (3.35) [0.32]	-1.03 (3.27) [0.75]	4.95 (3.07) [0.14]
Value of all sales and exchanges	287785.22 (315352.77) 1520	48463.11** (24318.03) [0.17]	29645.71 (25169.23) [0.48]	18817.40 (22167.60) [0.68]	440723.50 (461068.58) 1686	75783.73** (30027.99) [0.03]**	30559.25 (27649.03) [0.54]	45224.48* (25103.27) [0.14]
Value of all sales	233494.84 (292721.24) 1591	39058.70* (22580.75) [0.17]	13231.17 (22820.34) [0.75]	25827.52 (19575.44) [0.68]	364476.23 (428368.42) 1705	65761.91** (26544.51) [0.03]**	16116.88 (24352.45) [0.68]	49645.03** (22738.07) [0.12]
Value of exchanges	52325.81 (88037.41) 1635	9586.30 (7668.80) [0.28]	14398.90 (9180.83) [0.47]	-4812.60 (7242.70) [0.68]	75661.67 (111392.97) 1710	7178.64 (8136.67) [0.38]	12237.10 (8229.46) [0.54]	-5058.46 (7713.05) [0.51]
<i>Quantity sold and exchanged:</i>								
Total quantity sold	708.21 (855.14) 1622	113.11* (60.10) [0.15]	36.95 (59.66) [0.67]	76.16 (53.87) [0.40]	981.13 (1163.13) 1709	152.63** (65.32) [0.05]*	39.23 (62.96) [0.86]	113.40** (52.43) [0.08]*
Number of sales	1.49 (1.01) 1693	0.18*** (0.06) [0.01]**	0.04 (0.06) [0.67]	0.14** (0.06) [0.11]	1.66 (0.92) 1730	0.17*** (0.05) [0.00]**	0.06 (0.05) [0.60]	0.11** (0.05) [0.08]*
1 if exchanged cashew for rice	0.56 (0.50) 1706	0.00 (0.03) [0.92]	-0.03 (0.03) [0.67]	0.03 (0.03) [0.44]	0.56 (0.50) 1724	-0.01 (0.03) [0.75]	0.01 (0.03) [0.86]	-0.01 (0.03) [0.59]
Total quantity exchanged	217.70 (323.48) 1681	14.48 (23.18) [0.80]	10.12 (24.29) [0.68]	4.37 (23.09) [0.85]	231.72 (346.31) 1707	34.74 (26.69) [0.32]	56.83** (27.08) [0.18]	-22.09 (25.54) [0.59]
Share of quantity sold over quantity exchanged and sold	0.73 (0.32) 1604	0.01 (0.02) [0.80]	0.01 (0.02) [0.67]	-0.00 (0.02) [0.85]	0.79 (0.25) 1691	0.01 (0.02) [0.75]	-0.00 (0.02) [0.86]	0.01 (0.01) [0.59]

Notes: Producer-level intention-to-treat (ITT) estimates reported in columns 2 and 3 across year 1 (2020) and year 2 (2021). Columns 4 tests for differences in parameters obtained in previous two columns. Outcome variables are listed on the left and described in detail in the pre-analysis plan. The unit of observation is the individual producer. All models control for the randomisation triplet fixed-effects and the baseline value of the outcome when it was available. Standard errors are in parentheses and are clustered at the village-level. Stars on the coefficient estimates reflect unadjusted *p*-values. Sharpened *q*-values controlling the false discovery rate across outcomes within each family are shown in brackets. * denotes significance at 10 pct.; ** at 5 pct.; and *** at 1 pct. level. Column 1 and column 5 display the control mean; standard deviation; and total number of observations across all groups in the estimation sample.

Table 14: Robustness — Main outcomes estimated using PDSLASSO

	Year 1 (1) Mean (SD) Total obs.	(2)	(3)	(4)	Year 2 (5) Mean (SD) Total obs.	(6)	(7)	(8)
		Treatment	Spillover	Treatment-Spillover		Treatment	Spillover	Treatment-Spillover
<i>Price and revenue:</i>								
Price per sale per kg	295.71 (104.36) 1587	2.07 (3.36) [0.54]	2.33 (3.26) [0.62]	-0.26 (3.21) [0.93]	355.77 (83.13) 1705	6.89** (3.32) [0.05]*	1.32 (3.28) [0.69]	5.57* (2.97) [0.08]*
Value of all sales and exchanges	287785.22 (315352.77) 1527	49955.96** (23903.98) [0.14]	30500.25 (24599.16) [0.43]	19455.72 (21429.83) [0.50]	440723.50 (461068.58) 1696	92352.29*** (29925.93) [0.00]***	39643.84 (27802.66) [0.31]	52708.45** (25310.76) [0.07]*
Value of all sales	233494.84 (292721.24) 1594	39165.14* (21653.65) [0.14]	10791.39 (21815.02) [0.62]	28373.75 (19148.46) [0.50]	364476.23 (428368.42) 1710	80224.19*** (26425.18) [0.00]***	22902.73 (24418.94) [0.46]	57321.47** (23194.90) [0.05]*
Value of exchanges	52325.81 (88037.41) 1639	11216.27 (7925.40) [0.21]	17525.80* (9502.94) [0.26]	-6309.53 (7093.11) [0.50]	75661.67 (111392.97) 1716	9382.75 (7769.93) [0.23]	13794.64* (7972.89) [0.31]	-4411.89 (7354.12) [0.55]
<i>Quantity sold and exchanged:</i>								
Total quantity sold	708.21 (855.14) 1625	112.08* (58.72) [0.14]	28.20 (58.31) [0.63]	83.88 (52.46) [0.27]	981.13 (1163.13) 1714	177.22*** (64.75) [0.01]**	50.18 (61.83) [0.69]	127.04** (52.70) [0.04]**
Number of sales	1.49 (1.01) 1693	0.21*** (0.06) [0.00]***	0.07 (0.06) [0.63]	0.14** (0.06) [0.09]*	1.66 (0.92) 1731	0.20*** (0.05) [0.00]***	0.08* (0.05) [0.19]	0.11** (0.05) [0.04]**
1 if exchanged cashew for rice	0.56 (0.50) 1707	-0.00 (0.03) [0.89]	-0.03 (0.03) [0.63]	0.03 (0.03) [0.47]	0.56 (0.50) 1727	-0.01 (0.03) [0.62]	-0.00 (0.03) [0.90]	-0.01 (0.03) [0.72]
Total quantity exchanged	217.70 (323.48) 1685	20.22 (22.92) [0.63]	14.13 (24.44) [0.63]	6.08 (22.13) [0.86]	231.72 (346.31) 1713	45.40* (25.46) [0.12]	63.69** (26.22) [0.08]*	-18.30 (24.52) [0.59]
Share of quantity sold over quantity exchanged and sold	0.73 (0.32) 1610	0.01 (0.02) [0.76]	0.01 (0.02) [0.63]	-0.00 (0.02) [0.86]	0.79 (0.25) 1700	0.01 (0.02) [0.62]	-0.00 (0.02) [0.90]	0.01 (0.01) [0.59]

Notes: Producer-level intention-to-treat (ITT) estimates reported in columns 2 and 3 across year 1 (2020) and year 2 (2021). Columns 4 tests for differences in parameters obtained in previous two columns. Outcome variables are listed on the left and described in detail in the pre-analysis plan. The unit of observation is the individual producer. All models control for the randomisation triplet fixed-effects and the baseline value of the outcome when it was available. Standard errors are in parentheses and are clustered at the village-level. Stars on the coefficient estimates reflect unadjusted *p*-values. Sharpened *q*-values controlling the false discovery rate across outcomes within each family are shown in brackets. * denotes significance at 10 pct.; ** at 5 pct.; and *** at 1 pct. level. Column 1 and column 5 display the control mean; standard deviation; and total number of observations across all groups in the estimation sample.

Table 15: Robustness — Main outcomes estimated clustering standard errors at the randomisation triplet-level

	Year 1 (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treatment-Spillover	Year 2 (5) Mean (SD) Total obs.	(6) Treatment	(7) Spillover	(8) Treatment-Spillover
<i>Price and revenue:</i>								
Price per sale per kg	295.71 (104.36) 1587	2.07 (4.43) [0.64]	2.33 (3.89) [0.55]	-0.26 (3.03) [0.93]	355.77 (83.13) 1704	6.89* (3.64) [0.08]*	1.32 (3.45) [0.70]	5.57* (2.84) [0.14]
Value of all sales and exchanges	287785.22 (315352.77) 1520	61649.88** (28196.67) [0.06]*	44746.80 (30931.74) [0.37]	16903.08 (29016.04) [0.75]	440723.50 (461068.58) 1686	105532.81*** (38646.95) [0.01]**	58182.34* (31129.62) [0.19]	47350.47 (33431.31) [0.21]
Value of all sales	233494.84 (292721.24) 1591	51335.03** (22663.69) [0.06]*	24128.93 (23608.62) [0.41]	27206.10 (26001.84) [0.70]	364476.23 (428368.42) 1705	95365.15*** (30726.31) [0.01]**	41745.18* (24768.03) [0.19]	53619.97* (29301.24) [0.14]
Value of exchanges	52325.81 (88037.41) 1635	10968.68 (9491.92) [0.34]	17251.21 (12859.48) [0.37]	-6282.54 (6686.81) [0.70]	75661.67 (111392.97) 1710	9774.72 (10476.33) [0.35]	15940.01 (11222.08) [0.21]	-6165.30 (8449.50) [0.47]
<i>Quantity sold and exchanged:</i>								
Total quantity sold	708.21 (855.14) 1622	141.75** (67.14) [0.09]*	63.53 (65.52) [0.55]	78.21 (71.98) [0.54]	981.13 (1163.13) 1709	211.49*** (74.30) [0.01]**	93.73 (67.09) [0.28]	117.77* (65.77) [0.19]
Number of sales	1.49 (1.01) 1693	0.21*** (0.06) [0.01]**	0.07 (0.06) [0.55]	0.14*** (0.05) [0.03]**	1.66 (0.92) 1730	0.20*** (0.06) [0.00]**	0.08 (0.06) [0.28]	0.11** (0.05) [0.10]*
1 if exchanged cashew for rice	0.56 (0.50) 1706	-0.01 (0.04) [0.88]	-0.03 (0.04) [0.55]	0.03 (0.03) [0.54]	0.56 (0.50) 1724	-0.01 (0.04) [0.70]	-0.00 (0.04) [0.99]	-0.01 (0.02) [0.56]
Total quantity exchanged	217.70 (323.48) 1681	19.62 (32.85) [0.69]	20.91 (34.91) [0.55]	-1.30 (21.96) [0.96]	231.72 (346.31) 1707	44.46 (32.66) [0.29]	70.75* (37.15) [0.28]	-26.28 (26.09) [0.52]
Share of quantity sold over quantity exchanged and sold	0.73 (0.32) 1604	0.02 (0.02) [0.68]	0.02 (0.03) [0.55]	0.00 (0.02) [0.96]	0.79 (0.25) 1691	0.01 (0.02) [0.70]	-0.00 (0.02) [0.99]	0.01 (0.01) [0.52]

Notes: Producer-level intention-to-treat (ITT) estimates reported in columns 2 and 3 across year 1 (2020) and year 2 (2021). Columns 4 tests for differences in parameters obtained in previous two columns. Outcome variables are listed on the left and described in detail in the pre-analysis plan. The unit of observation is the individual producer. All models control for the randomisation triplet fixed-effects and the baseline value of the outcome when it was available. Standard errors are in parentheses and are clustered at the village-level. Stars on the coefficient estimates reflect unadjusted p -values. Sharpened q -values controlling the false discovery rate across outcomes within each family are shown in brackets. * denotes significance at 10 pct.; ** at 5 pct.; and *** at 1 pct. level. Column 1 and column 5 display the control mean; standard deviation; and total number of observations across all groups in the estimation sample.

Table 16: Robustness — Main outcomes pooling across years

	(1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover
<i>Price and revenue:</i>				
Price per sale per kg	326.60 (98.69) 3292	4.79* (2.52) [0.08]*	1.77 (2.55) [0.49]	3.02 (2.31) [0.26]
Value of all sales and exchanges	367270.23 (404914.15) 3207	83574.12*** (24422.26) [0.00]***	50786.10** (23862.50) [0.07]*	32788.02 (20451.26) [0.22]
Value of all sales	300792.18 (374321.08) 3297	72884.01*** (21215.67) [0.00]***	31846.50 (20495.86) [0.16]	41037.51** (18187.91) [0.10]*
Value of exchanges	64140.44 (101175.95) 3346	9693.10 (6934.40) [0.16]	16223.28** (7730.56) [0.07]*	-6530.18 (6333.36) [0.30]
<i>Quantity sold and exchanged:</i>				
Total quantity sold	848.06 (1033.22) 3332	174.78*** (53.00) [0.00]***	77.30 (51.45) [0.22]	97.48** (44.18) [0.07]*
Number of sales	1.57 (0.97) 3424	0.20*** (0.04) [0.00]***	0.08* (0.04) [0.16]	0.13*** (0.04) [0.01]**
1 if exchanged cashew for rice	0.56 (0.50) 3431	-0.01 (0.03) [0.74]	-0.01 (0.03) [0.69]	0.01 (0.02) [0.74]
Total quantity exchanged	224.72 (335.03) 3389	33.01 (21.61) [0.21]	46.87** (22.78) [0.16]	-13.86 (18.89) [0.74]
Share of quantity sold over quantity exchanged and sold	0.76 (0.29) 3296	0.01 (0.02) [0.58]	0.01 (0.02) [0.69]	0.01 (0.01) [0.74]

Notes: Producer-level intention-to-treat (ITT) estimates reported in columns 2 and 3 pooling across 2020 and 2021. Columns 4 tests for differences in parameters obtained in previous two columns. Outcome variables are listed on the left and described in detail in the pre-analysis plan. The unit of observation is the individual producer. All models control for the randomisation triplet fixed-effects and the baseline value of the outcome when it was available. Standard errors are in parentheses and are clustered at the village-level. Stars on the coefficient estimates reflect unadjusted *p*-values. Sharpened *q*-values controlling the false discovery rate across outcomes within each family are shown in brackets. * denotes significance at 10 pct.; ** at 5 pct.; and *** at 1 pct. level. Column 1 displays the control mean; standard deviation; and total number of observations across all groups.

Table 17: Mechanisms — Marketing behaviour (pre-specified)

	Year 1 (1) Mean (SD) Total obs.	(2)	(3)	(4)	Year 2 (5) Mean (SD) Total obs.	(6)	(7)	(8)
		Treatment	Spillover	Treatment-Spillover		Treatment	Spillover	Treatment-Spillover
1 if delayed sales because expected price to raise	0.79 (0.41) 1703	-0.01 (0.02) [0.63]	-0.01 (0.02) [0.72]	0.00 (0.02) [0.96]	0.85 (0.36) 1717	-0.04* (0.02) [0.13]	-0.02 (0.02) [0.76]	-0.02 (0.02) [0.61]
1 if sped up sales because expected price to drop	0.27 (0.44) 1694	0.04 (0.02) [0.28]	0.03 (0.02) [0.39]	0.00 (0.03) [0.96]	0.22 (0.41) 1713	0.01 (0.02) [0.88]	0.01 (0.02) [0.76]	-0.01 (0.02) [0.83]
1 if sold most to local buyer	0.43 (0.50) 1622	-0.02 (0.03) [0.63]	0.04 (0.03) [0.39]	-0.06* (0.03) [0.38]	0.56 (0.50) 1709	-0.05* (0.03) [0.13]	-0.02 (0.03) [0.76]	-0.03 (0.03) [0.61]
1 if sold most to itinerant buyer	0.43 (0.50) 1622	0.04 (0.03) [0.28]	0.01 (0.03) [0.79]	0.04 (0.03) [0.64]	0.39 (0.49) 1709	0.06** (0.03) [0.13]	0.03 (0.03) [0.76]	0.03 (0.03) [0.61]
1 if sold most directly to a market	0.04 (0.20) 1622	-0.02** (0.01) [0.13]	-0.02 (0.01) [0.39]	-0.01 (0.01) [0.67]	0.01 (0.11) 1709	-0.00 (0.01) [0.88]	0.00 (0.01) [0.76]	-0.00 (0.01) [0.83]

Notes: Producer-level intention-to-treat (ITT) estimates reported in columns 2 and 3 across year 1 (2020) and year 2 (2021). Columns 4 tests for differences in parameters obtained in previous two columns. Outcome variables are listed on the left and described in detail in the pre-analysis plan. The unit of observation is the individual producer. All models control for the randomisation triplet fixed-effects and the baseline value of the outcome when it was available. Standard errors are in parentheses and are clustered at the village-level. Stars on the coefficient estimates reflect unadjusted *p*-values. Sharpened *q*-values controlling the false discovery rate across outcomes within each family are shown in brackets. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. Column 1 and column 5 display the control mean, standard deviation, and total number of observations across all groups in the estimation sample.

Table 18: Mechanisms — Reasons for selling more than once and type of advice sought

	Year 1 (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover
<i>Sold more than once because...</i>				
...they think they get better prices	0.23 (0.42) 1693	0.07*** (0.02) [0.01]***	0.02 (0.02) [0.91]	0.04 (0.03) [0.12]
...it was advised by <i>n'kalô</i>	0.01 (0.09) 1693	0.03*** (0.01) [0.00]***	-0.00 (0.01) [0.91]	0.03*** (0.01) [0.01]***
...they can smooth consumption	0.22 (0.41) 1693	0.05** (0.02) [0.05]**	-0.01 (0.03) [0.91]	0.06** (0.03) [0.04]**
<i>Sought market advice on...</i>				
...the best time to sell	0.48 (0.50) 1700	0.05 (0.03) [0.31]	-0.04 (0.03) [0.28]	0.08*** (0.03) [0.01]**
...the price to sell at	0.61 (0.49) 1706	0.01 (0.03) [0.76]	-0.05 (0.03) [0.28]	0.06** (0.03) [0.06]*
...whom to sell to	0.24 (0.42) 1632	0.03 (0.02) [0.42]	-0.01 (0.02) [0.67]	0.04 (0.02) [0.11]

Notes: Producer-level intention-to-treat (ITT) estimates in 2020 (year 1) reported in columns 2 and 3. Columns 4 tests for differences in parameters obtained in previous two columns. Outcome variables are listed on the left and described in detail in the pre-analysis plan. The unit of observation is the individual producer. All models control for the randomisation triplet fixed-effects and the baseline value of the outcome when it was available. Standard errors are in parentheses and are clustered at the village-level. Stars on the coefficient estimates reflect unadjusted *p*-values. Sharpened *q*-values controlling the false discovery rate across outcomes within each family are shown in brackets. * denotes significance at 10 pct.; ** at 5 pct.; and *** at 1 pct. level. Column 1 displays the control mean; standard deviation; and total number of observations across all groups.

Table 19: Mechanisms — Beliefs about prices and sharing of offers

	Year 1 (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treatment-Spillover	Year 2 (5) Mean (SD) Total obs.	(6) Treatment	(7) Spillover	(8) Treatment-Spillover
<i>Beliefs about prices</i>								
Expected sale price (XOF/kg)	512.48 (193.57) 1580	-15.37 (10.51) [0.43]	-7.21 (11.24) [0.54]	-8.16 (11.84) [0.71]	664.05 (251.30) 1562	-15.28 (14.08) [0.39]	-8.48 (14.86) [1.00]	-6.79 (14.48) [0.64]
Desired reference price (XOF/kg)	697.85 (277.93) 1701	-15.66 (16.21) [0.50]	-10.37 (17.03) [0.54]	-5.30 (14.40) [0.71]	815.13 (252.18) 1693	-21.58 (15.79) [0.39]	0.52 (17.17) [1.00]	-22.10 (16.72) [0.56]
1 if thinks that the reference price is important in marketing	0.38 (0.49) 1689	0.01 (0.03) [0.69]	0.02 (0.03) [0.54]	-0.01 (0.03) [0.71]	0.24 (0.43) 1717	-0.02 (0.02) [0.39]	0.00 (0.02) [1.00]	-0.02 (0.03) [0.64]
<i>Number of producers...</i>								
...informed of an offer received	6.82 (8.66) 1596	0.35 (0.48) [0.46]	0.13 (0.48) [0.79]	0.23 (0.44) [0.73]	11.28 (13.32) 1633	-0.15 (0.72) [0.84]	-0.90 (0.78) [0.25]	0.75 (0.77) [0.45]
...that shared their offers	4.78 (8.62) 1583	0.71* (0.37) [0.11]	0.59 (0.37) [0.22]	0.13 (0.37) [0.73]	8.93 (16.05) 1614	-0.78 (0.82) [0.69]	-1.28 (0.84) [0.25]	0.50 (0.66) [0.45]

Notes: Producer-level intention-to-treat (ITT) estimates reported in columns 2 and 3 across year 1 (2020) and year 2 (2021). Column 4 tests for differences in parameters obtained in previous two columns. Outcome variables are listed on the left and described in detail in the pre-analysis plan. The unit of observation is the individual producer. All models control for the randomisation triplet fixed-effects and the baseline value of the outcome when it was available. Standard errors are in parentheses and are clustered at the village-level. Stars on the coefficient estimates reflect unadjusted p -values. Sharpened q -values controlling the false discovery rate across outcomes within each family are shown in brackets. * denotes significance at 10 pct.; ** at 5 pct.; and *** at 1 pct. level. Column 1 and column 5 display the control mean; standard deviation; and total number of observations across all groups in the estimation sample.

Table 20: Mechanisms — Placebo outcomes

	Year 1 (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treatment-Spillover	Year 2 (5) Mean (SD) Total obs.	(6) Treatment	(7) Spillover	(8) Treatment-Spillover
1 if filled the sale diary well	0.31 (0.46) 1719	-0.03 (0.03) [0.54]	-0.04 (0.03) [0.45]	0.01 (0.02) [0.53]				
1 if is extremely risk averse	0.35 (0.48) 1627	-0.00 (0.03) [0.89]	0.03 (0.03) [0.45]	-0.03 (0.03) [0.53]				
1 if trusts most people	0.70 (0.46) 1712	0.04* (0.03) [0.27]	0.02 (0.03) [0.45]	0.02 (0.03) [0.53]	0.76 (0.43) 1713	0.04** (0.02) [0.05]**	0.01 (0.02) [0.79]	0.04 (0.03) [0.15]

Notes: Producer-level intention-to-treat (ITT) estimates reported in columns 2 and 3 across year 1 (2020) and year 2 (2021). Columns 4 tests for differences in parameters obtained in previous two columns. Outcome variables are listed on the left and described in detail in the pre-analysis plan. The unit of observation is the individual producer. All models control for the randomisation triplet fixed-effects and the baseline value of the outcome when it was available. Standard errors are in parentheses and are clustered at the village-level. Stars on the coefficient estimates reflect unadjusted *p*-values. Sharpened *q*-values controlling the false discovery rate across outcomes within each family are shown in brackets. * denotes significance at 10 pct.; ** at 5 pct.; and *** at 1 pct. level. Column 1 and column 5 display the control mean; standard deviation; and total number of observations across all groups in the estimation sample.

Table 21: Mechanisms — Cashew used to repay loans

	Year 1 (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treatment-Spillover	Year 2 (5) Mean (SD) Total obs.	(6) Treatment	(7) Spillover	(8) Treatment-Spillover
1 if borrowed money or rice	0.31 (0.46) 1702	0.02 (0.03) [0.65]	0.03 (0.03) [0.57]	-0.01 (0.03) [0.75]	0.42 (0.49) 1724	-0.04 (0.03) [0.15]	0.02 (0.03) [0.62]	-0.06** (0.03) [0.08]*
Implicit price of loans per kg of cashews	58.96 (142.79) 1645	12.62** (5.49) [0.09]*	3.30 (6.13) [0.59]	9.33 (5.79) [0.41]	91.35 (147.69) 1603	8.26* (4.61) [0.15]	-2.81 (3.74) [0.62]	11.07* (6.61) [0.13]
Total quantity paid for loans	44.09 (106.88) 1686	-3.23 (7.08) [0.65]	7.00 (8.99) [0.58]	-10.24 (8.11) [0.41]	57.17 (121.42) 1716	-4.86 (7.60) [0.52]	12.45 (8.75) [0.62]	-17.30** (8.49) [0.08]*
Share of quantity sold over quantity exchanged and paid in loans	0.86 (0.29) 1599	0.01 (0.02) [0.65]	0.03 (0.02) [0.49]	-0.02 (0.02) [0.44]	0.91 (0.20) 1696	0.02 (0.01) [0.15]	0.00 (0.01) [0.75]	0.01 (0.01) [0.23]

Notes: Producer-level intention-to-treat (ITT) estimates reported in columns 2 and 3 across year 1 (2020) and year 2 (2021). Columns 4 tests for differences in parameters obtained in previous two columns. Outcome variables are listed on the left and described in detail in the pre-analysis plan. The unit of observation is the individual producer. All models control for the randomisation triplet fixed-effects and the baseline value of the outcome when it was available. Standard errors are in parentheses and are clustered at the village-level. Stars on the coefficient estimates reflect unadjusted *p*-values. Sharpened *q*-values controlling the false discovery rate across outcomes within each family are shown in brackets. * denotes significance at 10 pct.; ** at 5 pct.; and *** at 1 pct. level. Column 1 and column 5 display the control mean; standard deviation; and total number of observations across all groups in the estimation sample.

Table 22: Mechanisms — Other uses of cashews

	Year 1 (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treatment-Spillover	Year 2 (5) Mean (SD) Total obs.	(6) Treatment	(7) Spillover	(8) Treatment-Spillover
Total quantity produced with imputations	1056.06 (1046.24)	177.39** (71.32)	113.68 (72.66)	63.71 (64.28)	1369.04 (1413.66)	238.62*** (89.25)	193.34** (85.41)	45.28 (67.39)
1 if lost cashew post-harvest	1718 (0.22)	[0.07]* (0.01)	[0.47] (0.01)	[0.55] (0.01)	1739 (0.27)	[0.04]** (0.01)	[0.12] (0.02)	[0.63] (0.02)
Quantity lost post-harvest	1708 (5.05)	[0.91] (0.27)	[0.47] (0.37)	[0.55] (0.36)	1719 (14.77)	[0.39] (0.52)	[0.88] (0.75)	[0.63] (0.74)
1 if processed cashew nuts	1692 (0.04)	[0.91] (0.00)	[0.67] (0.00)	[0.59] (0.00)	1704 (0.10)	[0.41] (0.00)	[0.88] (0.00)	[0.68] (0.00)
Quantity of processed raw cashews	1699 (3.68)	[0.70] (0.52)	[0.60] (0.34)	[0.91] (0.53)	1719 (37.27)	[0.41] (0.27)	[0.88] (0.40)	[0.61] (0.38)
	1699	[0.63]	[0.82]	[0.55]	1719	[0.37]	[0.88]	[0.61]
<i>Additional uses not pre-specified:</i>								
Quantity traded in small weekly exchanges	145.40 (289.09)	-9.38 (16.48)	-9.47 (17.19)	0.09 (17.03)	83.73 (145.90)	2.52 (7.64)	-2.88 (7.55)	5.40 (7.46)
Total quantity of cashews stored	1604 (10.70)	[0.79] (0.31)	[0.99] (0.39)	[1.00] (0.14)	1590 (1.02)	[0.74] (0.58)	[0.88] (0.76)	[0.85] (1.30)
Total quantity of cashews paid to labourers	1679 (261.04)	[0.79] (17.21)	[0.99] (16.03)	[1.00] (14.24)	1699 (245.84)	[0.68] (15.92)	[0.93] (15.97)	[0.85] (13.55)
1 if perceived production increased in 2021 relative to 2020	1421 (0.47)	[0.79] (0.03)	[0.99] (0.03)	[1.00] (0.03)	1392 (0.48)	[0.68] (0.03)	[0.63] (0.03)	[0.85] (0.03)
1 if perceived production decreased in 2021 relative to 2020	1707 (0.50)	[0.79] (0.03)	[0.99] (0.03)	[1.00] (0.03)	1723 (0.47)	[0.68] (0.03)	[0.63] (0.03)	[0.85] (0.03)
	1707	[0.79]	[0.99]	[1.00]	1723	[0.68]	[0.63]	[0.85]

Notes: Producer-level intention-to-treat (ITT) estimates reported in columns 2 and 3 across year 1 (2020) and year 2 (2021). Columns 4 tests for differences in parameters obtained in previous two columns. Outcome variables are listed on the left and described in detail in the pre-analysis plan. The unit of observation is the individual producer. All models control for the randomisation triplet fixed-effects and the baseline value of the outcome when it was available. Standard errors are in parentheses and are clustered at the village-level. Stars on the coefficient estimates reflect unadjusted *p*-values. Sharpened *q*-values controlling the false discovery rate across outcomes within each family are shown in brackets. * denotes significance at 10 pct.; ** at 5 pct.; and *** at 1 pct. level. Column 1 and column 5 display the control mean; standard deviation; and total number of observations across all groups in the estimation sample.

D Additional details on the cashew nut production and commercialisation

D.1 Production costs

The production costs relate to the payment of labour for two main activities: cleaning of the field and collection of the nuts. The cleaning of the field under the cashew trees is necessary in order to have easier access to the nuts (or nuts and fruits) that have fallen from the trees. Family or hired seasonal labour is hired to clean the fields before fruits are ripe and start falling. The second activity involves actually picking up fruits that have fallen from the trees. For the product to be ripe and of good quality, the fruit should be picked up from the floor after it has fallen from the tree. Occasionally, credit constrained producers may collect the fruits directly from the trees, but this means that the nut has not fully absorbed all nutrients from the tree and ends up being smaller and lower quality. Labour costs are noisy to measure, especially since they are often paid in-kind using a rule-of-thumb type of payment (e.g. every third day of work, the amount collected goes to the labourers), or through shared meals.

D.2 Rice exchanges and interlinked contracts

Many producers exchange their cashews in return for rice or to obtain loans to pay for their immediate needs. While technically illegal (according to national legislation), in-kind exchanges and loans of rice are common and represent an important margin on the marketing decision of producers in this market. In our baseline sample, 35% of the producers have bartered cashews for rice and 34% have borrowed money or rice in exchange for cashews. The quantities involved in these transactions are generally a relatively small fraction of the overall production for most producers. In 2019, we estimate that on average 13% of marketable production was used for barter with rice or loans. The majority of these informal loans are reported to take place in the months preceding the trading season (November to February), when producers are most liquidity constrained. These interlinked contracts reduce the ability of producers to pick a better time during the trading season to sell their production. However, even those who took up a loan (in-kind or in-cash) still made sales during the trading season.

D.3 How did the COVID-19 pandemic affect the 2020 and 2021 cashew trading seasons?

The global surge of COVID-19 in the first quarter of 2020 occurred during the run-up to the cashew trading season in Guinea-Bissau, substantially affecting the market conditions due to a combination of international and local factors. The 2020 trading season started on the 27th of May, two months after what would have been the usual start of the season. The delay was a result of COVID-19 and a contested presidential election held in December 2019. However, small trades of cashews were recorded from a number of buyers since March 2020.

Internationally, the two largest importers of raw cashew nuts, India and Vietnam, had already closed their borders and cashew processing plants when Guinea-Bissau confirmed its first two positive cases of COVID-19 on the 25th of March 2020. Even though demand for processed cashew remained high in the first half of 2020, this disruption in the supply chain caused a substantial slump in the international demand for raw cashew nuts. Sales in other cashew-producing countries in West Africa almost came to a complete halt, as border closures all over the world brought additional market uncertainty. Guinea-Bissau's air and land borders were closed, as a preventive measures to reduce the spread of COVID-19. These measures prevented international and regional cashew value-chain agents, mostly buyers and intermediaries, from entering Guinea-Bissau, also reducing the influx of capital needed to buy raw cashew from producers. National travel restrictions between regions also limited the movement of seasonal workers that support the labour-intensive collection of cashew nuts.

Table 23: Mechanisms — Perceived productivity changes

	Year 2 (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover
1 if exerted very high effort in collection	0.30 (0.46) 1719	-0.03 (0.03) [0.58]	-0.05* (0.03) [0.32]	0.02 (0.03) [0.81]
1 if exerted very high effort in monitoring	0.34 (0.47) 1722	-0.02 (0.03) [0.75]	-0.04 (0.03) [0.49]	0.02 (0.03) [0.81]
1 if exerted high effort in collection	0.70 (0.46) 1719	-0.01 (0.03) [0.75]	-0.02 (0.03) [0.65]	0.00 (0.03) [0.87]
1 if exerted high effort in monitoring	0.72 (0.45) 1722	-0.03 (0.03) [0.58]	-0.00 (0.03) [0.86]	-0.03 (0.03) [0.81]
1 if increased area of plantation	0.51 (0.50) 1725	0.01 (0.03) [0.83]	0.02 (0.03) [0.65]	-0.01 (0.03) [0.81]
1 if new trees yielded	0.44 (0.50) 1725	0.02 (0.03) [0.75]	0.03 (0.03) [0.63]	-0.01 (0.03) [0.81]

Notes: Producer-level intention-to-treat (ITT) estimates in 2021 (year 2) reported in columns 2 and 3. Columns 4 tests for differences in parameters obtained in previous two columns. Outcome variables are listed on the left and described in detail in the pre-analysis plan. The unit of observation is the individual producer. All models control for the randomisation triplet fixed-effects and the baseline value of the outcome when it was available. Standard errors are in parentheses and are clustered at the village-level. Stars on the coefficient estimates reflect unadjusted *p*-values. Sharpened *q*-values controlling the false discovery rate across outcomes within each family are shown in brackets. * denotes significance at 10 pct.; ** at 5 pct.; and *** at 1 pct. level. Column 1 displays the control mean; standard deviation; and total number of observations across all groups.

Locally, the government delayed the start of the official trading season, typically starting by the end of March, until the 27th of May. While trade between producers and intermediaries generally takes place before the official start of the season, this is technically illegal and demand for raw cashew nuts generally increases substantially once the government announces the official start of the season.

In 2021, the national market operated more regularly, as a result of both the government and other stakeholders in the cashew value chain agreeing on a set of measures to reduce risk of spreading COVID-19 during the trading season. The start of the official trading season was still delayed until April 7th. However, the number of foreign exporters buying in the local market in 2021 was smaller than pre-pandemic years, due to the reduced international travel.

E Weekly messages

Table 24: Messages sent during the 2020 and 2021 cashew trading seasons

Date sent	Format	English translation
2020 trading season		
01/04/2020	SMS 1	BELAB and the <i>n'kalô</i> service will send you information each week on the cashew nut market. Since the voice-messages are not ready yet, we will send you an SMS.
01/04/2020	SMS 2	Opening of the season was delayed because of coronavirus. We advise to wait to sell until the confusion is over. Dry your nuts well to keep quality
16/04/2020	SMS 3	Opening of the season was delayed still because of coronavirus. Wait to sell until it opens and until the price goes above 300f. Dry your nuts well to keep quality
28/04/2020	SMS 4 (placebo)	BELAB/ <i>n'kalô</i> that interviewed you in 2019 wishes you a good cashew trading season. Remember to fill in your sales diary in order to take part in the lottery at the end of the season.
28/04/2020	SMS 5	Reference price was announced to be 375XOF/KG. Official opening is still delayed because of the state of emergency. Dry your nuts well to keep the quality.
08/05/2020	SMS 6	Port is still closed because of coronavirus. We think the price will increase in the next few weeks, so wait until the price reaches 375XOF/KG or more to sell.
16/05/2020	Robocall 1	"Dear stakeholders of the cashew sector, this is André NANQUE, from <i>nkalô</i> , to talk about the commercialization of cashew nuts this year. Each week, we will send messages to keep you informed about the market situation and prices. The official launch of the cashew trading season is still delayed due to the Coronavirus epidemic. But the season is due to launch in a few weeks. In neighboring countries like Senegal, Gambia and Guinea-Conakry, prices have increased in recent weeks. In fact, cashew processors in India and Vietnam are short of cashews and need it in the coming months. Prices in other countries have risen above 350 XOF / kg. We believe that as soon as the season really starts, prices will go up a lot; therefore, we recommend that you dry your nuts well and maintain your stocks until prices rise in June."
21/05/2020	Robocall 2	"Unfortunately, the official launch of the cashew trade season has been delayed. It shouldn't be too late, but no date is known yet. Despite this, prices started to rise this week due to the high demand in the international market. In Biombo, Cacheu and Oio, prices range from 250 to 300 XOF / Kg, in Gabu the prices fixed at 250 XOF / Kg and remain the same as last week, in Bafata, Bolama, Quinara and Tombali, where the price is also at 250 XOF / kg. In neighboring countries, Senegal, Gambia and Guinea Conakry, prices have exceeded 350 XOF / kg. We recommend that you wait and start selling your product only if the price offered to you reaches 375 XOF / kg or more."
29/05/2020	Robocall 3	"Since the season has started late, it is also likely to end late. It will take a few more weeks for exporters from the port of Bissau to start shipping cashews on ships to India and Vietnam. The demand for Guinea-Bissau cashews is very strong there and, therefore, we believe prices will increase in the coming weeks. Currently, in the regions of Bafata, Gabu, Bolama, Quinara and Tombali, prices have remained at 250 XOF / Kg and in Oio, prices still vary between 250 - 300 XOF / kg. On the other hand, in Cacheu, prices started to rise and are between 300-350 XOF / kg, while in Biombo, the price increase is even stronger and sales are made between 350-375 XOF / Kg. With competition that will rise, we believe that prices will still rise but in the coming weeks. In order not to take too much risk, but to take advantage of the price increase, we advise all producers to wait until 375 XOF / kg is offered before selling the first half of their production and keep the other half selling later when prices can still be higher. We remind you that in Senegal and The Gambia, prices exceed 400 XOF / kg."
11/06/2020	SMS 7	Prices are still rising: we advise to sell half of your stock if prices reach 375XOF/KG or more, and sell the rest later.
17/06/2020	Robocall 4	The commercialisation of cashew nuts is intensifying across the country and, in recent days, representatives of Indian and Vietnamese buyers have started to arrive in the country to check the quality. This week's prices have not changed much from last week. The purchase prices of producers remain between 300 and 350 XOF / kg in the regions of Bafata, Bolama, Gabu, Oio, Quinara and Tombali. Higher prices are practiced in Biombo regions, where sales of 375 XOF / kg are made and in Cacheu, where prices reach up to 400 XOF / kg in locations close to Senegal. At the port of Bissau, cashew nuts trucks are paid between 380 and 420 XOF / kg, depending on the quality of the nuts. When the nuts are very dry, they are more expensive. With increasing competition between buyers, we continue to think that prices will increase slightly in the coming weeks and we always advise to store the cashews until the price of 375 XOF / kg is offered, and to sell half of your stocks when that price is offered.
25/06/2020	Robocall 5	"Large quantities have arrived in Bissau since the season was launched. This slightly reduced demand at the port and caused a slight drop in prices for wholesale prices at the port of Bissau. As a result, producer prices have fallen slightly in some areas of production and are stable in others. Currently, prices are between 300 and 325 XOF / kg in the regions of Bafata and Gabu and between 300 and 350 XOF / kg in all other regions of the country. With the coronavirus epidemic still a problem in many countries around the world, cashew buyers have reduced their orders. Unlike the past few weeks, we are no longer sure that prices will rise. That is why we recommend that you sell most of your cashews if the prices offered are higher than 325 XOF / kg."

Impact of Market Information on Cashew Producers in Guinea-Bissau

01/07/2020	Robocall 6	"This week, the first shipment has already left the ports of Bissau, bound for India. Despite these first exports, there are still a lot of stocks in the port of Bissau and, therefore, exporters are not in a hurry to buy. Above all, they want to export the nuts they have in their stores before placing further orders with traders in the production areas. That is why prices practically do not change, always with sales between 300 and 350 XOF / kg in the areas of cashew production. We always recommend selling most of your nuts if a price of 325 XOF / kg or more offers you, because with the situation of the coronavirus, we do not know how the prices will evolve in the coming weeks."
14/07/2020	Robocall 7	"This week, there was a certain slowdown in terms of cashew nut transactions in Bissau. Traders have reduced deliveries and are demanding higher prices from exporters. But at the same time, demand from the Vietnam and India factories remains low and cashew nut prices have fallen in Senegal, Gambia and the Ivory Coast. In certain regions of Guinea Bissau, prices also drop a little. In the regions of Bolama, Quinara and Tombali, prices remained at 300 XOF / Kg; in the regions of Bafata and Gabu, prices will vary between 250-350XOF / Kg; in Biombo and Oio, prices are between 300-350 XOF / Kg and in Cacheu, there were slight increases of 25 XOF in prices, with variations between 325-375 XOF / kg. We recommend selling most of the production when a price of 325 XOF / kg or more is offered to you"
16/07/2020	Robocall 8	"This week, new exporters started to buy cashew nuts which increased demand at the port. Prices rose marginally at the port of Bissau. The prices at the weybridge averaged between 375 and 390 XOF / kg, whereas last week they were between 370 et 380 XOF / kg. This increase in demand also increased producer prices. In the regions of Bafata, Gabu, Oio and Bolama the producer price is around 300 XOF / kg. In Quinara and Tombali, producer prices are between 300 et 325 XOF / kg, slightly better than the previous week. In Biombo, certain producers can sell up to 330 XOF / kg, however in Cacheu prices are between 350 et 375 XOF / kg. As last week, we recommend to sell most or all production when a price of 325 XOF / kg or more is offered to you."
22/07/2020	Robocall 9	"This week, the competition between cashew exporters in Bissau port further increased and prices reached 400 XOF / kg. The producer prices also slightly increased. Higher prices continue to be paid in Cacheu, where producer prices are between 350 and 375 XOF / kg. In the regions of Bafata, Bolama, Gabu, Biombo and Oio prices are between 300 and 350 XOF / kg i.e. an increase of between 10 and 50 XOF / kg. The lower prices are paid in Quinara and Tombali where cashews are purchased for prices between 300 and 325 XOF / kg. This increased demand is an excellent opportunity to try to negotiate a good price for the cashew nuts you still have with you. We recommend negotiating a price of 350 XOF / kg to sell all remaining stocks."
31/07/2020	Robocall 10	"This week, the competition between nut exporters in the port of Bissau continues to be great and the prices also increased slightly. The producer prices also increased slightly in the production zones. The highest prices in Cacheu producer reached between 350 and 400 XOF / kg. In the regions of Bafata, Biombo, Bolama, Oio and Gabu prices were 350 XOF / kg, which was the same price of last week. In the regions of Quinara and Tombali the price increase was strong. In these two regions the prices went from 300 and 325 XOF / kg last week to 350 and 375 XOF / kg this week. The season will end in a few weeks. We therefore recommend negotiating a minimum price of 350 XOF / kg to sell all remaining stocks. You can get a good price without waiting too much more to sell."
06/08/2020	Robocall 11	"This week, the cashew trading season is near the end in the regions of Bafata, Biombo, Bolama, and Oio where the last prices paid to producers were 350 XOF / kg. Quinara and Tombali persist in high demand for cashew nuts and prices paid are between 350 and 375 XOF / kg, like last week. In Cacheu, cashews nuts are still purchased between 375 and 400 XOF / kg but there is limited quantity available. In the port of Bissau, the competition fell slightly and prices decreased slightly. International demand is limited at this point so it is very unlikely that prices will keep increasing. Our advice is to sell the nuts that you still have as fast as possible."
13/08/2020	Robocall 12	"This week, the commercialization season cashews finished in the regions of Bafata, Biombo, Bolama and Oio, where heavy rain began and the few available stocks made the trade very difficult. In Quinara and Tombali, there is still demand for cashews with stable prices between 350 and 375 XOF / kg. In Cacheu, nuts are still purchased between 375 and 400 XOF / kg. In the port of Bissau, the competition increased slightly and prices also marginally increased. International demand is limited at this point what makes very unlikely rising prices. Our advice and sell the nuts that are with you as fast as possible."
22/08/2020	Robocall 13	"This week the trading season ended in all cashew producing regions. Almost all seasonal trading points of intermediaries from producers have closed, with no purchase and sales of cashews recorded inside the country, throughout the week. The last sales are made in the capital, Bissau, between traders and exporters with a sale price between 400 and 430 XOF / kg. As such, we'll stop sending weekly messages from next week. We hope you have enjoyed our information and we will work to continue to inform you from the beginning of the next cashew trading season. Until the next year!"
09/09/2020	SMS 8 (placebo)	BELAB/n'kalô that interviewed you in 2019 reminds you to keep your sales diary in order to take part in the lottery before the next trading season.

Impact of Market Information on Cashew Producers in Guinea-Bissau

2021 trading season		
SMS 1	24/03/2021	"We are restarting the <i>n'kalô</i> messages for 2021. Producer prices are between 250-300 XOF/kg. We think they will increase in the coming months so we advise you not to sell yet. Have a good season!"
Robocall 1	23/04/2021	"This week, prices rose slightly in all Guinea-Bissau cashew producing areas. However, prices have not yet reached peak, as not all buyers and exporters received their permits and, therefore, the product cannot yet go out to the harbour. This week, the highest prices are observed in the regions of Biombo, Cacheu and Oio, where producer prices are between 350 and 360 XOF / kg. In the regions of Bafata, Quinara and Tombali prices range from 300 to 350 XOF / kg. The regions where the smallest prices are observed are Bolama and Gabu where prices are between 250 and 350 XOF / kg. Even in these regions, prices tend to increase. We continue to recommend waiting for prices to increase before selling. A price of 450 XOF / kg seems to us easily reach before sale."
Robocall 2	29/04/2021	"Again this week, prices are slightly rising across the country. The competition between buyers begins to emerge but did not reach its maximum because deliveries in the port of Bissau are not yet possible. The producer prices recorded are: in the Gabu region, prices have been varied between 250 and 350 XOF/kg; In the Bolama region, the minimum price has grown by 50 XOF/kg, with prices between 300 and 350 XOF/kg, similar to the regions of Bafata, Quinara and Tombali; In the Biombo region, minimum price reductions were recorded at 50 XOF/kg and the maximum price at 10 XOF/kg compared to last week. In this region the prices ranged between 300 and 350 XOF/kg; In the Oio region, prices remained relatively stable throughout the week, with prices ranging between 350 and 360 XOF/kg; In the Cacheu region, along with the borders with Senegal, maximum prices grew by 15 XOF/kg compared to last week, reaching values higher than the reference price announced by the government, prices are ranging between 350 and 375 XOF/kg along the border. In neighboring countries, prices continue to go up. They are between 400 and 525 XOF / kg in Senegal and most areas of West African cashews production, where the season started earlier. We are therefore convinced that prices will continue to increase in the coming weeks and therefore always recommend that you avoid selling at a price below 450 XOF / kg."
Robocall 3	13/05/2021	This week, despite the official opening of the scale in Bissau, the trucks did not begin to deliver the cashew nuts to the harbour. The reason stems from negotiations between cashew buyers and the government on taxes. Once the situation is resolved, sales can finally accelerate and prices should rapidly increase. In recent days, prices have continued to increase despite everything, exceeding 350 XOF / kg in most production zones prices of 400 XOF / kg that is already available in the regions of Biombo and Cacheu. Some buyers in the Cacheu region began offering 450 XOF / kg. We continue to recommend that you wait until this price of 450 XOF / kg is offered to you before selling most of your cashews stocks.
Robocall 4	21/05/2021	"The cashew market in Guinea-Bissau, throughout the week, registered positive evolutions in terms of negotiations, which had been taking place between the government and the confederation of cashews associations, and also in relation to the price to the producer. From the information collected from the Cashew National Agency, soon the new fees and taxes resulting from the above mentioned negotiations should be announced. In recent days, the following variations have been recorded: in the Bolama region, prices remained relatively stable compared to last week, ranging between 300-350 XOF/kg; In the Tombali region, the prices were relatively stable throughout the week, with oscillation of around 350 XOF/kg, in the regions of Bafata and Biombo, this week, similar variations were registered to those last week, that is, between 350-400 XOF/kg; In Gabu and Oio regions, prices remained relatively stable throughout the week, with variations between 350-375 XOF/kg, similar variations for Quinara region, where prices grew by 25 XOF/kg compared to last week. Finally, in the Cacheu region, near the borders with Senegal, prices have been ranging between 400 - 450 XOF/kg like last week. Prices will increase in the next days after the Government announcements. We still advise to wait 450 XOF/kg to sell most of your production."
Robocall 5	28/05/2021	"This week, cashew traders and exporters finally reached an agreement with the government about the level of export taxes and deliveries to the port of Bissau have began. The first sale prices of cashews that arrive in Bissau are between 450 and 490 XOF / kg. In the vast majority of production areas, prices increased to 400 XOF / kg, although some 350 XOF / kg sales are still observed in the Oio and Bolama regions. In the regions of Cacheu and Bafata some producers can negotiate 450 XOF / kg. Now that the campaign is fully launched, our recommendation is as follows: If you receive an offer of 400 XOF / kg, sell half of your cashews stocks and if 450 XOF / kg are offered, sell all your cashews stocks. Even if the demand is good, it is quite uncertain that prices rise above 450 XOF / kg, so we should avoid looking for higher prices because it is also possible for prices to fall at the beginning of the rainy season."
Robocall 6	09/06/2021	"This week, in Bissau, the prices oscillated between 460-475 XOF/kg, ie there was a minimum price growth at 10 XOF/kg and a decrease in the maximum price at 15 XOF/kg, compared to last week. This decrease in the maximum price, next to the scales, is due to the mass arrival of significant amount (between 30,000 - 40,000 tons) of nuts from the regions, resulting from the recent opening of Bissau scales and also, due to the need to free space in the warehouses of the regions, so that there is continuity of the purchases of the intermediaries with the producers. This week, in the regions, overall, producer price variations continued between 350 and 450 XOF/kg with most purchases made at 400 XOF/kg. Like last week, our recommendation is as follows: If you receive an offer of 400 XOF / kg, sell at least half of your cashew stocks and if 450 XOF / kg are offered, sell all your cashews stocks."

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Robocall 7	11/06/2021	"This week prices are around 475 XOF/kg in Bissau, a slight increase over last week. The demand is strong in the port of Bissau, as in other cashew producing countries. Like the last week, in the regions, producer price variations remain between 350 and 450 XOF/kg with most purchases at 400 XOF/kg. In parts of the Cacheu region the prices have increased up to 475 XOF/kg. Like our recommendation last week: If you receive an offer of 400 XOF / kg, sell at least half of your cashew stocks and if 450 XOF / kg is offered, sell all your cashews stocks."
Robocall 8	18/06/2021	"This week, prices are relatively stable in Bissau as in production areas. The demand remains good, but the supply is strong and more than 130,000 tons have already been delivered to the port of Bissau. Bissau prices are still between 465 and 475 XOF / kg and prices in production areas are between 350 and 475 XOF / kg. More precisely, in Bolama and Oio regions, price variations remain between 350 - 400 XOF/kg. Bafata and Biombo have prices of around 400 XOF/kg, i.e. a minimum price rise of 50 XOF/kg compared to last week; In the regions of Gabu, Quinara and Tombali, price variations are between 375 - 400 XOF/kg, i.e. minimum price increases of 25 XOF/kg compared to last week; In the Cacheu region, price variations remain between 350 - 475 XOF/kg. The rains are about to start, which is why our recommendation is now: If you are offered 400 XOF / kg, sell all your cashew stocks."
Robocall 9	25/06/2021	"This week, the shipments of cashews from the harbour finally could start and this caused a slight increase in Bissau prices as in most areas of cashews production. The prices of cashews that arrive in Bissau increased, ranging between 470 and 480 XOF / kg. At the level of production zones, the price of 400 XOF / kg to the producer tends to be most common in all regions. Only the Bolama and Oio regions still record some purchases at 350 XOF / kg, but even in these regions the price of 400 XOF / kg is offered by some buyers. In all other regions, prices reached 400 XOF / kg and in the Cacheu region prices reached 475 XOF / kg in some villages. As the rains are starting, we recommend that you enjoy this situation to negotiate the sale of your latest stocks at 400 XOF / kg and finish your season."
Robocall 10	02/07/2021	"This week, the competition among cashew exporters to access the latest remaining stock increased markedly. Prices in the port of Bissau rose between 495 and 500 XOF / kg. This increase in port prices resulted in a slight increase in producer prices in production areas. The price of 400 XOF / kg spread virtually to all zones and prices even increase between 425 and 500 XOF / kg in the Cacheu region. As the campaign approaches the end, we continue to advise to take advantage of this good demand to negotiate at least 400 XOF / kg and sell your remaining stocks."
Robocall 11	09/07/2021	"At the end of the past week, the quotations of cashew prices climbed in the international market. Processors in India and Vietnam have been informed that the harvest is coming to an end in West Africa and made large requests from the latest fresh cashew stocks available in Guinea-Bissau. With this increase in demand, Bissau prices have increased even more and are now between 500 and 510 XOF / kg for the cargo arriving in the port. In production areas, producers' purchase prices have increased even more and now range from 400 to 450 XOF / kg, with most purchases at 450 XOF / kg. This unexpected increase is an unexpected opportunity to sell your remaining cashews. Therefore, we recommend that you negotiate 450 XOF / kg to sell your latest fresh cashews stocks. Note that the season will end soon."
Robocall 12	17/07/2021	"Now the commercialization of cashews ended in other major producing countries, such as Ivory Coast, Nigeria and Ghana, so Indian buyers and Vietnamese are increasing their requests for Guinea-Bissau cashews. Under these conditions, wholesale prices increased slightly in the port of Bissau and are between 515 and 520 XOF / kg. Producers prices remain between 400 and 450 XOF / kg, with more and more purchases at 450 XOF / kg. As last week, we recommend that you negotiate a price of 450 XOF / kg to sell your latest nuts and finish the season before rainfall decreases quality."
Robocall 13	23/07/2021	"This week, the demand remains very strong in the port of Bissau to capture the latest cashew nuts available in Guinea-Bissau. The prices of trucks loaded with cashews delivered in the port of Bissau have increased even more and now between 520 and 540 XOF / kg. In production zones, the latest major sales are between 400 and 500 XOF / kg, with most sales at 450 XOF / kg. We always recommend negotiating at least a price of 450 XOF / kg to sell your latest stocks before rainfall degrade the quality. The season is coming to an end and we will soon be completing the spread of our cashew market information messages."
Robocall 14	29/07/2021	"This week, the demand remains very high in the port of Bissau to buy the latest cashews available in Guinea-Bissau. Wholesale cashew prices delivered to the port of Bissau grew a lot and range from 535 to 550 XOF / kg. In production zones, the last major sales were between 400 and 500 XOF / kg, with most sales at 450 XOF / kg. We recommend negotiating at least this price of 450 XOF / kg to sell all your stock, as the campaign is at the end. Despite the end of the cashew trading season, N'Kalô will continue its services, informing you of various issues, which may help you make better decisions."

F Transaction diaries

During our baseline survey, producers in our sample were provided with a simple paper-based template to record their transactions during the 2020 trading season. We refer to this template as a transaction diary.²⁵ The data-collection team showed producers how to correctly fill in this diary to record the date, quantity sold and price of a transaction.

Importantly, producers recorded the exact day in which they made a transaction. Instead, in our in-person interview, we only asked producer to recall in which month they had made a transaction.

During the in-person follow-up interview, the survey team collected the diaries that had been left after the baseline. After data-collection, two operators digitised the data from the sales diaries to check whether they had been correctly used to record the date, quantity and price of sales made during the 2020 seasons. In the rest of this sub-section we use the data recorded in these transaction diaries to provide further evidence on the impact of the intervention.

The information provided by the intervention matched the price dynamic observed in our sample. Appendix Figure 9 shows the daily average prices recorded by the sample of producers that filled in the diaries.²⁶ The vertical lines show the dates in which producers received a message from *n'kalô*. Blue lines show a "bearish" message, that suggested that prices were low and it was better to wait. Orange lines provided a "bullish" message, that suggested that prices were high and it was a good time to sell. The price movements recorded in the transactions are consistent with the information that producers received.

The transaction diaries provide further evidence that the effects of our intervention may be due to the messages content, rather than just due to a salience effect, as in [Bettinger et al. \(2021\)](#). For example, we can rule out that producers mechanically sold more frequently when they received messages. As Appendix Figure 10 illustrates, there is not a consistent pattern between days in which the messages were delivered and days in which producers recorded making a sale. The message content matters.

In Appendix Table 25, we estimate treatment effects on outcomes constructed using the data from the transaction diaries. Several of these effects point in the same direction as those we had estimated for the full sample. Average prices are marginally higher for treated producers, but not significantly so, relative to the control group. The number of sales is not greater for the treatment group relative to the control group, but this may be because we are selecting a group of producers that is already making at least one transaction.

The treatment effect on quantity sold and the value of sales point in the same direction as those estimated for the full sample using data from the in-person follow-up survey.

As Appendix Figure 10 had alluded, we find no evidence that treated producers sell within days of receiving a message. Instead, we find that treated producers recorded making more transaction in June relative to the control group, a time when prices were at their highest and when the intervention messages had advised producers to sell. We do not find that they sold earlier in the trading season, but this lack of finding may also be because the sub-sample of control producers had recorded more sales in this period relative to the full sample of control producers.

²⁵ To encourage the use of the sales diary, the data-collection team promised that producers that kept records could participate in a lottery to win a 50kg bag of rice. Two placebo messages sent to all producers in 2020 reminded producers to use the sales diary.

²⁶ Appendix Table 20 shows that having filled in the diary is not correlated with treatment. Not all producers filled in the diaries. Other producers could not find the diary during the in-person follow-up interview.

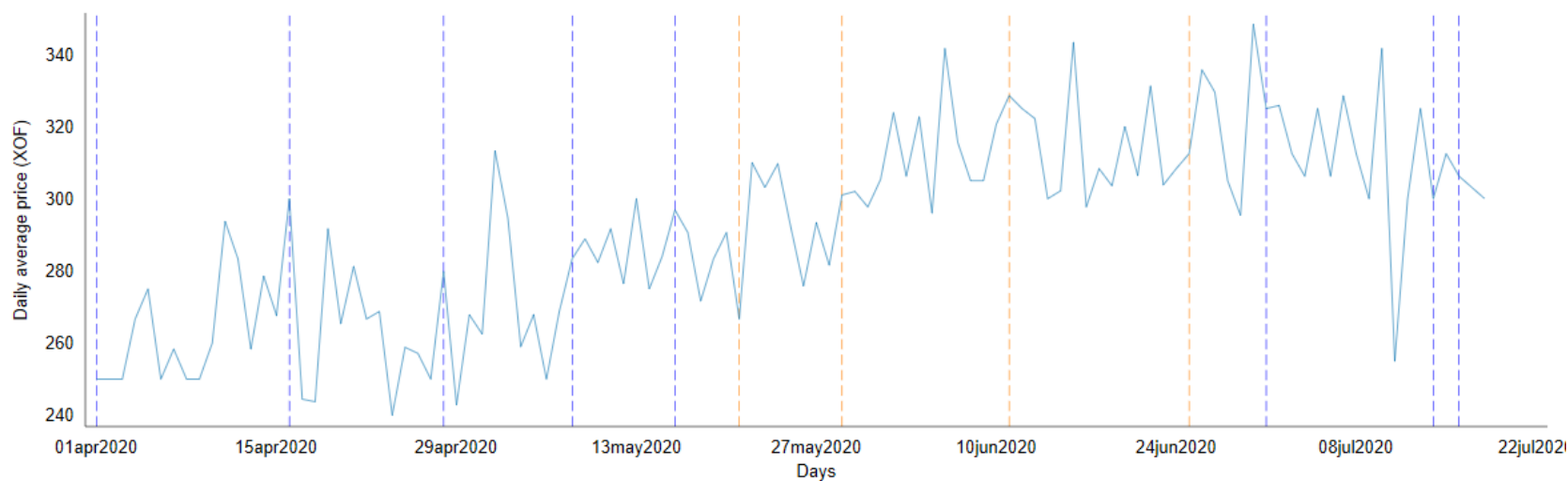


Figure 9: Daily average prices recorded in transaction diaries^a

^a Daily average price recorded in transaction diaries left with producers after baseline. The vertical lines show the dates in which producers received a message from *n'kalô*. Blue lines show a "bearish" message that suggested that prices were low and it was better to wait. Orange lines show a "bullish" message that suggested that prices were high and it was a good time to sell.

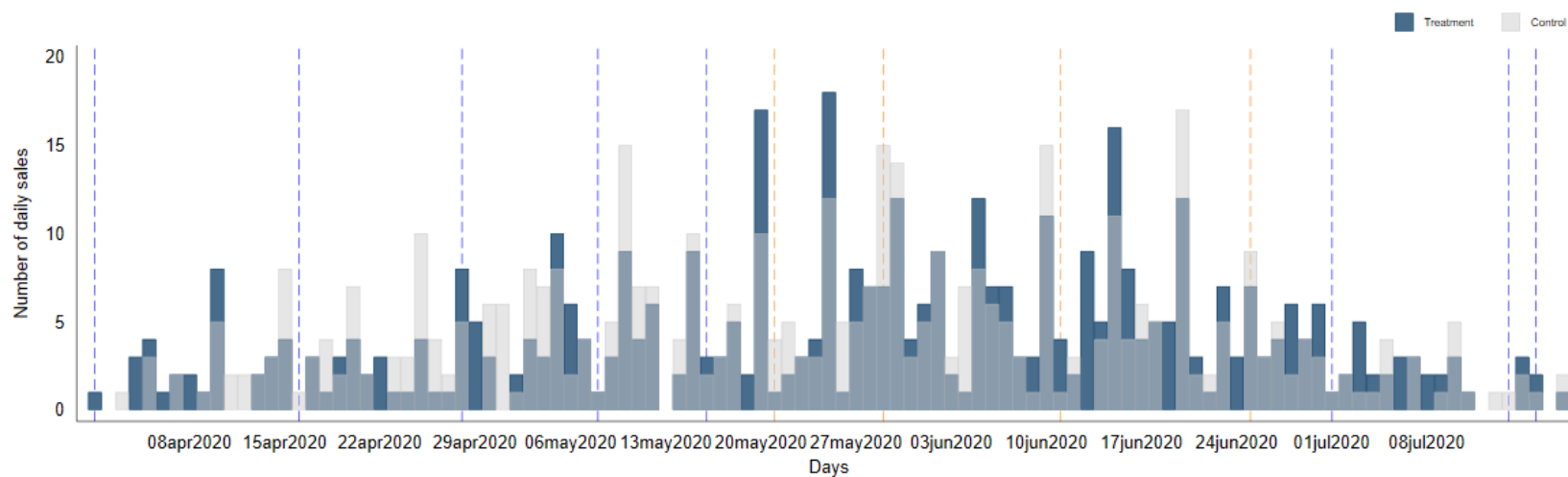


Figure 10: Number of daily sales recorded in transaction diaries by treatment^a

^a Number of daily sales recorded in transaction diaries left with producers after baseline, between treatment and control producers (spillover group is omitted from the graph). The vertical lines show the dates in which producers received a message from *n'kalô*. Blue lines show a "bearish" message that suggested that prices were low and it was better to wait. Orange lines show a "bullish" message that suggested that prices were high and it was a good time to sell.

G Village-trader outcomes

During our in-person follow-up, we tried to interview an intermediary present in the village at the time the data-collection team surveyed producers. We were not able to survey an intermediary in every village, and almost all of the intermediaries that we interviewed were those that lived permanently in the village. Table 26 shows the main outcomes measured at the intermediary-level across treated and control villages.

Table 26: Village intermediaries outcomes

	(1) Control	(2) Treatment	(3) (1) - (2) [<i>p</i> -value]	(4) N
1 if intermediary was interviewed in 2021	0.42 (0.50)	0.47 (0.50)	-0.05 [0.39]	304.00
1 if made any purchase	0.85 (0.36)	0.83 (0.38)	0.02 [0.71]	136.00
Number of producers with whom they exchanged	4.23 (6.47)	6.32 (10.21)	-2.09 [0.16]	131.00
Quantity purchased	10626.15 (17849.26)	11823.76 (16001.22)	-1197.62 [0.70]	136.00
Purchase price	314.63 (51.53)	316.10 (39.39)	-1.46 [0.88]	114.00
Resale price	395.00 (71.06)	382.71 (81.00)	12.29 [0.57]	53.00
Margin	74.17 (67.37)	65.57 (78.32)	8.60 [0.68]	53.00
1 if made any exchange	0.56 (0.50)	0.60 (0.49)	-0.04 [0.66]	136.00
Quantity of cashews obtained from exchanges	2749.68 (4103.98)	3145.35 (5085.62)	-395.67 [0.63]	132.00
Quantity of rice exchanged for cashews	2545.21 (3942.35)	2967.53 (4702.57)	-422.32 [0.58]	133.00

Notes: Column 1 reports the mean in control villages for outcomes related to the 2020 trading season. Column 3 reports the mean in treatment villages. Outcome variables are listed on the left. The data comes from a survey of village-based intermediaries. Due to logistical issues it was not possible to interview an intermediary in every village. Standard deviations are reported in parentheses. Columns 3 reports the difference between columns 1 and 2 and the *p*-value from a *t*-test of equality between the means of intermediaries in treatment and control villages in brackets.

No outcome is statistically different between the two groups of villages. Consistent with the information provided by treated producers, village traders in treated villages record purchasing a larger amount of raw cashew nuts relative to those in control villages, though the difference is not statistically significant, it represents about 10% of the control mean. We also see a reduction in the margins earned by the sample of surveyed intermediaries in treated villages relative to control villages. We are cautious in not over-interpreting this difference because the sample for whom we can observe the resale price is small: many intermediaries reported not knowing the resale price but buying on commission for an exporter in exchange of a fixed income or quantity-based fee.

We also sought information on the number of intermediaries that visited the villages. We asked the village leader, or whom they had delegated, how many itinerant intermediaries had visited the village during the trading season. In line with our findings of positive *between*-village spillovers, we find in table 27 that control villages received a higher number of visits by itinerant intermediaries.

Table 25: Mechanisms — Salience and diary-based measures

	Control (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover
Average price from the diary	310.96 (59.06) 448	3.92 (6.14) [0.52]	-8.94 (5.97) [0.54]	12.85** (5.81) [0.11]
Number of sales from the diary	3.00 (2.45) 523	-0.26 (0.27) [0.44]	-0.16 (0.26) [0.67]	-0.10 (0.20) [0.62]
Total quantity sold from the diary	1022.76 (1856.30) 462	259.82 (235.80) [0.44]	-89.52 (203.95) [0.67]	349.34 (235.91) [0.19]
Total value of sales from the diary	338145.95 (775014.25) 463	85771.11 (81123.21) [0.44]	-29425.56 (68545.01) [0.67]	115196.68 (74244.75) [0.19]
Timing:				
1 if sold cashews within two days of a message	0.46 (0.50) 514	-0.02 (0.05) [0.87]	-0.00 (0.06) [1.00]	-0.02 (0.06) [0.89]
1 if sold cashews in April 2020 or before	0.27 (0.44) 514	-0.03 (0.05) [0.87]	0.01 (0.05) [1.00]	-0.04 (0.05) [0.89]
1 if sold cashews in May 2020	0.58 (0.50) 514	0.01 (0.06) [0.87]	-0.01 (0.06) [1.00]	0.02 (0.06) [0.89]
1 if sold cashews in June 2020	0.54 (0.50) 514	0.10* (0.06) [0.47]	0.04 (0.06) [1.00]	0.06 (0.05) [0.89]
1 if sold cashews in July 2020 or later	0.15 (0.35) 514	0.01 (0.04) [0.87]	0.02 (0.04) [1.00]	-0.01 (0.05) [0.89]

Notes: Producer-level intention-to-treat (ITT) estimates in 2020 (year 1) from transaction diaries reported in columns 2 and 3. Columns 4 tests for differences in parameters obtained in previous two columns. Outcome variables are listed on the left and described in detail in the pre-analysis plan. The unit of observation is the individual producer. All models control for the randomisation triplet fixed-effects and the baseline value of the outcome when it was available. Standard errors are in parentheses and are clustered at the village-level. Stars on the coefficient estimates reflect unadjusted *p*-values. Sharpened *q*-values controlling the false discovery rate across outcomes within each family are shown in brackets. * denotes significance at 10 pct.; ** at 5 pct.; and *** at 1 pct. level. Column 1 displays the control mean; standard deviation; and total number of observations across all groups.

Table 27: Village intermediaries visits

	(1) Control	(2) Treatment	(3) (1) - (2) [<i>p</i> -value]	(4) N
Number of itinerant traders that visited the village	10.13 (14.01)	6.75 (9.29)	3.38 [0.03]	284.00
Number of seasonal traders that stayed in the village	2.81 (4.54)	2.16 (3.32)	0.65 [0.20]	284.00
Number of traders based in the village	4.42 (5.00)	4.03 (4.71)	0.39 [0.52]	282.00
1 if itinerant traders visited in March	0.07 (0.25)	0.09 (0.29)	-0.02 [0.46]	284.00
1 if itinerant traders visited in April	0.62 (0.49)	0.51 (0.50)	0.11 [0.07]	284.00
1 if itinerant traders visited in May	0.78 (0.41)	0.68 (0.47)	0.11 [0.04]	284.00
1 if itinerant traders visited in June	0.72 (0.45)	0.57 (0.50)	0.15 [0.01]	284.00
1 if itinerant traders visited in July	0.10 (0.30)	0.08 (0.28)	0.02 [0.66]	284.00

Notes: Column 1 reports the mean in control villages for outcomes related to the 2020 trading season. Column 3 reports the mean in treatment villages. Outcome variables are listed on the left. The data comes from a survey of village leaders. Standard deviations are reported in parentheses. Columns 3 reports the difference between columns 1 and 2 and the *p*-value from a t-test of equality between the means between treatment and control villages in brackets.