Impact of Market Information on Cashew Producers in Guinea-Bissau

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Brais Álvarez Pereira ⓡ Giulio Schinaia (**JMP**) ⓡ
Sebastian Schäber ⓡ Dayvikson Raiss Laval Tavares ⓡ
Adewusi Mendonça

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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 control trial among 1988 cashew producers in 290 villages in Guinea-Bissau. Treated producers received weekly messages to their mobiles during the trading season in 2020. The messages provided up-to-date market news, farmgate prices, and gave marketing advice. We found that treated producers sold their cashews more frequently relative to the producers in other experimental groups, who tend to sell their cashews in a single transaction. We only found a small positive and not statistically significant effect on average prices during this trading season. However, treated producers earned more from all sales and barters, relative to the control group mean. We explore several mechanisms to understand our results. We find evidence that between-village spillovers may lead us to underestimate treatment effects on average prices within treated villages. We found no evidence suggesting that treated producers changed their buyers, the location of their sales, better record keeping, or different attitudes towards risk. Given the low cost of our intervention, market information can be a cost-effective tool to increase producers' revenues.

[PRE-ANALYSIS PLAN]

Giulio Schinaia's Job Market Paper. 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 Commision; Raiss Laval Tavares: Universidade Lusofona de Bissau and Ministry of Economy and Finance of Guinea-Bissau; Mendonça: Ministry of Economy and Finance of Guinea-Bissau.

1 Introduction

Agricultural commodity markets are characterised by substantial price fluctuations over time (Deaton and Laroque, 1992). These fluctuations significantly impact the income of smallholder farmers in low-income countries. In particular, producers of export cash crops may be particularly affected by these fluctuations, which are determined by international markets. Smallholder producers typically have less access to prices and market information relative to their buyers, who are more integrated into global value chains or wholesale markets (Fafchamps and Hill, 2008). In these contexts, imperfect information may weaken the bargaining power of producers relative to their buyers and lower the prices producers earn. Information communication technologies can improve market performance by increasing the availability of information (e.g. Fabregas et al. 2019; Aker and Mbiti 2010). Information can reduce price uncertainty, potentially enabling farmers to increase their bargaining power. But if markets are competitive, information may not necessarily alter the prices and income earned by producers (Casaburi and Reed, 2022).

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 country'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 locally-sourced 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 generally 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. During the period of our intervention, these messages were only directly accessible to treated producers in the country. Because there were both treated and untreated producers in treated villages, our research design also allowed us to estimate within-village spillover effects. We evaluated this intervention during the 2020 trading season by analyzing a set of outcomes collected during in-person interviews between April and May 2021. Our analysis plan was registered

[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.

before we completed these in-person interviews.[2]

Besides its intrinsic interest, the Bissau-Guinean context is a particularly useful one to understand whether reducing price uncertainty can help producers to bargain for better deals. The price, timing and frequency of sales can have an important impact on producers' incomes, while there are few spatial arbitrage opportunities. First, producers make a high-stake 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. Raw cashew nuts are not highly perishable and can be stored within the same trading season. However, producers do not store cashews across different years, as the value of the stock would depreciate heavily over longer time-horizons. Third, the timing of these transactions matters. 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. Most producers sell their cashew in their own village to either locally-based intermediaries or itinerant traders, with very few producers transacting in centralised wholesale markets.

We have four main results from our analysis of the effects of the market information system during the 2020 trading season. First, we find that the information system marginally increased farmgate prices. While we find a small and not statistically significant intention-to-treat effect on average prices comparing treated producers with control producers, *between*-village spillovers can explain why did not find a larger increase in average prices. Our intention-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 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. 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. Third, we find that treated producers start selling some of

[2] See `https://www.socialscienceregistry.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).

their stock earlier, despite prices being lower at the beginning of the trading season. These earlier sales are consistent with the treatment reducing the reservation price, making treated producers 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. Fourth, treated producers earned 21% higher revenue from all sales and barters, 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 barters, 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.

We explore several potential mechanisms that may drive our results. First, we interpret the changes in the timing of sales as evidence that reducing price uncertainty makes producers attempt less risky sales strategies. 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. Second, treated producers report are less likely to rely on intermediaries 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. Third, we find no evidence that producers changed where or to whom they sold their cashew nuts. In our context and at the time of our intervention, which coincided with the onset of the COVID-19 pandemic, it may have been challenging for producers to sell their stocks outside of their villages.[4] Fourth, we are also able to rule out the possibility that our effects are due to better record-keeping induced by our intervention or by changes in preference parameters, such as risk aversion. Fifth, we rule out that salience of our messages induced produced to sell more frequently. We do not observe producers being more likely to report a transaction within two days of having received a message, leveraging transaction diaries that a subset of producers filled with the exact date of their transaction. Finally, exploiting interviews conducted with intermediaries across a sub-sample of study villages, we find treatment effects that are in direction and magnitude consistent among buyers with those obtained from producers' data. The intermediary data suggests that treatment villages had higher pass-through than control villages, as intermediaries earned lower margins but bought higher quantities, although these differences are not statistically significant.

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 might lack in-

[4] In appendix J, we provide more details on the differences between the 2019 and 2020 trading seasons to describe the overall changes that occurred at the onset of the COVID-19 pandemic.

formation on both *where* and *when* to sell their output to maximize profits. Previous studies have shown that, with the introduction of new information technologies, producers have been better able to decide *where* to sell (Aker 2010; Jensen 2007). 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 (Burke et al. 2019; Aggarwal et al. 2018; Kadjo et al. 2018). But, 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 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

[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 Hildebrandt et al. (2021) 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) 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.

(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 explicitly designed to estimate potential *within*-cluster spillovers of our intervention.[8] Like these papers, our research design allows us to estimate spillover effects *within*-clusters.[9] 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 (Hildebrandt et al., 2021; 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

Guinea-Bissau is a price-taker in the international market for raw cashew nutes. In 2019, Guinea-Bissau was the fifth largest producer of raw cashew nuts in the world. In the last decades, the country has been heavily dependent upon the production and commercialization of raw cashew nuts. Cashew nuts are the main export product, accounting for over 90% of the total official exports. Only 10% of national production goes to the domestic market (processing units) and, more than 90% is exported to the international market, mostly to India, Vietnam, and China.[10] The national cashew-nut market, as with many agricultural commodities, is particularly

[8] 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.

[9] 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.

[10] Although most of the production is exported as raw cashew nuts, there are also exports of processed nuts. A small portion of cashew nuts are consumed domestically, but the most dynamic and profitable markets are in importing high-income countries.

influenced by the dynamics of international supply and demand. However, as shown in Figure 1, the margins between export and farmgate prices are variable and large across different years, which suggest that market imperfections may be reducing pass-through rates to producers.[11]

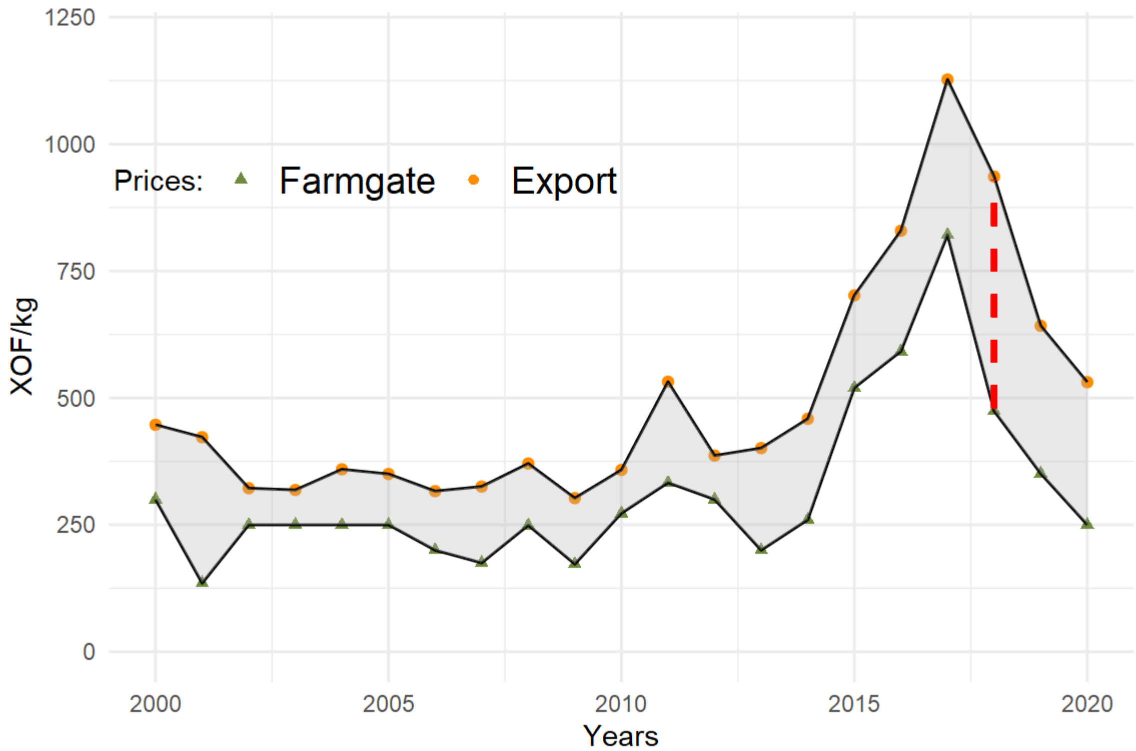

Figure 1: Margin between export and farmgate prices across years.[a]

[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.

Margins between export and farmgate prices vary substantially also within the same year. As shown in figure 2, between 2019 and 2021 export prices have remained relatively stable during the trading season. There was a reduction in export prices between 2019 and 2020.[12] In 2020, prices were particularly low at the farmgate, with a higher margin compared to previous years. Farmgate prices have varied substantially within the trading seasons. In general, farmgate prices tend to increase towards the end of the trading season.

[11] 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.

[12] Nominal export prices in 2021 denominated in USD were above the 2020 levels, but due a depreciation of the dollar in 2021, they appear lower than in 2020 once converted to XOF.

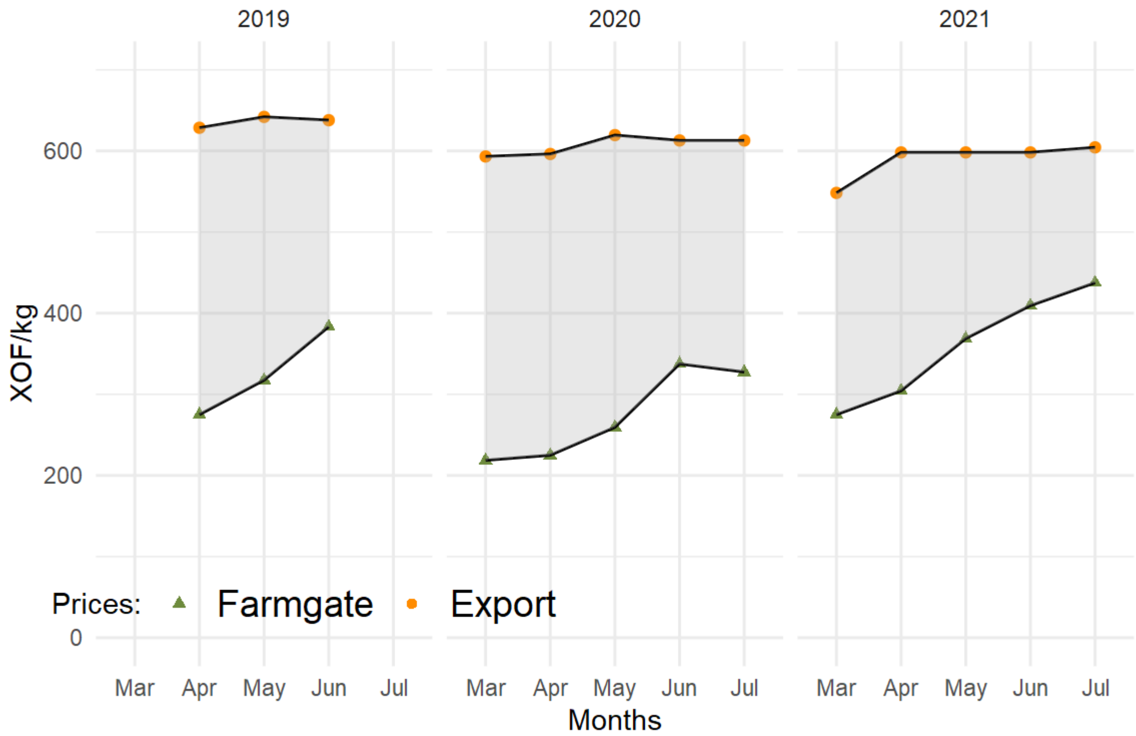

Figure 2: Margin between export and farmgate prices *within* years.[a]

[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.

The majority (85%) of raw cashew nuts producers are smallholders. At least 40% of households in the country are involved in cashew production. In the rest of this section, baseline data was used to characterize producers in this market. Details of the baseline survey are provided in the next section.

Our descriptive statistics, in Table 1, show that 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; all producers lived in rural areas. 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. The size of the median cashew plantation was around 3.4 hectares.[13] About two thirds of our respondents reported having no difficulties storing their cashew production. In general, producers sell their entire annual harvest during the same trading season between March and June. Almost no producer stores cashews across seasons because their quality

[13] Appendix E briefly describes the costs of production involved in cashew production, which are mostly related to labour.

would deteriorate significantly during the rainy season, which starts in July.

Table 1: Sample characteristics at baseline

	Mean	St. Dev.	25th percentile	Median	75th percentile
Individual-level characteristics:					
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
Area plantation (hectares)	3.45	2.58	2.00	3.00	4.00
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
1 if cashews are the main source of income	0.80	0.40	1.00	1.00	1.00
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 acted as intermediary	0.04	0.18	0.00	0.00	0.00
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
Total quantity produced (kg)	1526.59	1627.36	669.50	1125.00	1935.00
1 if borrowed money or rice	0.35	0.48	0.00	0.00	1.00
1 if exchanged cashew for rice	0.34	0.47	0.00	0.00	1.00
Total quantity exchanged (kg)	146.73	303.85	0.00	0.00	180.00
Total quantity paid for loans (kg)	54.43	175.52	0.00	0.00	45.00
Village-level characteristics:					
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
Distance in km to the nearest primary road	20.27	18.25	6.37	15.48	28.06
Distance in km to the nearest secondary road	3.57	2.99	1.11	2.91	4.86
Distance in km to the nearest road	2.99	2.55	0.98	2.48	4.15
Observations	1988				

Sales

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. For 80% of our sample, cashew sales constitute the primary source of 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.[14] 54% of buyers are traders that temporarily visit the villages, whilst 39% are intermediaries that live regularly in the same village as the producers. In our sample around 4% of producers reported acting as intermediaries, buying from other producers and selling onto other traders quantities that range between 2-20 metric tons, much larger than what individual producers sell on average. 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.

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, the average producer had reported producing about 1,500 kg of cashews. In 2019, we estimate that on average 13% of marketable production was used for barters 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.

Village characteristics

Villages in our study are all in relatively remote rural areas. The median village has 24 cashew producers and 3 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,

[14] According to qualitative interviews implemented in some of the villages in our sample.

Bissau, where most of the raw cashew nuts are exported from.[15] The (Eucledian) distance between a village and a paved road is on average 3 km. Villages are on average 20 km away from a primary road, which is suitable for large trucks that transport cashews to the the main export hub in the capital, Bissau. Villages are closer to secondary roads, which are suitable for smaller vehicles such as cars or motorcycle cargos and tend to be of worse quality.[16]

COVID-19 in Guinea-Bissau and the 2020 cashew trading season

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.

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.

In this context, 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 does take 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 our sample, we find that decreases in both farmgate prices and quantity produced resulted in a 47% decrease in producers' average revenue from cashew from 2019 to 2020.[17] The introduction of the market information system (*n'kalô*) and the impact evaluation reported in this paper took place in a particularly difficult year for cashew producers. In appendix J we provide further details on the differences in producers' outcomes between 2019 and 2020.

[15] 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.

[16] Appendix figure 6 shows the distribution of villages relative to the main road network.

[17] We estimate this difference using only data for producers in the control group.

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 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 $[\overline{x}, \underline{x}]$. 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, $W(.)$ 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.[18]

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

[18] 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.

its associated first order condition:

$$p_1 W'(p_1 q_1) = \delta p_2^* W'(p_2(1 - q_1)) \tag{1}$$

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 will depend on the interplay between wealth and substitution effects. The wealth effect is captured by the concativity of $W(.)$. 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 the price increases, $W'(.)$ has to fall, which means that q_1^* will have to increase. The net effect will depend on the curvature of $W(.)$. 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 $x_t \in [\underline{x}, \overline{x}]$. 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:

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

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 2020 trading season, 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.

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

In this case, treated producers may be selling more in the first period, relative to uninformed producers, if wealth effects dominate. Relative to the uninformed producers, treated ones might earn lower prices in the first period, but higher prices in the second period.
Instead, consider the case where the export prices across the two periods are independent, then $\alpha_H^H = \alpha_L^H$. The current signal will not change the probability of selling at a higher price in the second period. This might be a plausible assumption for the 2020 trading season, given the uncertainty that producers faced at the outset of the trading season, and the prospect of making no trades at all because of the restrictions imposed by the government. In this case, the right-hand side of equation (2) is independent of the signal.
Given the concavity of $W(.)$, 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.

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 marketing season, via weekly short text messages or robocalls between April and August 2020.[19]

The market information delivered through those weekly messages consisted of: (i) the current range of farmgate raw cashew nut prices across regions in Guinea-

[19] The 2020 marketing season started on the 27^{th} 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.

Bissau, (ii) important news about the market, and (iii) a sales advice based on the expected market trends.[20]

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

Our weekly short text messages were sent to those randomly selected producers between the 1st of April and the 8th of May. From the 16th of May, the weekly messages were sent via robocalls.[21] Finally, since the 26th of June, the information was also available on demand, as the audio content of the weekly robocalls could be accessed using an Interactive Voice Response (IVR) service.[22]

4.2 Study design

We implemented a two-stage sampling and randomisation to conduct our study. We first selected a random sample of villages (and producers within those villages) with the aim of picking villages that were as far from each other as possible.

We then randomly allocated two thirds of villages to receive our intervention, and one third to act as our pure control group. In treated villages, we randomly allocated the intervention only to a subset of the sampled producers.

Figure 3 shows how we allocated participants across treatment, spillover and control groups. In appendix , we provide the details of how we conducted the sampling and randomisation.

[20] The content of the messages was developed weekly, in line with the methodology of the *n'kalô* service, a Market Information System already operating in other West African countries, which we introduced to Guinea-Bissau in collaboration with the Ministry of Finance. In appendix table 29 we show the full content of messages delivered in 2020.

[21] Section A.3 details our randomisation strategy.

[22] The Interactive Voice Response (IVR) service was active for only two months prior to the end of the 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.

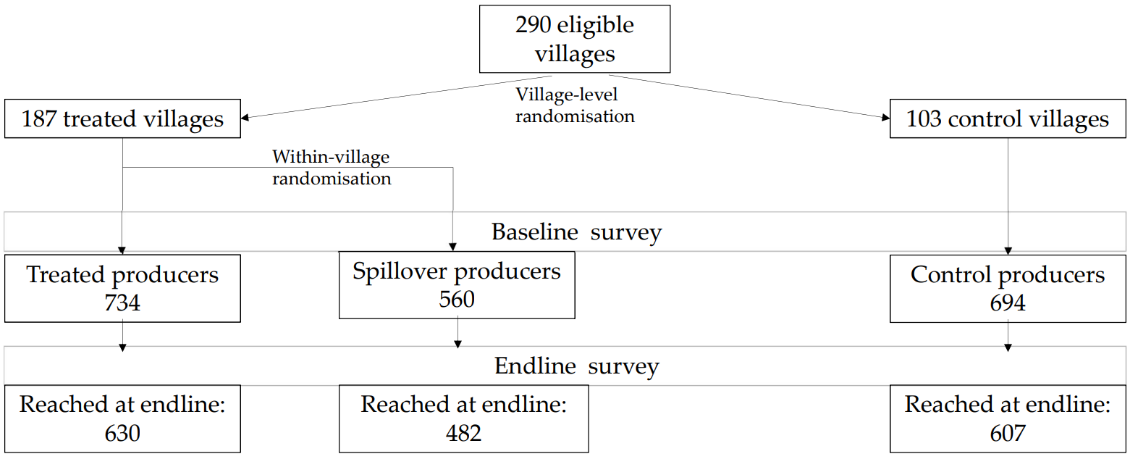

Figure 3: Flowchart of participants through the study.

Our analysis of the effects of the intervention is based on outcomes collected during an in-person survey with producers in every village in our study, between April and May 2021. 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.

Finally, after completing the survey, producers in both the treatment and control villages were provided with information on the service and were shown how to subscribe as fee-paying users to receive information during the remainder of the 2021 trading season.

4.3 Empirical strategy

We estimate models of the form:

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

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 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.

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;

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 differentially affected by the treatment relative to untreated producers living in the same village.[23]

4.5 Experimental integrity

In appendix B we present the baseline balance, compliance, and attrition from the endline survey.

Tables 12 and 13 show that our baseline sample is relatively balanced across treatment arms for our outcome variables of interest. The differences between the mean of 7 of the 27 outcome variables are statistically significant across groups, but these differences are small, as none of the pairwise standardised differences in means are larger than 0.22 standard deviations. Also, only three of these differences are statistically significant at the 5% level.
Our empirical strategy includes the baseline value of the outcomes as control variables, which would allay concerns that unbalance in the outcomes of interest is affecting our estimates of the treatment effects.

Tables 14 and 16 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 10 of the 25 producer-level characteristics are statistically significant across treatment groups. However, these differences are small, as none of the pairwise standardised differences in means are larger than

[23] In section 6.1, we test whether spillovers may have occurred *between-villages*. This type of spillovers is theoretically plausible and, if we assumed them to be positive, could bias downward our ITT effects, as found in other contexts (Hildebrandt et al., 2021).

0.20 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 substantially unbalanced at baseline, and report these results in section C.2 in the Appendix. None of our conclusions are affected by these baseline unbalances.

Overall attrition in the in-person follow-up survey was 14% and it was not correlated with treatment assignment, as shown in table 17 in the Appendix.

To measure take-up of the intervention, we define a dummy variable equal to one if the respondent received at least 8 messages during the 2020 trading season. To construct this variable we use administrative data collected by the mobile network operator (MTN) that delivered the content of our messages. We include both robocalls listened for at least 45 seconds and SMS received to define this measure of take-up. We complement the administrative data with a survey-based answer to the question "Did you use the service in 2020?". We report in table 18 our administrative measure of compliance by treatment arm and in table 9 the survey-based measure. According to the administrative data 53% of treated producers received at least 8 messages, with no messages sent to other producers. Figure 5 shows the number of robocalls received by the treated producers during the 2020 season. Based on the survey responses, 25% of treated producers reported having used the service, with 2% and 3% of control and spillover producers, respectively, also reporting having used the service. 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 endline survey. 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, exchanged for rice, or used to repay loans). The analysis follows our pre-analysis plan, and we note where we may have deviated from it.

5.1 Prices and revenue

Treated producers earned a marginally higher revenue relative to the control group, despite our intervention having had no significant increase on the average price of sales among treated producers. Table 2 shows our results from the endline survey on prices and revenue from sales and exchanges. The estimation sample includes all the producers that we reached at endline. In order to not condition our outcome on the decision to sell, we recode the price per sale 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.

Our first finding is that the intervention did not increase the average price of sales among treated producers. The first row shows that producers in the treatment group reported sale prices that were, on average, 2 XOF higher than those of the control group (column 2) , but this difference represents less than 1% of the control mean and is not statistically significant. We find no difference between the treatment group and the spillover group in the average prices.
To better understand the failure of our intervention to increase average producer prices, we plot the distribution of prices across treatment arms in figure 4. Two patterns emerge from the distribution of prices. The y-axis plots the count of producers we interviewed during the 2021 follow-up. First, control producers appear to report more frequently the median price of 300 XOF/kg relative to both treatment and spillover group. Second, differences in outcomes are likely to be due to changes on the intensive margin (through additional sales), rather than the extensive margin (making any sale at all). 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.[24] Moreover, we can see that the variation in the average price received by producers is not significantly different across treatments.[25]

[24] To make the outcome unconditional, we recoded the average price to be equal to zero for producers that did not report any monetary sales.

[25] More formally, we cannot reject the null hypothesis that the variance of average prices is different across treatment groups using an F-test. This is also true if we pool treatment and spillover producers together and compare their price variance relative to the producers in the control villages. We also do not find, but do not report, village-level treatment effect on the coefficient of variation of prices across the season.

Table 2: Results - Prices and revenue

	Control (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover
Price per kg	295.71	2.07	2.33	-0.26
	(104.36)	(3.46)	(3.35)	(3.30)
	1587	[0.55]	[0.49]	[0.94]
Value of all sales and exchanges	287785.22	61876.32**	44172.78*	17703.54
	(315352.77)	(25318.49)	(26625.26)	(22487.00)
	1521	[0.05]**	[0.20]	[0.57]
Value of all sales	233494.84	51335.03**	24128.93	27206.10
	(292721.24)	(22677.17)	(23273.58)	(19950.57)
	1591	[0.05]**	[0.40]	[0.57]
Value of exchanges	52325.81	10597.78	16866.04*	-6268.26
	(88037.41)	(8099.23)	(9701.49)	(7302.75)
	1636	[0.25]	[0.20]	[0.57]

Notes: producer-level intention-to-treat (ITT) estimates 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.

Despite the failure to increase average prices, the second row in table 2 shows that the total value of cashews that were either sold or exchanged (directly for rice) is 61,176 XOF (q-value: 0.05) higher among producers in the treated group relative to the control mean. This increase in the value of cashews sold or exchanged corresponds to 21% of the control group mean.[26]

[26] 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.

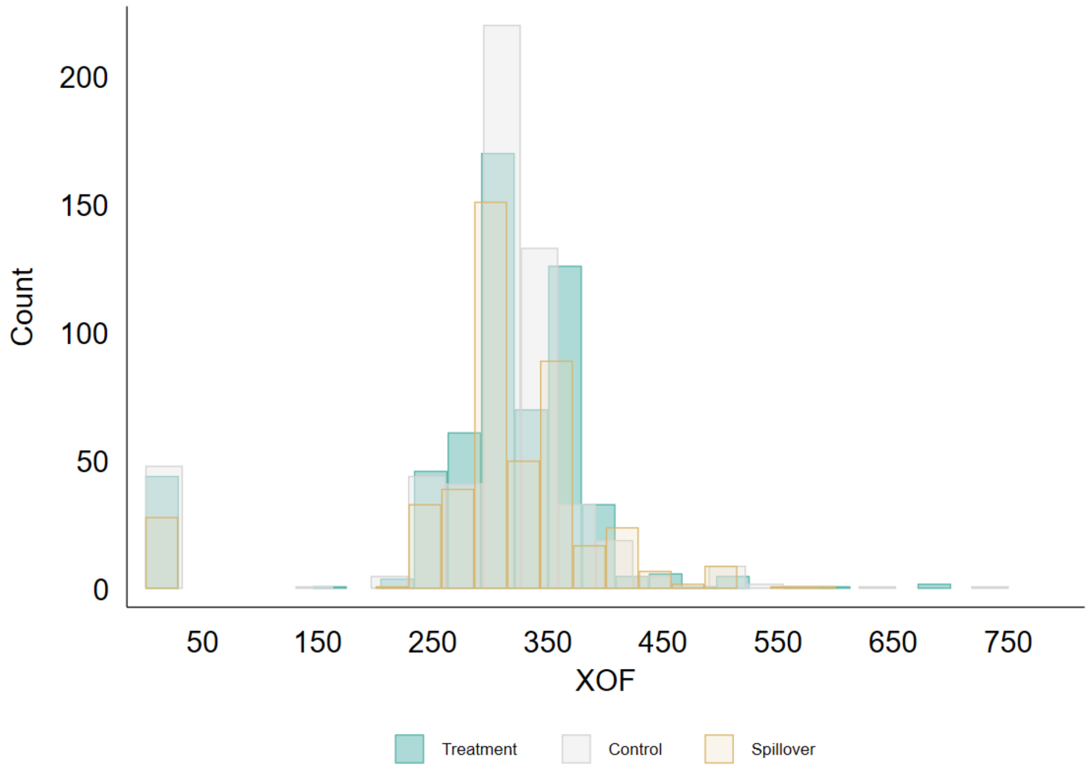

Figure 4: Distribution of average producer prices by treatment arm.[a]

[a] Source: Endline survey. Prices are reported in nominal West African CFA francs (XOF). We recode the price per sale to be equal to zero for producers that did not conduct any sales. Count refers to the number of producers in the sample.

We note that this estimate is noisy, judging from the standard error of the coefficient, though we find a similar pattern in our robustness checks. When we winzorize the total value of sales at the 99th and 95th percentiles, respectively in tables 19 and 21 in the Appendix, the treatement effect is smaller and not statistically significant, corresponding to about 8-10% of the control group mean.
Our estimates of the effect on revenue are smaller than the effect observed in table 40 once we control for unbalanced producer- and village-level characteristics or once we estimate the regression using a post-double selection shrinkage and selection operator (PDSLASSO), respectively in table 23 and table 25 in the Appendix.[27]
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. The spillover effect corresponds to around 15% of the control group mean, but is only statistically significant at the 10% level according to naive p-values. The coefficient decreases and loses statistical significance in our

[27] These coefficients are still significant at the 5% using naive p-values, but not significant once we correct for multiple-hypothesis testing.

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 show that an increase in the value of monetary sales accounts for the treatment effects on the sum of these two components. Treated producers obtain on average 51,335 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. In our robustness checks, we find similar but smaller effects of the intervention, which remain significant at the 10% level, even when we windsorize this variable at the 95^{th} percentile. Our treatment effect estimate on the value of exchanges remains around 20% of the control mean and not statistically significant across our different robustness checks.
We find that producers in the spillover group report higher value of cashews exchanged for rice relative to the control group, 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.
Overall, while average prices did not increase substantially for the treated producers, our results indicate that the intervention increased the value of sales and exchanges, mostly through an increase in the value of sales.

5.2 Quantity sold or exchanged, for rice or loans

To unpack our results on price and revenues, we next analyse the producers' quantities of cashews sold, exchanged, or used in interlinked credit transactions. Changes in the amounts of cashews sold or exchanged can help us understand our effects on revenue, given that we found no significant increase on average prices. Indeed, in table 3 we see that treatment group sell on average 142 kg (q-value: 0.05) more than those in the control group, a 20% increase relative to the control group mean.

Table 3: Results - Cashew sales, exchanges, and loans

	Control (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover
Total quantity sold	708.21	141.75**	63.53	78.21
	(855.14)	(61.11)	(62.11)	(54.72)
	1622	[0.05]*	[0.45]	[0.38]
Number of sales	1.49	0.21***	0.07	0.14**
	(1.01)	(0.06)	(0.06)	(0.06)
	1693	[0.00]***	[0.45]	[0.10]*
1 if exchanged cashew for rice	0.56	-0.01	-0.03	0.03
	(0.50)	(0.03)	(0.03)	(0.03)
	1706	[0.85]	[0.45]	[0.59]
Total quantity exchanged	217.70	19.62	20.91	-1.30
	(323.48)	(24.38)	(25.70)	(23.10)
	1681	[0.53]	[0.45]	[0.96]
Share of quantity sold over quantity exchanged and sold	0.73	0.02	0.02	0.00
	(0.32)	(0.02)	(0.02)	(0.02)
	1604	[0.53]	[0.45]	[0.96]
Loans:				
1 if borrowed money or rice	0.31	0.02	0.03	-0.01
	(0.46)	(0.03)	(0.03)	(0.03)
	1702	[0.65]	[0.57]	[0.75]
Implicit price of loans per kg of cashews	58.96	12.62**	3.30	9.33
	(142.79)	(5.49)	(6.13)	(5.79)
	1645	[0.09]*	[0.59]	[0.41]
Total quantity paid for loans	44.09	-3.23	7.00	-10.24
	(106.88)	(7.08)	(8.99)	(8.11)
	1686	[0.65]	[0.58]	[0.41]
Share of quantity sold over quantity exchanged and paid in loans	0.86	0.01	0.03	-0.02
	(0.29)	(0.02)	(0.02)	(0.02)
	1599	[0.65]	[0.49]	[0.44]

Notes: producer-level intention-to-treat (ITT) estimates 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.

In the four robustness specifications the coefficient remains positive and between 8-15% of control mean, but statistically significant at the 10% according to naive p-values. In our robustness checks, the effect is smaller when controlling for un-

balanced producer- and village-level characteristics and estimating the regression using PDSLASSO (in tables 23 and 25 in the Appendix) and is further reduced once we winzorize the outcomes at the 99th and 95th percentiles (in tables 19 and 21 in the Appendix). We do not find statistically significant differences between the control and spillover producers in the amount of cashews sold. The spillover is positive and lower than our treatment effects.
Our intervention induced producers to sell more frequently their cashews relative to the control and spillover groups. Treated producers sell their cashews more times during the season, with the average number of sales being 0.21 higher (q-value: 0.00) relative to the control group, which represents 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 0.14 difference between the treatment and the spillover group is also robust to our alternative checks and remains statistically significant at the 5% level.
This behaviour is consistent with the advice provided by our intervention, which advised producers to sell cashews in multiple sales, given an expected increase in prices in the latter half of the trading season. We do not find an increase in the number of sales among producers in the spillover group. The estimated spillover effect is positive, smaller than for the treatment group but not statistically significant.

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 not statistically significant. We find no treatment or spillover effects of the intervention on the share of the quantity sold over the quantity exchanged and sold.

When looking at the impact of the treatment on producers' credit behavior we see that treated producers are not more likely to borrow money or rice through cashew nut repayments. The average quantity of cashews paid for loans is not different across different groups, and we also do not find differences in the share of quantity sold over the quantity exchanged and paid in loans.
We find that treated producers earned a marginally higher implicit value of the loans repaid through cashew nuts relative to control producers. We are cautious in interpreting this outcome, because it is only observed in around 30% of our sample, and recoded to zero for producers that did not engage in any loans.[28]

6 Mechanisms

In this section, we shed some light on which mechanisms may be driving our results. We test for *between*-spillovers to see whether the lack of price differences

[28] In our regression, we add as an additional control an indicator equal to one for producers whose implicit value of loans we had recoded to be equal to zero.

may have been attenuated by the information affecting control producers, although we had not pre-specified this analysis. Next, we follow our pre-analysis plan, to look at a number of different hypothesis in turn. First, we look at the timing of the transactions made by producers. We also explore whether the treatment changed producers' knowledge about other information related to the cashew market, or increased producers' bargaining power. Third, we check whether the increase in revenue is due to a change in the amount of cashews produced. Fourth, we 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.

To provide additional sources of evidence that can corroborate our results, in appendix G and H we show additional 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. These additional sources of data are less powered to detect statistically significant differences between treated and untreated producers but are broadly consistent with our main findings.

6.1 Spillovers across villages

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 Hildebrandt et al. (2021) 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, conditional on the number of villages in our sample within that radius, significantly changes our outcomes of interest beyond the producer-level treatment assignment.[29] 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.

In order to pick the most relevant radius distance, we estimate the effect of treatment intensity within a series of non-overlapping doughnuts, $d = 1, ..., D$, 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 $r \in \{0, 1\}$; . . .; with ten doughnuts ($d = 1, 2, ..., 10$) with $r \in \{0, 1, . . . , 9\}$, 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).[30]

[29] This specification is similar to Miguel and Kremer (2004).

[30] 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.

Table 4: Spatial spillovers on prices and quantity sold

	(1) Price	(2) Price	(3) Quantity sold	(4) Quantity sold
Treatment	2.16	7.09*	140.62**	94.17
	(3.37)	(4.08)	(61.25)	(69.35)
Spillover	2.49	6.92*	62.28	51.32
	(3.27)	(4.01)	(61.29)	(75.81)
Treated producers within 0-5km	2.03*	3.96***	-4.21	-17.45
	(1.20)	(1.41)	(16.17)	(27.88)
Villages within 0-5km	-6.22	-6.11	25.32	24.86
	(4.72)	(4.62)	(55.92)	(55.50)
Treatment*Treated producers within 0-5km		-2.90**		27.39
		(1.36)		(28.32)
Spillover*Treated producers within 0-5km		-2.59*		6.93
		(1.42)		(29.63)
# Obs.	1587	1587	1622	1622
P-value: Treat = Spillover	.918	.966	.143	.534
P-value (Interaction): Treat = Spillover		.742		.41
Control group mean	295.71	295.71	708.21	708.21
Control group st. dev.	104.26	104.26	855.14	855.14

Notes: OLS estimates of between-village effects, controlling for exogenous spatial treatment intensity. Each column represents a separate regression. The first three rows represent coefficients on household-level indicators for treatment assignment. The fourth row reports estimates of the coefficient π_2^d from equation 5 that calculates the effect of every additional treated within a radius of 0-5km of the observation. The radius of 0-5km 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. Outcome variables are listed across columns. The unit of observation is the producer. Regressions control for randomisation tripled fixed effects and baseline value of the outcomes. Conley (1999) standard errors are in parentheses, accounting for spatial correlation within a 5km radius. Stars on the coefficient estimates reflect unadjusted p-values. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level. Bottom rows displays the mean, standard deviation for the control group, and total number of observations.

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

$$\begin{aligned} y_{iv} =& treatment_{iv} \cdot \beta_1 + spillover_{iv} \cdot \delta_1 + y_{0iv} \cdot \gamma_1 \\ & \sum_{d=1}^{D} \left(\mathrm{V}^d_{iv\neg v} \cdot \pi_1^d + \mathrm{TP}^d_{iv\neg v} \cdot \pi_2^d \right) + \alpha_v + \epsilon_{iv} \end{aligned} \tag{5}$$

where variables are defined as in equation 4, $\mathrm{TP}^d_{iv\neg v}$ is the total amount of producers *assigned* to treatment within doughnut d of producer i (excluding treated producers in village v), and $\mathrm{V}^d_{iv\neg v}$ is the number of villages within doughnut d (excluding village v). Conditional on the number of villages within a given radius, the total

amount of treated individuals that are within $d$km is exogenous.[31] 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^d gives us the (indirect) effect of the total amount of treated individuals within the radii defined for doughnut d *between*-villages.
For analyses using treatment intensity within doughnuts, we account for spatial dependence using Conley standard errors with a uniform kernel up to the boundary of the largest doughnut (Conley, 1999).

In table 4 we report estimates of our test for *between*-village spillovers. We focus on our focal outcomes: average price earned by producers and the total quantity sold for cash. 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 $r + c = 5$.[32] We therefore focus on presenting results from this specification. To illustrate how treatment intensity varies across our sample, Figure 6 shows the spatial distribution of villages and plots a 5km around each village in our sample. The sum of all villages that overlap within the buffer of a given village represent $V_{iv\neg v}^d$ for that particular village.[33] In column 1 and 3, we report estimates of β_1, δ_1, π_1^d, π_2^d. In columns 2 and 4 of table 4, we further interact our treatment indicators with the count of treated producers within 0-5km, to check how the indirect effects vary with the direct ones.

Farmgate prices in more intensely treated areas are higher than less intensely treated ones. Our estimate of π_1^d in column 1 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. In column 3, we do not find the intervention having affected the quantity sold across villages. The estimate corresponds to 0.5% reduction in the amount sold, but it is not statistically significant.

These spillovers are positively affecting producers in control villages. We see in column 2 that 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, statistically significant and correspond to about 1% of the

[31] 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.

[32] 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.

[33] 40% of villages have another sampled village within a 5km distance, 9% of villages have two sampled villages within a 5km distance, 1% had three sampled villages within a 5km, and the remaining 50% had none. 27% of villages have another treated village within a 5km distance, 8% of villages have two treated villages within a 5km distance, and the remaining 65% had none.

control mean. Instead, the direct effects in treated villages that are not close to other treated villages are positive and statistically significant at the 10% level. Interestingly, 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 an increase in average prices in treated villages. Finally, in column 4, we find that the estimated coefficients on the effect of treatment intensity on the quantity sold go in the opposite direction compared to the effect on prices. The interaction coefficients are of a larger relative size in the quantity sold among treated producers in villages that were closer to other treated villages (3.4-3.8% of the control group mean) but noisier and not statistically significant. 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.[34] Fafchamps and Minten (2012) described this mechanism potentially having a role in interventions that reduce asymmetric information between buyers and sellers of agricultural commodities.

6.2 Marketing behaviour and timing of sales

To better understand possible ways in which our treatment might have affected producers' behaviors and outcomes during the trading season, we next turn to the analysis of different decisions related to sales.

When looking at the timing of sales and exchanges in table 5, treated producers reported selling relatively more and exchanging less cashew for rice during the start of the trading season (in or before April 2020) relative to the control group.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.[35]

Treated producers were 3 percentage points more likely to report exchanging cashew for rice towards the end of the trading season (July 2020 or later), though this outcome is extremely low among the control group (only 1% of the control group reported exchanging cashews for rice in this period).

[34] In line with this hypothesis, in appendix table 32, we found that control villages reported receiving more itinerant traders than treated villages.

[35] Appendix table 29 shows the content of the messages sent in 2020.

Table 5: Results - Timing

	Control (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover
1 if sold cashews in April 2020 or before	0.15	0.04**	0.02	0.02
	(0.35)	(0.02)	(0.02)	(0.03)
	1361	[0.17]	[0.80]	[0.99]
1 if sold cashews in May 2020	0.51	0.02	-0.01	0.03
	(0.50)	(0.03)	(0.04)	(0.03)
	1361	[0.71]	[0.88]	[0.99]
1 if sold cashews in June 2020	0.49	0.05	0.05	-0.00
	(0.50)	(0.03)	(0.03)	(0.03)
	1361	[0.29]	[0.33]	[0.99]
1 if sold cashews in July 2020 or later	0.07	0.01	0.01	0.00
	(0.25)	(0.02)	(0.02)	(0.02)
	1361	[0.71]	[0.81]	[0.99]
1 if exchanged rice in April 2020 or before	0.09	-0.03*	-0.03*	0.00
	(0.28)	(0.01)	(0.02)	(0.02)
	1591	[0.17]	[0.21]	[0.99]
1 if exchanged rice in May 2020	0.23	-0.01	-0.00	-0.00
	(0.42)	(0.02)	(0.03)	(0.02)
	1591	[0.81]	[0.90]	[0.99]
1 if exchanged rice in June 2020	0.26	-0.01	-0.02	0.01
	(0.44)	(0.03)	(0.03)	(0.02)
	1591	[0.76]	[0.80]	[0.99]
1 if exchanged rice in July 2020 or later	0.01	0.03***	0.03***	0.00
	(0.11)	(0.01)	(0.01)	(0.01)
	1591	[0.01]**	[0.05]*	[0.99]
Not pre-specified:				
Quantity of cashews sold in May 2020 or before	324.45	93.90**	2.21	91.69*
	(597.20)	(44.06)	(44.02)	(46.78)
	1288	[0.07]*	[0.96]	[0.10]
Quantity of cashews sold in June 2020 or later	398.16	66.95	58.71	8.23
	(713.40)	(56.60)	(59.16)	(56.04)
	1288	[0.24]	[0.64]	[0.88]

Notes: producer-level intention-to-treat (ITT) estimates 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.

The difference in timing of sales is consistent with treated producers selling more than once during the trading season. When looking at the quantity sold prior to the official start of the trading season in 2020, we see in the bottom panel of table 5 that treated producers are selling a larger amount of cashews earlier relative to both control and spillover producers. From comparing the control group mean of the quantity sold in May 2020 or before (325 kg) relative to the quantity sold afterwards (398 kg) we can see that earlier sales are, on average, smaller relative to later ones. Thus, treated producers that had been informed by the intervention about current prices and the prospect of prices raising in the future, sell a higher amount of cashews before the official start of the cashew season than those in the control or spillover groups.[36]

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 27, we report the producers' responses by treatment status. We had not pre-specified the analysis of the outcomes in this table. 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 line with the market advice provided by our intervention, which advised producers to spread their sales, treated producers were 7 percentage points less likely to report they had sold only once in order to pay for lumpy or urgent expenses relative to the control.

We asked producers both whether they had delayed or sped up sales because of expected price changes and to whom they sold most of their cashew nuts. We also checked whether producers were more likely to report selling their cashews to a buyer in neighbouring Senegal. After accounting for multiple-hypothesis testing, we found no evidence of any change due to our intervention in any of these decisions, as shown in table 6. However,the proportion of producers in the treatment group selling directly to a market was 0.02 lower than in the control group, which represents 50% of the control group mean, though the effect is only significant with naive p-values. We highlight the high proportion of our sample that reported delaying their sales in the expectation that prices would increase, almost 80% among control group producers.[37]

[36] Section 3 provides a conceptual framework to illustrate why treated producers may have sold more frequently and why they may have started selling earlier relative to untreated producers.

[37] As figure 2 illustrated, prices tend to increase towards the end of the trading season. In 2020, waiting for prices to increase could have been profitable for producers as prices did increase in the latter half of the trading season, when international trade increased after the initial tightening due to the COVID-19 pandemic.

Table 6: Results - Marketing behavior

	Control (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover
1 if delayed sales because expected price to raise	0.79	-0.01	-0.01	0.00
	(0.41)	(0.02)	(0.02)	(0.02)
	1703	[0.73]	[0.82]	[0.96]
1 if sped up sales because expected price to drop	0.27	0.04	0.03	0.00
	(0.44)	(0.02)	(0.02)	(0.03)
	1694	[0.34]	[0.47]	[0.96]
1 if sold most to local buyer	0.43	-0.02	0.04	-0.06*
	(0.50)	(0.03)	(0.03)	(0.03)
	1622	[0.73]	[0.47]	[0.45]
1 if sold most to itinerant buyer	0.43	0.04	0.01	0.04
	(0.50)	(0.03)	(0.03)	(0.03)
	1622	[0.34]	[0.82]	[0.77]
1 if sold most directly to a market	0.04	-0.02**	-0.02	-0.01
	(0.20)	(0.01)	(0.01)	(0.01)
	1622	[0.16]	[0.47]	[0.80]
1 if sold to or close to Senegal	0.01	0.00	-0.00	0.00
	(0.11)	(0.01)	(0.01)	(0.01)
	1693	[0.73]	[0.82]	[0.86]

Notes: producer-level intention-to-treat (ITT) estimates 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.

6.3 Bargaining and information updating

We next explored whether the information received by treated producers increased their bargaining power during sales negotiations. As shown in the bottom panel of table 7, the service did not make treated producers more likely to successfully bargain for a higher price with buyers. We found 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. Interestingly, however, we found that producers in the spillover group reported receiving offers from more buyers relative to the control group. 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.

Table 7: Results - Bargaining

	Control (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover
Number of succesful bargains	0.14	-0.00	-0.02	0.01
	(0.37)	(0.02)	(0.02)	(0.02)
	1690	[0.89]	[0.44]	[0.57]
1 if acted as intermediary	0.07	0.01	0.01	-0.01
	(0.26)	(0.01)	(0.02)	(0.02)
	1713	[0.89]	[0.44]	[0.57]
Number of potential buyers that made offers	2.91	0.08	0.30***	-0.21**
	(1.82)	(0.09)	(0.10)	(0.10)
	1671	[0.89]	[0.01]***	[0.12]

Notes: producer-level intention-to-treat (ITT) estimates 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.

We found no evidence that treated producers reported a higher willingness-to-pay for the service after having had free access to it during the 2020 trading season, relative to the control or spillover groups.[38] We do not interpret a lack of treatment effects on the willingness-to-pay for the service as a lack of appreciation for the service, given that the majority of the qualitative feedback we collected from treated producers on the quality of the service was positive.[39]

[38] This finding contrasts with the findings of Cole and Fernando (2021), who show that free access to a mobile-based agronomic advice service in India increased the willingness-to-pay for this service among treated users. Both contexts and type of interventions vary between these studies, so a direct comparison is only illustrative.

[39] We elicited willingness-to-pay through unincentivised hypothetical questions, which have been shown to perform as well as incentivised elicitation methods in other contexts (Shapiro et al., 2020).

Table 8: Results - Information updating

	Control (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover
Willingness to pay for the service	216.15	-24.01	-5.33	-18.68
	(400.00)	(18.68)	(26.88)	(28.56)
	1629	[0.40]	[0.84]	[0.86]
1 if thinks that the reference price is important in marketing	0.38	0.01	0.02	-0.01
	(0.49)	(0.03)	(0.03)	(0.03)
	1689	[0.69]	[0.69]	[0.86]
Absolute difference with the reference price in 2020	7.56	1.49	1.94	-0.45
	(13.72)	(1.70)	(1.98)	(2.52)
	1438	[0.51]	[0.69]	[0.86]
Expected sale price (for 2021)	512.48	-15.37	-7.21	-8.16
	(193.57)	(10.51)	(11.24)	(11.84)
	1580	[0.40]	[0.69]	[0.86]
Not pre-specified:				
Desired reference price	697.85	-16.71	-12.94	-3.77
	(277.93)	(16.31)	(17.14)	(14.41)
	1702	[0.31]	[0.45]	[0.81]
Expected prices (for 2020) minus actual prices	489.56	-16.56	30.38	-46.94**
	(312.67)	(16.37)	(19.07)	(21.02)
	1472	[0.31]	[0.22]	[0.10]
Number of producers informed of an offer received	6.75	0.77*	0.59	0.18
	(7.99)	(0.46)	(0.47)	(0.46)
	1506	[0.19]	[0.28]	[0.81]
Number of producers that shared price offers	4.86	0.80**	0.71*	0.09
	(8.70)	(0.38)	(0.38)	(0.38)
	1495	[0.15]	[0.22]	[0.81]

Notes: producer-level intention-to-treat (ITT) estimates 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.

We only find weak evidence that treated producers were better informed about relevant news about the cashew market in the trading season, as shown in table 8. Treated producers were not more likely to know the official reference price set by the government in the 2021 trading season relative to those in the control group. When asked to report what they thought was the reference price announced, the absolute difference between the reported price and the actual official reference price (360 XOF per kg) was not different across producers across the different experimental groups.[40] Producers' expected sale price for the 2021 trading season were generally

[40] We had asked about knowledge of the reference price for the 2021 trading season, which was announced by the government in the first week of April 2021.

high (above 500 XOF per kg in the control group), especially in comparison to the reference price. We found that treated producers had lower expected prices relative to both the spillover and control group, though these differences were not statistically significant. Expecting a lower price in 2021 could be interpreted as having more realistic expectations.

In the bottom panel of table 8, we explore four additional outcomes related to information updating that we had not included in our pre-analysis plan. Consistent with the pattern we had found with expected prices, we found that treated producers' ideal reference price for the trading season was also relatively lower than for other groups, though this difference is never statistically different from zero.
We also found that the gap between expected prices and realised prices has decreased for treated producers relative to the other producers. During the baseline survey at the end of 2019, we asked producers at which price they expected to sell most of their cashews during the 2020 trading season. We construct the the gap between their expected prices and the actual average in 2020 by taking the difference of the expectation and the realisation, which producers reported about the sales made in 2020. The reduction in the expected gap is only statistically significant with naive p-values between the treated and spillover producers, corresponding to about 10% of the control mean. Expectations in 2019 for the following trading season were over-optimistic. In 2020 treated producers earned prices that were marginally closer to their pre-existing expectations.[41]

We found only weak evidence of information-sharing among producers as a result of the intervention. Treated producers were marginally more likely to share information about prices to others, which could help us explain some of the spillover effects found in the previous section. Treated producers shared information about offers they had received to 0.77 more producers relative to the control group mean. We also found evidence that producers in our sample were more likely to receive information about prices from other producers. In particular, we found a positive increase of 0.8 more producers sharing information with treated producers. As treated producers are better informed, it is plausible that they could have been the source of advice for other producers that want to know if the offers they received are profitable.[42] However the increase in communication among producers is not significant once we account for multiple-hypothesis testing.

[41] We cannot control for the baseline value of the expectations gap because we did not measure expectations for the 2019, because our baseline survey took place after the end of the 2019 trading season.

[42] In appendix table 28 we further decomposed the number of producers with whom prices offers are discussed or shared and found that most of the discussion of offers occurs within producers from the same village. Although we cannot completely rule out the existence of communication between villages, which is less frequent.

Table 9: Results - Sources and content of advice received

	Control (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover
Self-reported engagement with the service:				
1 if heard of mobile MIS	0.14	0.30***	0.10***	0.20***
	(0.35)	(0.02)	(0.03)	(0.03)
	1714	[0.00]***	[0.00]***	[0.00]***
1 if used mobile MIS	0.02	0.25***	0.03	0.22***
	(0.16)	(0.02)	(0.02)	(0.02)
	1714	[0.00]***	[0.10]	[0.00]***
Source of advice:				
1 if receive market advice from family and friends	0.49	0.02	-0.02	0.04
	(0.50)	(0.03)	(0.03)	(0.03)
	1713	[0.46]	[0.57]	[0.20]
1 if receive market advice from intermediaries	0.17	-0.06***	-0.02	-0.04**
	(0.37)	(0.02)	(0.02)	(0.02)
	1713	[0.00]***	[0.57]	[0.05]**
1 if receive market advice from mobile messages	0.04	0.15***	0.01	0.14***
	(0.19)	(0.02)	(0.01)	(0.02)
	1713	[0.00]***	[0.57]	[0.00]***
1 if receive market advice from other sources	0.12	-0.04***	-0.02	-0.02
	(0.33)	(0.02)	(0.02)	(0.01)
	1713	[0.01]***	[0.57]	[0.20]
Content of advice:				
1 if receives advice on best time to sell	0.48	0.05	-0.04	0.08***
	(0.50)	(0.03)	(0.03)	(0.03)
	1700	[0.31]	[0.28]	[0.01]**
1 if receives advice on price to sell at	0.61	0.01	-0.05	0.06**
	(0.49)	(0.03)	(0.03)	(0.03)
	1706	[0.76]	[0.28]	[0.06]*
1 if receives advice on who to sell to	0.24	0.03	-0.01	0.04
	(0.42)	(0.02)	(0.02)	(0.02)
	1632	[0.42]	[0.67]	[0.11]

Notes: producer-level intention-to-treat (ITT) estimates 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.

Finally, we see that the source of market information has changed among treated producers, despite there being no significant differences in their beliefs and knowledge about the reference price. In table 9, we see that treated producers reported engaging with the service more than other producers. Both treated and spillover producers are more likely to have heard about the service at follow-up relative to control producers. Treated producers are more likely to report having used the service relative to both the spillover and control groups. Interestingly, treated producers reported relying less on intermediaries and other sources of information, but they were 15 percentage points more likely to rely on the messages they received on their phones as part of the intervention as a source of information.

6.4 Total quantity produced and alternative uses of cashew nuts

What accounts for the increase in the quantity sold? It takes at least three years for a newly planted tree to yield fruits and nuts, so it is implausible to think that the extra sales we documented in the treatment group may be due to an increase in the area cultivated or number of trees. The production of cashew nuts is relatively inelastic over time. Instead, we posit that it is more likely that producers may have diverted their existing stock towards large sales and away from other uses. We used responses from the module on different uses of raw cashew nuts to understand what may have caused the increase in the amount of cashews sold.

Table 10 shows that treated producers reported 184 kg more of cashew nuts across all possible uses, a 17% increase relative to the control group mean.[43] However, while these differences are statistically significant at the 5% level when using naive p-values, they are not robust to multiple-hypothesis testing. We found no effects of similar magnitude in the quantity produced in the spillover group. The positive increase in the total quantity produced is driven by the larger amount sold reported in table 3. As noted earlier, this increase is affected by a few large producers and our estimate of the treatment effect on amount sold goes down to 59 kg once we winsorize this outcome at the 95^{th} percentile.

We found no evidence that the treatment affected the quantity or propensity of producers to lose cashew nuts post-harvest, due to spoilage. We also found no evidence of local processing among producers in our sample, though we note that this outcome is extremely rare in our sample, as less than 10 producers reported any local processing of raw cashews. We cannot fully account for the extra quantity sold based on the responses by producers, who could plausible remember more easily the amounts sold relative to those they lost.

[43] We defined total quantity produced as the sum of the cashews sold, paid to workers for cleaning and harvesting, exchanged for rice (excluding those used in small regular exchanges of less than 25 kg per week, since we did not collect this measure at baseline), locally processed, cashews used to repay loans and raw cashew nuts used for other purposes, such as seeds for future plantations.

Table 10: Results - Total quantity produced and other uses

	Control (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover
Total quantity produced	1079.84	188.82**	47.35	141.47*
	(1000.47)	(93.03)	(90.29)	(81.50)
	1267	[0.22]	[0.75]	[0.42]
1 if lost cashew post-harvest	0.05	0.00	0.02	-0.02
	(0.22)	(0.01)	(0.01)	(0.01)
	1708	[0.91]	[0.75]	[0.55]
Quantity lost post-harvest	0.74	-0.03	0.23	-0.26
	(5.05)	(0.27)	(0.37)	(0.36)
	1692	[0.91]	[0.75]	[0.59]
1 if processed cashew nuts	0.00	0.00	0.00	-0.00
	(0.04)	(0.00)	(0.00)	(0.00)
	1699	[0.70]	[0.75]	[0.91]
Quantity of processed raw cashews	0.15	0.59	0.08	0.52
	(3.68)	(0.52)	(0.34)	(0.53)
	1699	[0.63]	[0.82]	[0.55]
Not pre-specified:				
Quantity traded in small weekly exchanges	145.40	-9.38	-9.47	0.09
	(289.09)	(16.48)	(17.19)	(17.03)
	1604	[0.79]	[0.99]	[1.00]
Total quantity of cashews stored	0.60	-0.35	-0.43	0.08
	(10.70)	(0.31)	(0.39)	(0.14)
	1679	[0.79]	[0.99]	[1.00]
Total quantity of cashews paid to labourers	162.72	8.85	-0.25	9.10
	(261.04)	(17.21)	(16.03)	(14.24)
	1421	[0.79]	[0.99]	[1.00]
1 if perceived that production...				
...increased in 2020 relative to 2019	0.33	-0.01	0.00	-0.01
	(0.47)	(0.03)	(0.03)	(0.03)
	1707	[0.79]	[0.99]	[1.00]
...decreased in 2020 relative to 2019	0.56	0.01	0.01	0.00
	(0.50)	(0.03)	(0.03)	(0.03)
	1707	[0.79]	[0.99]	[1.00]

Notes: producer-level intention-to-treat (ITT) estimates 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.

It is still possible that treated producers sold more cashew nuts in bulk transactions instead of using them as a medium of exchange for small regular purchases. It is not uncommon for producers to use small quantities (less than 25 kg) of cashews as a medium of exchange to buy goods in their village or in neighbouring ones. Treated producers reported using a smaller amount of cashews in those small exchanges, though the difference is not statistically significant. We could not estimate exactly the amounts sold in those regular small exchanges, as we only asked producers the amount they exchanged during a regular week during the trading season, and multiplied it by 18 (the total number of weeks in the trading season). Moreover, we found no evidence that treated producers stored more cashew to sell in the following trading season, something which is extremely rare in our sample as the control group mean is less than 1 kg.

A further possibility that could explain the increase in the total quantity sold is that producers may have exerted more effort to ensure that they did not leave uncollected nuts during the harvest. This hypothesis is possible because the harvest may continue until May in certain regions of the country. Moreover, treated producers had been trained on the use of information, and it is possible that this may have affected their supply of labour. The amount of cashews used to pay labourers is a loose proxy for labour supply, as many hired labourers are paid in proportion to the amount they collect. However, we did not find a large or significant increase in the amount used to pay labourers that could suggest an increase in the effort exerted by producers. We are unable to document an increase in productivity with our data. Treated producers did not perceive their production having increased in 2020 relative to the previous year.

6.5 Placebo outcomes

Finally, we investigate whether our results may be driven by behavioural outcomes that we would have not expected to change *a-priori*. We first check whether treated producers were more likely to report sales because of better records. At the end of the baseline survey, all producers received a transaction diary to record their sales during the 2020 trading season from the survey team.
In the first row of table 11, we found no evidence that treated producers were more likely to have used these transaction diary relative to spillover or control producers. In Appendix section G, we further describe the data contained in the transaction diaries. In general, even if this data comes from a selected sub-sample of producers, observing these records is uncorrelated with treatment assignment. The transactions recorded in the diaries broadly support the patterns described in section 5.

We found no evidence that treated producers were more likely to respond to an hypothetical lottery in a way that would be extremely risk averse.[44] We found a

[44] We measure extreme risk aversion asking producers to pick their preferred hypothetical lottery from a six alternatives with varying expected mean and variance, but constants standardised mean, in the style of Binswanger (1980).

marginally higher proportion of treated producers reporting that they trust most people in Guinea-Bissau relative to producers in the other two experimental groups. However, this difference is not statistical significant after accounting for multiple-hypothesis testing. We interpret this difference as a weak evidence of an unintended consequence of having taken part in the experiment, though we are not confident in this finding given the lack of statistically significance after correcting for multiple inference.

Table 11: Results - Placebo outcomes

	Control (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover
1 if filled the sale diary well	0.31	-0.03	-0.04	0.01
	(0.46)	(0.03)	(0.03)	(0.02)
	1719	[0.54]	[0.51]	[0.53]
1 if is extremely risk averse	0.35	-0.00	0.03	-0.03
	(0.48)	(0.03)	(0.03)	(0.03)
	1627	[0.89]	[0.51]	[0.51]
1 if trusts most people	0.38	0.06**	0.01	0.04
	(0.49)	(0.03)	(0.03)	(0.03)
	1712	[0.12]	[0.65]	[0.34]

Notes: producer-level intention-to-treat (ITT) estimates 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.

6.6 Heterogeneity

Potential heterogeneity in treatment effects can help us understand which type of producers most benefited from the intervention. In this section, we briefly comment on the heterogeneity of our effects relative to three baseline variables that we had set out in our pre-analysis plan: the road distance in km to the nearest regional capital, as a proxy for the nearest large market and storage hub; the respondent's principal component of assets (Filmer and Pritchett, 2001), as a proxy for socio-economic status; and the number of cashew producers that live in the village of the

respondent, as a proxy for the level of competition among producers.[45]
To test for heterogeneous effects along these dimensions, we interact our treatment indicator with baseline measures of these variables. We trichotomize the variable — interacting in each case 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. (2021). We focus on the main outcomes from tables 2 and 3 and report the tables in appendix I. We analyse outcomes winsorized at the 99^{th} percentile when estimating heterogeneous treatment effects in order to avoid drawing conclusions from sub-groups that could be driven by very few observations with large amounts.

We did not find a consistent pattern across the dimensions of heterogeneity we considered, but still obtain a more nuanced understanding of which sub-groups experienced larger effects.
Treated producers living farther from a regional capital report higher prices relative to untreated producers in comparably remote villages. This finding could be consistent with the spatial spillovers discussed in the previous subsection 6.1, as more isolated treatment villages are those that are usually farther away from a regional capital. Moreover, it could also reflect the fact that prices tend to be higher around villages close to the national borders, which are usually farthest from the regional capitals. In table 33, across terciles of the road distance to the nearest regional capital, we found that treated producers in the top tercile received higher prices relative to treated producers in the middle tercile of this variable (significant at the 10% level without accounting for multiple hypothesis testing). However, in terms of revenue from sales and exchanges, we found that our treatment effects were marginally more pronounced among producers within a shorter distance from a regional capital. In particular, treated producers in the bottom tercile of this distance variable reported earning a larger value of sales compared to those treated producers in the middle tercile (significant at the 10% level even accounting for multiple hypothesis testing). These results are consistent with those reported in table 34, which shows that treated producers living closer to a regional capital increased the amount of cashews sold relative to other treated producers or to untreated producers within a similar distance to a regional capital. Villages closer to a regional hub tend to have better road connections than those further away, which can help producers selling more frequently and sell larger amounts. Interestingly, producers that were both in the bottom and top tercile of this distance

[45] We had also tested for heterogeneity of treatment effects by other pre-specified variables for which we find weak or no heterogeneity: an indicator variable equal to one if respondent sells their output to a fixed buyer; the respondent's numeracy index; an indicator variable equal to one if the respondent is an hyperbolic discounter; an indicator variable equal to one if the respondent is extremely risk-averse; the number of intermediaries that live in the village of the respondent, the respondent's years of experience in the cashew market, the respondent's area of cashew plantation (in hectares); an indicator variable equal to one if the respondent could read a sentence in Bissau-Guinean Kriol, to understand whether literacy could be associated with our results; the road distance in km to the nearest sector capital, and the country capital, Bissau.

variable reported a higher frequency of sales.

Treated producers with fewer assets were more likely to obtain more value from barters, which are in general more common among worse-off producers. We did not find heterogeneous treatment effects on prices depending on the level of assets, in table 35. Across terciles of the principal component of assets, we found that treated producers in the top tercile received higher prices relative to treated producers in the bottom tercile of this variable (significant at 10% level without accounting for multiple hypothesis testing). However, in terms of revenue from sales and exchanges, we found that our treatment effects were marginally more pronounced among producers with a smaller level of assets. In particular, treated producers in the bottom tercile of our assets index (Filmer and Pritchett, 2001) reported earning a larger amount (of rice in monetary terms) from barter exchanges compared to those treated producers in the middle or top terciles. These results are consistent with those reported in table 36, which shows that treated producers with fewer assets are more likely to have increased the amount of cashews bartered relative to other treated producers or to untreated producers with low levels of assets.

We found that treatment effects were marginally larger among producers living in villages with fewer other producers. We did not find heterogeneous treatment effects on the price of sales relative to the number of producers, in table 37. Across terciles, however, we found that treated producers living in villages within the bottom tercile received somewhat higher prices relative to treated producers in the top tercile of this variable (significant at 10% level without accounting for multiple hypothesis testing). These findings may be consistent with the previously discussed heterogeneous patterns, as more remote villages also have fewer producers and inhabitants more generally. In terms of revenue from sales and exchanges, we found that our treatment effects were more pronounced among producers living in villages with fewer other producers, though these differences were not statistically significant across terciles. In table 38, we found a marginally higher amount sold among treated producers living in smaller villages relative to control producers in similar villages, but those effects were not different across terciles. Treated producers living in villages with many producers are more likely to have reduced the probability of bartering relative to other treated producers or to untreated producers living in villages with fewer other producers. Instead, these producers are more likely to have made a higher number of sales.

To summarise, we found no consistent pattern in our analysis of heterogeneous treatment effects, though these are plausibly consistent with the spillovers documented in the previous subsection 6.1. Treated producers that were in more remote villages reported earning higher prices. We found that treated producers that were more likely to sell a larger amount of cashews were those living in villages with fewer other producers and those villages located closer to a regional capital. Treated producers with fewer assets were more likely to barter cashews for rice.

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 weekly text and voice-messages during the 2020 trading season to treated producers. The information sent to producers contained up-to-date farm-gate prices, market news, and marketing advice on when to sell. Treated producers changed the frequency and timing of their sales. We found that treated producers sold more frequently cashews in monetary transactions, selling their stock across multiple transactions rather than a single one. By doing so, treated producers earned a larger total revenue relative to untreated ones. We were able to rule out several potential mechanisms that could explain our results. We did not find evidence showing that producers changed where or to whom they sold their cashew nuts. In our context and at the time of our intervention, which coincided with 2020 COVID-19 pandemic, it may have been challenging for better informed producers to sell their stocks elsewhere. We did not find evidence to suggest that treated producers had more bargaining power or had a different information set about the following trading season. We were also able to rule out that our effects may be due better record-keeping induced by our intervention. However, we find that *between*-village spillovers of the intervention could explain why we did not find an increase in average prices among treated producers relative to control producers.

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. For example, we need to better understand how the service affects producers once it is available to any user in the country, at a cost, and not just our treated sample of producers. One of the limitations of our study is that it offered the service for free to treated producers. Thus, we were unable to measure take-up of the service if users had to pay for it. Our measure of take-up was only based on the probability of randomly selected users to be reached and listen to the messages sent by the service, or noisier self-reports on whether respondents reported having used the service. As the service is 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. Section D illustrates effects of the intervention on additional outcomes not reported in the main text. In section E, we provide a brief description of the production costs involved in cashew production. In sections G and H 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. In section I we report heterogeneous treatment effects of the intervention across several pre-specified baseline dimensions. Finally, section J uses the control group at baseline and follow-up to show how the main outcomes changed over time, before and after the start of the COVID-19 pandemic.

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.[46] 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.

[46] 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:

$$||z_l - z_j|| = ((z_l - z_j)' S^{-1} (z_l - z_j))^{1/2} \tag{6}$$

where z_l and z_j denote the vector or 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.

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 12: 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	-3.66	1.35	-5.01	0.05
	(96.24)	(5.51)	(5.60)	(4.75)	1968
	1968	[0.73]	[0.81]	[0.56]	
Value of all sales and exchanges	525563.79	35895.89	24851.78	11044.11	0.03
	(921998.28)	(44423.03)	(43333.93)	(29750.44)	1978
	1978	[0.68]	[0.77]	[0.84]	
Value of all sales	486696.38	14815.85	15806.13	-990.28	0.01
	(912168.16)	(43992.64)	(43245.56)	(28614.91)	1984
	1984	[0.80]	[0.77]	[0.97]	
Value of exchanges	38130.25	22023.34***	10701.90*	11321.43**	0.22
	(83549.96)	(6769.25)	(5951.64)	(5656.60)	1982
	1982	[0.02]**	[0.40]	[0.29]	
Total quantity sold	1206.35	54.39	36.52	17.87	0.01
	(1855.65)	(96.73)	(94.05)	(66.13)	1984
	1984	[0.75]	[0.77]	[0.85]	
Number of sales	1.62	0.01	0.07	-0.05	0.08
	(0.81)	(0.04)	(0.05)	(0.04)	1988
	1988	[0.80]	[0.40]	[0.56]	
1 if exchanged cashew for rice	0.30	0.04	0.02	0.02	0.15
	(0.46)	(0.03)	(0.03)	(0.02)	1985
	1985	[0.34]	[0.77]	[0.56]	
Total quantity exchanged	113.66	57.26***	25.75	31.51**	0.21
	(247.43)	(19.08)	(17.06)	(15.94)	1982
	1982	[0.02]**	[0.40]	[0.29]	
Share of quantity sold over quantity exchanged and sold	0.90	-0.02	-0.01	-0.02	0.13
	(0.20)	(0.01)	(0.01)	(0.01)	1979
	1979	[0.34]	[0.77]	[0.47]	
1 if borrowed money or rice	0.35	-0.00	0.04	-0.05*	0.10
	(0.48)	(0.03)	(0.03)	(0.03)	1987
	1987	[0.92]	[0.40]	[0.29]	
Implicit price of loans per kg of cashews	356.61	-11.98	-19.76	7.77	0.18
	(101.18)	(11.23)	(12.33)	(12.43)	516
	516	[0.62]	[0.40]	[0.76]	
Total quantity paid for loans	45.23	17.69*	8.85	8.85	0.10
	(126.18)	(9.09)	(7.27)	(9.30)	1987
	1987	[0.23]	[0.42]	[0.56]	
Share of quantity sold over quantity exchanged and paid in loans	0.94	-0.01	-0.01	0.00	0.03
	(0.15)	(0.01)	(0.01)	(0.01)	1983
	1983	[0.68]	[0.40]	[0.76]	

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 13: Baseline balance — Secondary outcome Variables

	Control (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover	(5) Max pairwise st. diff.
1 if delayed sales because expected price to raise	0.72	0.00	-0.01	0.02	0.04
	(0.45)	(0.02)	(0.02)	(0.03)	1984
	1984	[0.90]	[0.87]	[0.83]	
1 if sped up sales because expected price to drop	0.30	-0.01	-0.01	-0.00	0.00
	(0.46)	(0.03)	(0.03)	(0.03)	1978
	1978	[0.90]	[0.93]	[0.99]	
1 if sold most to local buyer	0.37	0.00	0.02	-0.02	0.06
	(0.48)	(0.03)	(0.03)	(0.03)	1984
	1984	[0.95]	[0.87]	[0.94]	
1 if sold most to itinerant buyer	0.55	-0.00	-0.02	0.02	0.04
	(0.50)	(0.03)	(0.03)	(0.03)	1984
	1984	[0.99]	[0.87]	[0.83]	
1 if sold most directly to a market	0.06	0.00	0.01	-0.00	0.03
	(0.24)	(0.01)	(0.01)	(0.01)	1984
	1984	[0.90]	[0.87]	[0.97]	
1 if sold to or close to Senegal	0.00	-0.00	-0.00	0.00	0.05
	(0.07)	(0.00)	(0.00)	(0.00)	1988
	1988	[0.90]	[0.87]	[0.97]	
1 if sold cashews in April 2019 or before	0.20	-0.02	-0.00	-0.02	0.06
	(0.40)	(0.02)	(0.02)	(0.02)	1854
	1854	[0.88]	[0.93]	[0.78]	
1 if sold cashews in May 2019	0.50	0.01	0.07**	-0.06**	0.14
	(0.50)	(0.03)	(0.03)	(0.03)	1854
	1854	[0.90]	[0.28]	[0.42]	
1 if sold cashews in June 2019	0.55	0.04*	0.01	0.04	0.11
	(0.50)	(0.02)	(0.03)	(0.03)	1854
	1854	[0.83]	[0.93]	[0.69]	
1 if sold cashews in July 2019 or later	0.19	-0.01	-0.00	-0.00	0.06
	(0.39)	(0.02)	(0.02)	(0.02)	1854
	1854	[0.90]	[0.93]	[0.98]	
Total quantity produced	1477.87	147.53	58.96	88.57	0.07
	(1997.03)	(108.25)	(102.99)	(77.61)	1912
	1912	[0.88]	[0.87]	[0.70]	
1 if lost cashew post-harvest	0.06	-0.01	0.02	-0.03**	0.13
	(0.24)	(0.01)	(0.01)	(0.02)	1988
	1988	[0.90]	[0.87]	[0.42]	
Quantity lost post-harvest	2.46	-0.85	1.10	-1.94*	0.13
	(15.04)	(0.75)	(1.13)	(1.07)	1986
	1986	[0.88]	[0.87]	[0.51]	
1 if processed cashew nuts	0.01	-0.00	-0.01	0.00	0.11
	(0.11)	(0.01)	(0.00)	(0.00)	1985
	1985	[0.90]	[0.87]	[0.83]	
Quantity of processed raw cashews	0.63	-0.11	-0.07	-0.04	0.03
	(5.81)	(0.27)	(0.37)	(0.44)	1985
	1985	[0.90]	[0.93]	[0.98]	
Number of succesful bargains	0.23	0.03	0.04	-0.01	0.11
	(0.50)	(0.03)	(0.03)	(0.03)	1987
	1987	[0.88]	[0.87]	[0.98]	
1 if acted as intermediary	0.03	-0.00	0.00	-0.00	0.02
	(0.18)	(0.01)	(0.01)	(0.01)	1987
	1987	[0.90]	[0.93]	[0.97]	
Number of potential buyers that made offers	2.68	0.19*	0.05	0.13	0.16
	(1.71)	(0.10)	(0.10)	(0.10)	1981
	1981	[0.83]	[0.87]	[0.69]	
1 if thinks that the reference price is important in marketing	0.36	0.03	0.03	0.01	0.07
	(0.48)	(0.02)	(0.03)	(0.03)	1987
	1987	[0.88]	[0.87]	[0.97]	
Expected sale price	819.62	-11.02	13.38	-24.40	0.08
	(312.12)	(13.76)	(15.83)	(18.15)	1986
	1986	[0.90]	[0.87]	[0.69]	
1 if is extremely risk averse	0.55	-0.02	0.01	-0.03	0.07
	(0.50)	(0.02)	(0.03)	(0.03)	1959
	1959	[0.90]	[0.87]	[0.69]	
1 if trusts most people	0.28	-0.01	0.01	-0.02	0.03
	(0.45)	(0.02)	(0.02)	(0.02)	1988
	1988	[0.90]	[0.93]	[0.83]	

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 14: 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	0.00	-0.00	0.00	0.02
	(0.23)	(0.01)	(0.01)	(0.01)	1985
	1985	[0.95]	[1.00]	[0.99]	
Age	43.47	-1.06	-1.11	0.05	0.06
	(14.96)	(0.79)	(0.77)	(0.75)	1988
	1988	[0.37]	[0.46]	[0.99]	
Household size	13.40	0.57	1.02*	-0.45	0.08
	(7.88)	(0.54)	(0.57)	(0.41)	1988
	1988	[0.44]	[0.34]	[0.85]	
Years of education	3.89	0.01	-0.10	0.11	0.04
	(3.77)	(0.23)	(0.23)	(0.18)	1988
	1988	[0.95]	[1.00]	[0.97]	
1 if Kriol is the most spoken language at home	0.21	-0.01	-0.01	0.00	0.05
	(0.41)	(0.02)	(0.02)	(0.02)	1988
	1988	[0.89]	[1.00]	[0.99]	
1 if cashew is the main source of income	0.79	0.02	0.00	0.02	0.06
	(0.41)	(0.02)	(0.02)	(0.02)	1988
	1988	[0.44]	[1.00]	[0.85]	
1 if faces storage limitations	0.27	0.06**	0.02	0.04*	0.15
	(0.45)	(0.03)	(0.02)	(0.02)	1988
	1988	[0.05]*	[0.89]	[0.85]	
Minimum age of trees	3.32	-0.06	0.08	-0.14	0.06
	(3.43)	(0.17)	(0.20)	(0.20)	1966
	1966	[0.89]	[1.00]	[0.97]	
Max age of trees	17.23	-0.20	0.04	-0.25	0.04
	(8.41)	(0.47)	(0.48)	(0.48)	1868
	1868	[0.89]	[1.00]	[0.99]	
1 if trees were plagued	0.69	-0.03	-0.01	-0.03	0.08
	(0.46)	(0.03)	(0.03)	(0.03)	1980
	1980	[0.37]	[1.00]	[0.85]	
1 if sells other crops	0.65	0.09***	0.10***	-0.01	0.21
	(0.48)	(0.02)	(0.02)	(0.02)	1988
	1988	[0.00]***	[0.00]***	[0.99]	
Index of trust (Anderson, 2008)	-0.00	0.07	0.07	-0.00	0.05
	(1.00)	(0.05)	(0.05)	(0.06)	1988
	1988	[0.37]	[0.55]	[0.99]	
Index of food security (Anderson, 2008)	0.00	-0.14***	-0.08	-0.05	0.09
	(1.00)	(0.05)	(0.05)	(0.05)	1983
	1983	[0.02]**	[0.43]	[0.85]	
Area plantation	3.26	0.28**	0.27*	0.01	0.11
	(2.29)	(0.13)	(0.14)	(0.15)	1988
	1988	[0.14]	[0.34]	[0.99]	
Years of experience in cashew sector	10.17	0.65*	0.25	0.41	0.08
	(7.05)	(0.39)	(0.38)	(0.38)	1988
	1988	[0.24]	[1.00]	[0.85]	
Standardised principal component of wealth (Filmer and Pritchett, 2001)	-0.19	0.30***	0.37***	-0.07	0.20
	(1.63)	(0.10)	(0.10)	(0.09)	1979
	1979	[0.02]**	[0.00]***	[0.97]	
Index of numeracy (Anderson, 2008)	0.00	0.10*	0.02	0.08	0.11
	(1.00)	(0.06)	(0.06)	(0.05)	1988
	1988	[0.24]	[1.00]	[0.85]	
1 if can read	0.29	0.00	0.00	-0.00	0.02
	(0.45)	(0.03)	(0.03)	(0.02)	1988
	1988	[0.95]	[1.00]	[0.99]	

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 15: Balance — Secondary outcomes pre-specified as producer characteristics

	Control (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover	(5) Max pairwise st. diff.
Desired reference price	1042.00	-16.38	14.87	-31.25	0.09
	(297.53)	(16.78)	(22.31)	(23.25)	1988
	1988	[0.38]	[0.68]	[0.42]	
Minimum price at which sold most cashew in the last 3 years	318.64	-5.68	-1.93	-3.75	0.06
	(88.85)	(4.22)	(4.70)	(4.40)	1988
	1988	[0.25]	[0.68]	[0.67]	
1 if delayed sales because expected price to raise	0.72	0.00	-0.01	0.02	0.04
	(0.45)	(0.02)	(0.02)	(0.03)	1984
	1984	[0.82]	[0.68]	[0.67]	
Number of potential buyers that made offers	2.68	0.19*	0.05	0.13	0.16
	(1.71)	(0.10)	(0.10)	(0.10)	1981
	1981	[0.13]	[0.68]	[0.42]	
1 if thinks that the reference price is important in marketing	0.36	0.03	0.03	0.01	0.07
	(0.48)	(0.02)	(0.03)	(0.03)	1987
	1987	[0.25]	[0.68]	[0.79]	
Number of producers that shared price offers	4.91	0.60**	0.74**	-0.13	0.11
	(5.79)	(0.29)	(0.34)	(0.39)	1863
	1863	[0.13]	[0.11]	[0.79]	
Number of producers informed of an offer received	5.95	0.81**	1.72***	-0.91*	0.20
	(6.86)	(0.35)	(0.48)	(0.53)	1878
	1878	[0.13]	[0.00]***	[0.42]	

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 16: Balance — Village characteristics

	Control (1) Mean (SD) Total obs.	(2) Treatment
Number of cashew producers in the village	33.87	-0.67
	(23.49)	(2.83)
	288	[0.81]
Number of cashew buyers in the village	2.90	1.14***
	(2.58)	(0.41)
	287	[0.05]*
Road distance in km to nearest sector capital	25.59	1.02
	(23.70)	(2.65)
	290	[0.81]
Road distance in km to nearest region capital	51.48	-0.85
	(33.51)	(2.96)
	290	[0.81]
Road distance in km to the capital	162.30	4.15
	(69.26)	(4.60)
	290	[0.59]
Distance in km to the nearest primary road	20.16	2.35
	(17.17)	(1.60)
	290	[0.38]
Distance in km to the nearest secondary road	3.19	0.53
	(2.85)	(0.32)
	290	[0.38]
Distance in km to the nearest road	2.74	0.30
	(2.40)	(0.28)
	290	[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 17: Attrition in May 2021 - In-Person Endline Survey

	Control (1) Mean (SD)	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover	(5) Max pairwise st. diff. Total obs.
1 if attrited at endline	0.13	0.01	0.00	0.00	0.05
	(0.33)	(0.02)	(0.02)	(0.02)	1988
		[0.66]	[0.80]	[0.90]	

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 randomization triplet fixed-effects. Standard errors are in parentheses and are clustered at the village-level. Asterisks on the coefficient estimates reflect unadjusted p-values. p-values reported in brackets. Significance levels: *10%, **5%, and ***1%. Column 1 displays the control mean, standard deviation, and total number of observations.

B.3 Compliance

Table 18: Compliance - Take-up of the intervention using MTN Administrative Data

	Control (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover
1 if received at least 8 messages in 2020 (Take-up)	0.00	0.53***	0.00	0.53***
	(0.00)	(0.02)	(0.01)	(0.03)
	1719	[0.00]***	[0.90]	[0.00]***

Notes: Coefficient of treatment and spillover at endline (Columns 2-3). Column 4 tests for differences in parameters obtained in previous two columns. Outcome variable listed on the left. The unit of observation is the individual producer. All models control for randomization triplet fixed-effects. Standard errors are in parentheses and are clustered at the village-level. Asterisks on the coefficient estimates reflect unadjusted p-values. Minimum p-values reported in brackets. Significance levels: *10%, **5%, and ***1%. Column 1 displays the control mean, standard deviation, and total number of observations.

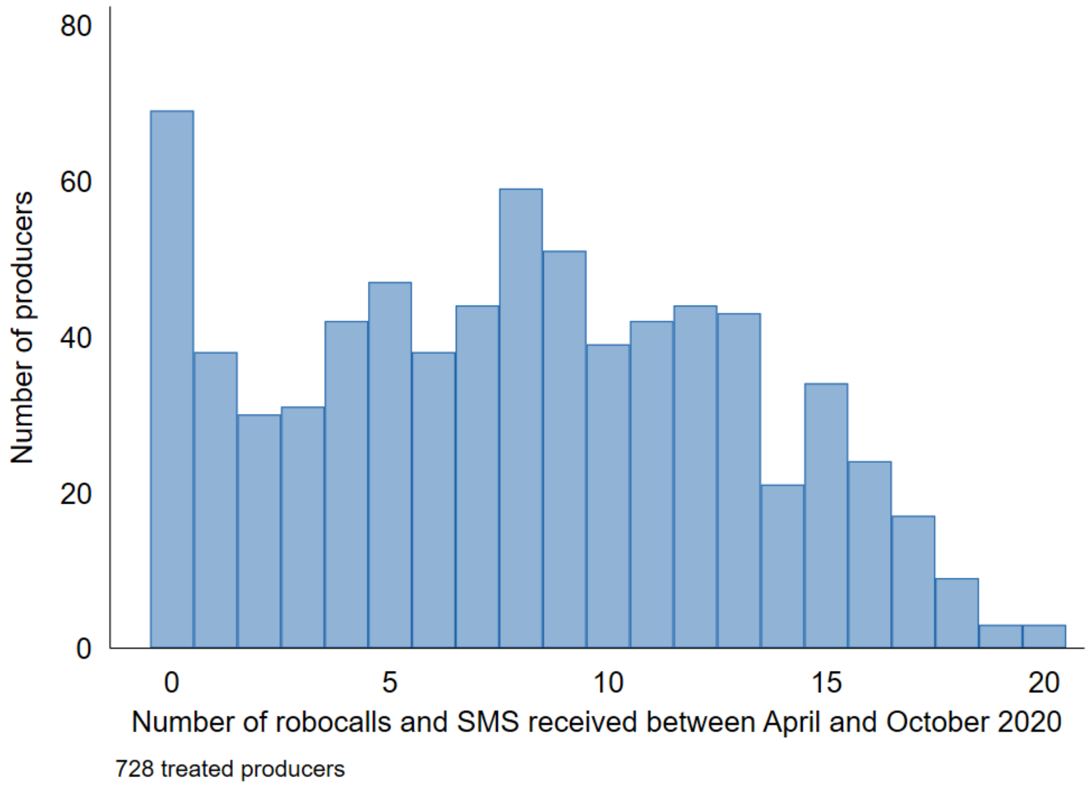

Figure 5: Number of robocalls and SMS received by treated producer using MTN's administrative data

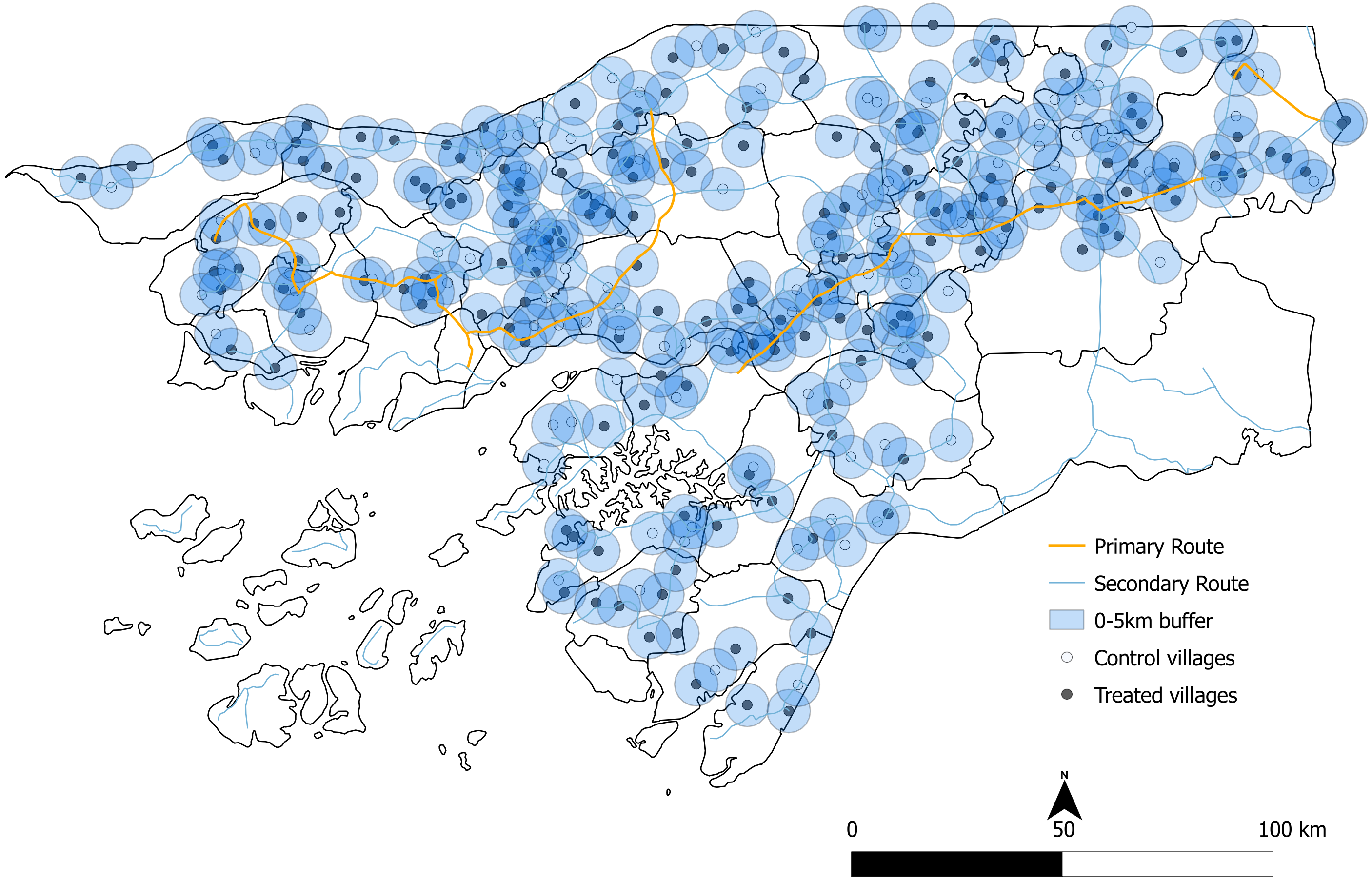

Figure 6: Geographic distribution of treatment with 0-5km buffer around villages

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.

Table 19: Robustness — Prices and revenue winsorized at 99^{th} percentile

	Control (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover
Price per sale per kg	295.71	2.07	2.33	-0.26
	(104.36)	(3.46)	(3.35)	(3.30)
	1587	[0.55]	[0.65]	[0.94]
Value of all sales and exchanges	284193.51	30314.08	17476.25	12837.83
	(289006.62)	(18522.67)	(21126.86)	(18488.60)
	1521	[0.26]	[0.65]	[0.75]
Value of all sales	230293.14	25365.33	2140.42	23224.92
	(269143.16)	(16662.85)	(18341.82)	(15886.19)
	1591	[0.26]	[0.91]	[0.58]
Value of exchanges	52020.20	9056.52	12391.42	-3334.89
	(86004.73)	(7604.14)	(8227.55)	(5758.68)
	1636	[0.31]	[0.53]	[0.75]

Notes: producer-level intention-to-treat (ITT) estimates 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 20: Robustness — Cashew sales, exchanges, and loans winsorized at 99^{th} percentile

	Control (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover
Total quantity sold	695.97	82.83*	22.28	60.55
	(764.74)	(48.32)	(52.72)	(45.51)
	1622	[0.22]	[0.67]	[0.46]
Number of sales	1.48	0.21***	0.08	0.13**
	(0.94)	(0.06)	(0.06)	(0.06)
	1693	[0.00]***	[0.64]	[0.10]*
1 if exchanged cashew for rice	0.56	-0.01	-0.03	0.03
	(0.50)	(0.03)	(0.03)	(0.03)
	1706	[0.85]	[0.64]	[0.59]
Total quantity exchanged	217.70	13.48	16.16	-2.68
	(323.48)	(23.44)	(24.58)	(21.08)
	1681	[0.71]	[0.64]	[0.96]
Share of quantity sold over quantity exchanged and sold	0.73	0.02	0.02	0.00
	(0.32)	(0.02)	(0.02)	(0.02)
	1604	[0.70]	[0.64]	[0.96]
Loans:				
1 if borrowed money or rice	0.31	0.02	0.03	-0.01
	(0.46)	(0.03)	(0.03)	(0.03)
	1702	[0.65]	[0.57]	[0.75]
Implicit price of loans per kg of cashews	58.96	12.62**	3.30	9.33
	(142.79)	(5.49)	(6.13)	(5.79)
	1645	[0.09]*	[0.59]	[0.41]
Total quantity paid for loans	44.09	-3.23	7.00	-10.24
	(106.88)	(7.08)	(8.99)	(8.11)
	1686	[0.65]	[0.58]	[0.41]
Share of quantity sold over quantity exchanged and paid in loans	0.86	0.01	0.03	-0.02
	(0.29)	(0.02)	(0.02)	(0.02)
	1599	[0.65]	[0.49]	[0.44]

Notes: producer-level intention-to-treat (ITT) estimates 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 21: Robustness — Prices and revenue winsorized at 95th percentile

	Control (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover
Price per sale per kg	295.71	2.07	2.33	-0.26
	(104.36)	(3.46)	(3.35)	(3.30)
	1587	[0.55]	[0.73]	[0.94]
Value of all sales and exchanges	268381.83	22903.46	9411.73	13491.73
	(227328.24)	(13888.83)	(15516.25)	(12917.55)
	1521	[0.20]	[0.73]	[0.59]
Value of all sales	212156.79	19263.07*	870.66	18392.41*
	(198563.75)	(11401.24)	(12720.95)	(10984.52)
	1591	[0.20]	[0.94]	[0.38]
Value of exchanges	47953.99	5281.72	6158.32	-876.60
	(69132.58)	(5559.90)	(6019.41)	(4434.11)
	1636	[0.46]	[0.73]	[0.94]

Notes: producer-level intention-to-treat (ITT) estimates 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 22: Robustness — Cashew sales, exchanges, and loans winsorized at 95^{th} percentile

	Control (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover
Total quantity sold	644.94	59.06*	5.31	53.75*
	(572.80)	(33.74)	(37.73)	(32.42)
	1622	[0.20]	[0.89]	[0.25]
Number of sales	1.43	0.19***	0.08	0.11**
	(0.81)	(0.05)	(0.05)	(0.05)
	1693	[0.00]***	[0.61]	[0.10]*
1 if exchanged cashew for rice	0.56	-0.01	-0.03	0.03
	(0.50)	(0.03)	(0.03)	(0.03)
	1706	[0.85]	[0.61]	[0.59]
Total quantity exchanged	204.12	4.86	14.63	-9.77
	(270.63)	(20.20)	(21.21)	(16.49)
	1681	[0.85]	[0.61]	[0.69]
Share of quantity sold over quantity exchanged and sold	0.73	0.02	0.02	0.00
	(0.32)	(0.02)	(0.02)	(0.02)
	1604	[0.70]	[0.61]	[0.96]
Loans:				
1 if borrowed money or rice	0.31	0.02	0.03	-0.01
	(0.46)	(0.03)	(0.03)	(0.03)
	1702	[0.65]	[0.57]	[0.75]
Implicit price of loans per kg of cashews	58.96	12.62**	3.30	9.33
	(142.79)	(5.49)	(6.13)	(5.79)
	1645	[0.09]*	[0.59]	[0.41]
Total quantity paid for loans	44.09	-3.23	7.00	-10.24
	(106.88)	(7.08)	(8.99)	(8.11)
	1686	[0.65]	[0.58]	[0.41]
Share of quantity sold over quantity exchanged and paid in loans	0.86	0.01	0.03	-0.02
	(0.29)	(0.02)	(0.02)	(0.02)
	1599	[0.65]	[0.49]	[0.44]

Notes: producer-level intention-to-treat (ITT) estimates 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.

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 14 and 16. We include the following covariates:

- Household size
- 1 if faces difficulties storing
- 1 if sells other agricultural products
- Food security index (Anderson, 2008)
- Area of plantation
- Years of experience in the sector
- Standardized principal component of assets (Filmer and Pritchett, 2001)
- Numeracy index (Anderson, 2008)
- 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.

Table 23: Robustness — Prices and revenue controlling for unbalanced characteristics

	Control (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover
Price per sale per kg	295.71	-0.25	0.30	-0.55
	(104.36)	(3.39)	(3.40)	(3.26)
	1587	[0.94]	[0.93]	[0.87]
Value of all sales and exchanges	287785.22	49809.83**	29780.55	20029.28
	(315352.77)	(25065.82)	(25399.31)	(22224.67)
	1521	[0.18]	[0.48]	[0.69]
Value of all sales	233494.84	39560.21*	13572.24	25987.96
	(292721.24)	(23279.46)	(23054.11)	(19835.65)
	1591	[0.18]	[0.74]	[0.69]
Value of exchanges	52325.81	9411.33	14030.58	-4619.25
	(88037.41)	(7609.53)	(9174.84)	(7170.67)
	1636	[0.29]	[0.48]	[0.69]

Notes: producer-level intention-to-treat (ITT) estimates 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 24: Robustness — Cashew sales, exchanges, and loans controlling for unbalanced characteristics

	Control (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover
Total quantity sold	708.21	116.96*	38.40	78.55
	(855.14)	(62.13)	(60.07)	(54.65)
	1622	[0.15]	[0.65]	[0.38]
Number of sales	1.49	0.19***	0.04	0.14**
	(1.01)	(0.06)	(0.06)	(0.06)
	1693	[0.01]***	[0.65]	[0.09]*
1 if exchanged cashew for rice	0.56	0.00	-0.03	0.03
	(0.50)	(0.03)	(0.03)	(0.03)
	1706	[0.99]	[0.65]	[0.46]
Total quantity exchanged	217.70	12.87	8.70	4.18
	(323.48)	(23.17)	(24.34)	(23.10)
	1681	[0.73]	[0.72]	[0.88]
Share of quantity sold over quantity exchanged and sold	0.73	0.01	0.01	-0.00
	(0.32)	(0.02)	(0.02)	(0.02)
	1604	[0.73]	[0.65]	[0.88]
Loans:				
1 if borrowed money or rice	0.31	0.03	0.04	-0.01
	(0.46)	(0.03)	(0.03)	(0.03)
	1702	[0.54]	[0.43]	[0.83]
Implicit price of loans per kg of cashews	58.96	11.14**	1.95	9.19
	(142.79)	(5.45)	(5.96)	(6.06)
	1645	[0.17]	[0.74]	[0.38]
Total quantity paid for loans	44.09	-4.32	4.34	-8.66
	(106.88)	(7.08)	(8.65)	(8.14)
	1686	[0.72]	[0.74]	[0.38]
Share of quantity sold over quantity exchanged and paid in loans	0.86	0.00	0.02	-0.02
	(0.29)	(0.02)	(0.02)	(0.02)
	1599	[0.90]	[0.43]	[0.38]

Notes: producer-level intention-to-treat (ITT) estimates 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.

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.

Table 25: Robustness — Prices and revenue estimated using PDSLASSO

	Control (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover
Price per sale per kg	295.71	2.07	2.33	-0.26
	(104.36)	(3.36)	(3.26)	(3.21)
	1587	[0.54]	[0.62]	[0.93]
Value of all sales and exchanges	287785.22	49901.72**	30277.59	19624.13
	(315352.77)	(23926.89)	(24644.43)	(21443.29)
	1527	[0.14]	[0.44]	[0.48]
Value of all sales	233494.84	39165.14*	10791.39	28373.75
	(292721.24)	(21653.65)	(21815.02)	(19148.46)
	1594	[0.14]	[0.62]	[0.48]
Value of exchanges	52325.81	10748.98	17267.64*	-6518.67
	(88037.41)	(7882.14)	(9471.93)	(7097.88)
	1639	[0.23]	[0.27]	[0.48]

Notes: producer-level intention-to-treat (ITT) estimates reported in columns 2 and 3, estimated using post-double least absloute shrinkage least absolute shrinkage and selection operator (Belloni et al., 2014). 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 partial out 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 26: Robustness — Cashew sales, exchanges, and loans estimated using PDSLASSO

	Control (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover
Total quantity sold	708.21	112.08*	28.20	83.88
	(855.14)	(58.72)	(58.31)	(52.46)
	1625	[0.14]	[0.63]	[0.27]
Number of sales	1.49	0.21***	0.07	0.14**
	(1.01)	(0.06)	(0.06)	(0.06)
	1693	[0.00]***	[0.62]	[0.09]*
1 if exchanged cashew for rice	0.56	-0.01	-0.04	0.02
	(0.50)	(0.03)	(0.03)	(0.03)
	1707	[0.68]	[0.62]	[0.61]
Total quantity exchanged	217.70	20.22	14.13	6.08
	(323.48)	(22.92)	(24.44)	(22.13)
	1685	[0.63]	[0.63]	[0.86]
Share of quantity sold over quantity exchanged and sold	0.73	0.01	0.01	-0.00
	(0.32)	(0.02)	(0.02)	(0.02)
	1610	[0.68]	[0.63]	[0.86]
Loans:				
1 if borrowed money or rice	0.31	0.02	0.03	-0.01
	(0.46)	(0.03)	(0.03)	(0.03)
	1703	[0.64]	[0.50]	[0.71]
Implicit price of loans per kg of cashews	58.96	12.62**	3.30	9.33*
	(142.79)	(5.34)	(5.95)	(5.63)
	1645	[0.07]*	[0.58]	[0.36]
Total quantity paid for loans	44.09	-3.23	7.29	-10.51
	(106.88)	(6.89)	(8.74)	(7.88)
	1687	[0.64]	[0.54]	[0.36]
Share of quantity sold over quantity exchanged and paid in loans	0.86	0.01	0.03	-0.02
	(0.29)	(0.02)	(0.02)	(0.02)
	1603	[0.64]	[0.45]	[0.42]

Notes: producer-level intention-to-treat (ITT) estimates reported in columns 2 and 3, estimated using post-double least absloute shrinkage least absolute shrinkage and selection operator (Belloni et al., 2014). 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 partial out 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.

D Additional outcomes

Table 27: Results - Reasons for choosing how many times to sell

	Control (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover
1 if they sold *more* than once because:				
They thought they would get better prices	0.23	0.07***	0.02	0.04
	(0.42)	(0.02)	(0.02)	(0.03)
	1693	[0.01]**	[0.71]	[0.16]
It was adviced by n'kalô	0.01	0.03***	-0.00	0.03***
	(0.09)	(0.01)	(0.01)	(0.01)
	1693	[0.00]***	[0.93]	[0.02]**
To smoothen consumption	0.22	0.05**	-0.01	0.06**
	(0.41)	(0.02)	(0.03)	(0.03)
	1693	[0.07]*	[0.93]	[0.07]*
1 if they sold *only* once because:				
They thought they would get better prices	0.23	-0.03	0.03	-0.06*
	(0.42)	(0.02)	(0.03)	(0.03)
	1693	[0.22]	[0.71]	[0.11]
It was adviced by n'kalô	0.01	0.01	0.00	0.01
	(0.12)	(0.01)	(0.01)	(0.01)
	1693	[0.17]	[0.93]	[0.19]
To pay for lumpy or urgent expenses	0.27	-0.07**	-0.03	-0.04
	(0.45)	(0.03)	(0.03)	(0.03)
	1693	[0.03]**	[0.71]	[0.16]

Notes: producer-level intention-to-treat (ITT) estimates 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 28: Results — Additional information-sharing outcomes

Number of producers...	Control (1) Mean (SD) Total obs.	(2) Treatment	(3) Spillover	(4) Treat. vs. spillover
...informed of an offer received frome the same village	5.07	0.23	0.19	0.04
	(5.91)	(0.33)	(0.33)	(0.30)
	1563	[0.48]	[0.56]	[0.90]
...informed of an offer received from other villages	1.67	0.29	0.19	0.11
	(2.94)	(0.18)	(0.19)	(0.22)
	1540	[0.21]	[0.44]	[0.82]
...that shared price offers from the same village	3.63	0.52**	0.28	0.24
	(7.45)	(0.26)	(0.26)	(0.24)
	1546	[0.18]	[0.44]	[0.82]
...that shared price offers from other village	1.20	0.21	0.31*	-0.10
	(2.40)	(0.16)	(0.18)	(0.19)
	1533	[0.26]	[0.33]	[0.82]

Notes: producer-level intention-to-treat (ITT) estimates 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.

E Costs of production

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.

F Weekly messages

Table 29: Results: Messages sent during the 2020 cashew marketing season

Date sent	Format	English translation
01/04/2020	SMS 1	BELAB and the N'kalo 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 campaign 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 campaign 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/N'kalo 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 375FCFA/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 375FCFA/KG or more to sell.
16/05/2020	Robocall 1	"Dear stakeholders of the cashew sector, this is André NANQUE, from n´kalô, 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 chestnut marketing campaign is still delayed due to the Coronavirus epidemic. But the campaign 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, chestnut processors in India and Vietnam are short of chestnuts and need it in the coming months. Prices in other countries have risen above 350 FCFA / kg. We believe that as soon as the campaign 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. See you next week"
21/05/2020	Robocall 2	"Dear stakeholders of the cashew sector, this is André NANQUE, from n´kalô, 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. Unfortunately, the official launch of the castanha trade campaign 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 FCFA / Kg, in Gabu the prices fixed at 250 FCFA / Kg and remain the same as last week, in Bafata, Bolama, Quinará and Tombali, where the price is also at 250 FCFA / kg. In neighboring countries, Senegal, Gambia and Guinea Conakry, prices have exceeded 350 FCFA / kg. We recommend that you wait and start selling your product only if the price offered to you reaches 375 FCFA / kg or more.
29/05/2020	Robocall 3	"Dear stakeholders of the cashew sector, this is André NANQUE, from n´kalô, to talk about the commercialization of cashew nuts this year. This week, the cashew trade campaign was finally officially launched. Since the campaign starts 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 chestnuts on ships to India and Vietnam. The demand for Guinea-Bissau chestnuts 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 Tombalí, prices have remained at 250 FCFA / Kg and in Oio, prices still vary between 250 - 300 FCFA / kg. On the other hand, in Cacheu, prices started to rise and are between 300-350 FCFA / kg, while in Biombo, the price increase is even stronger and sales are made between 350-375 FCFA / 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 FCFA / 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 FCFA / kg. So hope and have a good week!
11/06/2020	SMS 7	Prices are still rising: we advise to sell half of your stock if prices reach 375FCFA/KG or more, and sell the rest later.
17/06/2020	Robocall 4	"Dear stakeholders of the cashew sector, this is André NANQUE, from n´kalô, to talk about the commercialization of cashew nuts this year. The marketing 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 FCFA / kg in the regions of Bafata, Bolama, Gabu, Oio, Quinara and Tombali. Higher prices are practiced in Biombo regions, where sales of 375 FCFA / kg are made and in Cacheu, where prices reach up to 400 FCFA / kg in locations close to Senegal. At the port of Bissau, cashew nuts trucks are paid between 380 and 420 FCFA / 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 chestnuts until the price of 375 FCFA / kg is offered, and to sell half of your stocks when that price is offered. Good week to everyone

25/06/2020	Robocall 5	“Dear stakeholders of the cashew sector, this is André NANQUE, from n´kalô, to talk about the commercialization of cashew nuts this year. Large quantities have arrived in Bissau since the campaign was launched. This slightly reduced demand at the port and caused a slight drop in prices for chestnut trucks delivered to 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 FCFA / kg in the regions of Bafata and Gabu and between 300 and 350 FCFA / kg in all other regions of the country. With the coronavirus epidemic still a problem in many countries around the world, chestnut 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 nuts if the prices offered are higher than 325 FCFA / kg.
01/07/2020	Robocall 6	“Dear stakeholders of the cashew sector, this is André NANQUE, from n´kalô, to talk about the commercialization of cashew nuts this year. 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 FCFA / kg in the areas of cashew production. We always recommend selling most of your nuts if a price of 325 FCFA / kg or more offers you, because with the situation of the covid virus, we do not know how the prices will evolve in the coming weeks.
14/07/2020	Robocall 7	Dear stakeholders of the cashew sector, this is André NANQUE, from n´kalô, to talk about the commercialization of cashew nuts this year. 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 FCFA / Kg; in the regions of Bafata and Gabu, prices will vary between 250-350FCFA / Kg; in Biombo and Oio, prices are between 300-350 FCFA / Kg and in Cacheu, there were slight increases of 25 FCFA in prices, with variations between 325-375 FCFA / kg. We recommend selling most of the production when a price of 325 FCFA / kg or more is offered to you ”
16/07/2020	Robocall 8	"Dear stakeholders of the cashew sector, this is André NANQUE, from n’kalô, to talk about the marketing of cashew nuts this year. 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 CFA / kg, whereas last week they were between 370 et 380 CFA / kg. This increase in demand also increased producer prices. In the regions of Bafata, Gabu, Oio and Bolama the producer price is around 300 FCFA / kg. In Quinará and Tombali, producer prices are between 300 et 325 FCFA / kg, slightly better than the previous week. In Biombo, certain producers can sell up to 330 CFA francs / kg, however in Cacheu prices are between 350 et 375 FCFA / kg. As last week, we recommend to sell most or all production when a price of 325 CFA francs / kg or more is offered to you. See you later"
22/07/2020	Robocall 9	"Dear stakeholders of the cashew sector, this is André NANQUE, from n’kalô, to talk about the commercialization of cashew nuts. This week, the competition between nut exporters in Bissau port further increased and prices reached 400 CFA francs / kg. The producer prices also slightly increased. Higher prices continue to be paid in Cacheu, where producer prices are between 350 and 375 CFA francs / kg. In the regions of Bafata, Bolama, Gabu, Biombo and Oio prices are between 300 and 350 CFA / kg i.e. an increase of between 10 and 50 CFA / kg. The lower prices are paid in Quinara and Tombali where cashews are purchased for prices between 300 and 325 CFA / 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 CFA francs / kg to sell all remaining inventory. See you next week".
31/07/2020	Robocall 10	"Dear stakeholders of the cashew sector, this is André NANQUE, from n’kalô, to talk about the commercialization of cashew nuts. This week, the competition between nut exporters in the port of Bissau continues to be great and the prices also increased slightly. The prices productor also increased slightly in the production zones. The highest prices in Cacheu productor reached between 350 and 400 CFA / kg. In the regions of Bafata, Biombo, Bolama, Oio and Gabu prices were 350 CFA francs / 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 CFA / kg last week to 350 and 375 CFA / kg this week. The campaign will end in a few weeks. We therefore recommend negotiating a minimum price of 350 CFA francs / kg to sell all remaining stocks. You can get a good price without waiting too much more to sell. See you next week"
06/08/2020	Robocall 11	"Dear stakeholders of the cashew sector, this is André NANQUE, from n’kalô, to talk about the commercialization of cashew nuts. This week, the marketing campaign cashews is near the end in the regions of Bafata, Biombo, Bolama, and Oio where the last prices paid to Producers were 350 CFA / kg. Quinara and Tombali persist in high demand for cashew nuts and prices paid are between 350 and 375 CFA francs / kg , like last week. In Cacheu, cashews nuts are still purchased between 375 and 400 CFA francs / 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. See you next week"

13/08/2020	Robocall 12	"Dear stakeholders of the cashew sector, this is André NANQUE, from n'kalô, to talk about the commercialization of cashew nuts. This week, the commercialization campaign chestnut 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 CFA francs / kg. In Cacheu, nuts are still purchased between 375 and 400 CFA francs / 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. See you next week"
22/08/2020	Robocall 13	"Dear stakeholders of the cashew sector, this is André NANQUE, from n'kalô, to talk about the commercialization of cashew nuts. This week the marketing season ended in all cashew producing regions. Almost all seasonal trading points of intermediaries from producers have closed, with no purchase and sales of chestnut 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 CFA francs / 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 marketing season. Until the next year! "
09/09/2020	SMS 8 (placebo)	BELAB/N'kalo that interviewed you in 2019 reminds you to keep your sales diary in order to take part in the lottery before the next marketing season.

G 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.[47] 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. Figure 7 shows the daily average prices recorded by the sample of producers that filled in the diaries.[48] 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 figure 8 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 table 30, 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 figure 8 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.

[47] 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.

[48] Table 11 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.

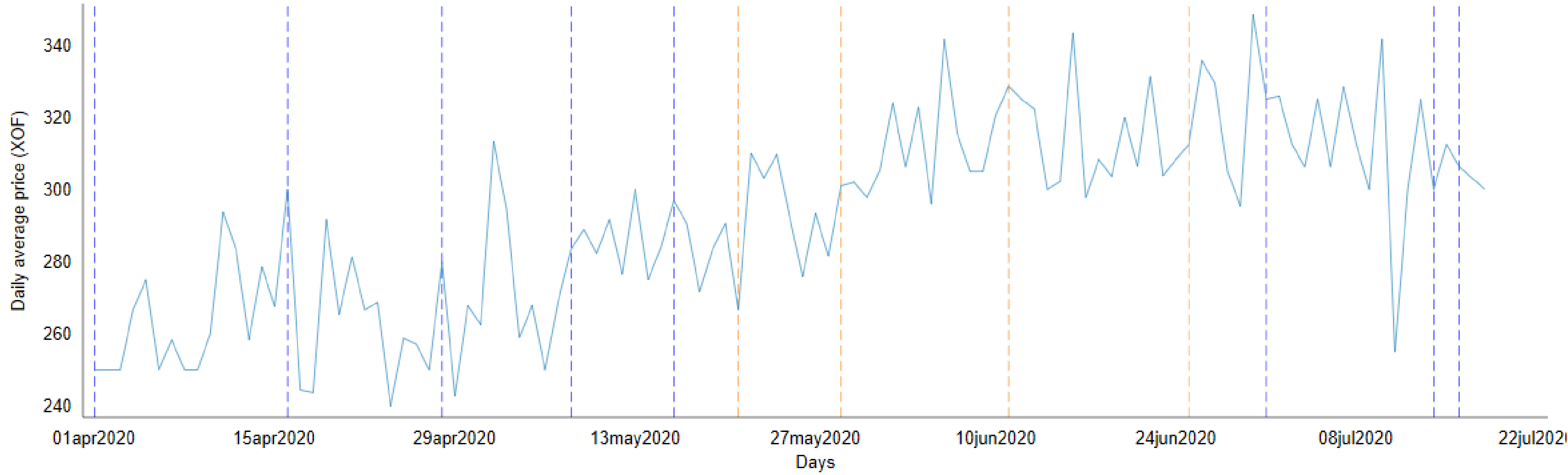

Figure 7: Daily average prices recorded in transaction diaries[a]

[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.

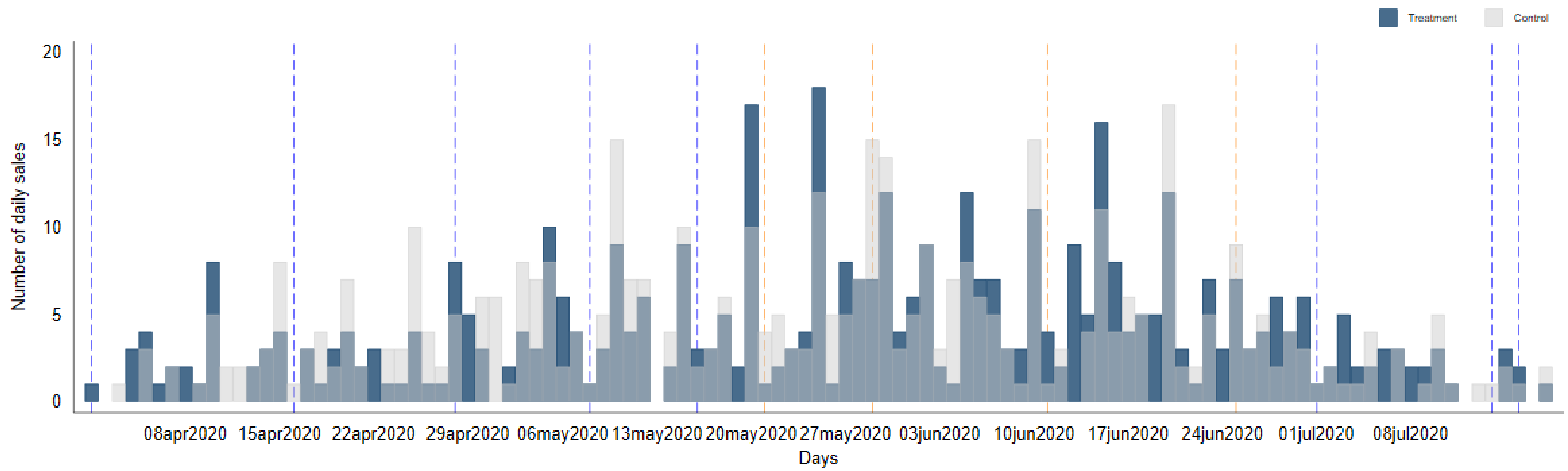

Figure 8: Number of daily sales recorded in transaction diaries by treatment[a]

[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.

Table 30: Results — Sales recorded in transaction diaries

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

Notes: producer-level intention-to-treat (ITT) estimates 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.

H 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 31 shows the main outcomes measured at the intermediary-level across treated and control villages.

Table 31: Village intermediaries outcomes

	(1) Control	(2) Treatment	(3) p-value (1) - (2)	(4) N
1 if intermediary was interviewed in 2021	0.42	0.47	-0.05	
	(0.50)	(0.50)	[0.39]	304.00
1 if made any purchase	0.85	0.83	0.02	
	(0.36)	(0.38)	[0.71]	136.00
Number of producers with whom they exchanged	4.23	6.32	-2.09	
	(6.47)	(10.21)	[0.16]	131.00
Quantity purchased	10626.15	11823.76	-1197.62	
	(17849.26)	(16001.22)	[0.70]	136.00
Purchase price	314.63	316.10	-1.46	
	(51.53)	(39.39)	[0.88]	114.00
Resale price	395.00	382.71	12.29	
	(71.06)	(81.00)	[0.57]	53.00
Margin	74.17	65.57	8.60	
	(67.37)	(78.32)	[0.68]	53.00
1 if made any exchange	0.56	0.60	-0.04	
	(0.50)	(0.49)	[0.66]	136.00
Quantity of cashews obtained from exchanges	2749.68	3145.35	-395.67	
	(4103.98)	(5085.62)	[0.63]	132.00
Quantity of rice exchanged for cashews	2545.21	2967.53	-422.32	
	(3942.35)	(4702.57)	[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 32 that control villages received a higher number of visits by itinerant intermediaries.

Table 32: Village intermediaries visits

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

I Heterogeneous treatment effects

Table 33: Heterogenous treatment effects by terciles of road distance to a regional capital

	Control (1) Mean (SD) Total obs.	(2) Treat. * Low	(3) Treat. * Med.	(4) Treat. * High	(5) (3) - (2)	(6) (4) - (2)	(7) (4) - (3)
Price per sale per kg	303.79	0.11	-3.53	10.82*	-3.64	10.71	14.35**
	(85.32)	(4.39)	(4.44)	(6.42)	(5.74)	(7.00)	(7.12)
	1587	[0.98]	[0.86]	[0.19]	[0.70]	[0.25]	[0.18]
Value of all sales and exchanges	272948.05	67936.16***	-4888.07	21916.94	-72824.23**	-46019.23	26805.00
	(207665.38)	(22621.58)	(26598.41)	(34886.85)	(32362.38)	(38339.99)	(41344.63)
	1527	[0.01]***	[0.86]	[0.71]	[0.05]*	[0.26]	[0.69]
Value of all sales	217907.89	61873.01***	-4364.32	8115.94	-66237.33**	-53757.07	12480.25
	(186020.31)	(21385.94)	(24123.41)	(29444.67)	(29962.69)	(33402.08)	(35686.51)
	1594	[0.01]***	[0.86]	[0.78]	[0.05]*	[0.25]	[0.73]
Value of exchanges	53500.32	6417.11	2939.80	21007.29*	-3477.31	14590.19	18067.50
	(79854.80)	(9336.21)	(10931.28)	(12083.82)	(12666.80)	(12819.25)	(14098.34)
	1639	[0.66]	[0.86]	[0.19]	[0.78]	[0.26]	[0.40]

Notes: producer-level heterogeneous treatment effect. Column 2 to 4 report the coefficients from interacting the treatment indicator with an indicator for each of the three baseline road distance in km to nearest region capital terciles (where low or medium or high road distance in km to nearest region capital refers to individuals who were in the bottom or middle or highest terciles at baseline). The omitted category represents individuals in the control group from the lowest tercile of road distance in km to nearest region capital. Column 5 reports the difference between column 3 and 2. Column 6 reports the difference between column 4 and 2. Column 7 reports the difference between column 4 and 3. 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; an indicator for being in the spillover group; and two indicators equal to 1 if the individual had respectively medium or high road distance in km to nearest region capital at baseline. 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 and standard deviation for the omitted group in the control group and in the bottom tercile of road distance in km to nearest region capital; and total number of observations across all groups.

Table 34: Heterogenous treatment effects by terciles of road distance to a regional capital

	Control (1) Mean (SD) Total obs.	(2) Treat. * Low	(3) Treat. * Med.	(4) Treat. * High	(5) (3) - (2)	(6) (4) - (2)	(7) (4) - (3)
Total quantity sold	674.53	179.51***	9.53	26.15	-169.98*	-153.37*	16.62
	(555.58)	(63.84)	(69.94)	(81.23)	(89.53)	(92.52)	(98.63)
	1625	[0.01]**	[0.95]	[0.75]	[0.24]	[0.47]	[0.87]
Number of sales	1.52	0.26***	0.10	0.24***	-0.16	-0.03	0.13
	(0.90)	(0.08)	(0.09)	(0.09)	(0.12)	(0.12)	(0.11)
	1693	[0.01]***	[0.95]	[0.05]**	[0.29]	[0.81]	[0.87]
1 if exchanged cashew for rice	0.58	-0.04	0.00	0.02	0.04	0.06	0.02
	(0.49)	(0.05)	(0.05)	(0.05)	(0.06)	(0.06)	(0.06)
	1707	[0.58]	[0.95]	[0.75]	[0.68]	[0.47]	[0.87]
Total quantity exchanged	235.18	-1.42	11.18	38.65	12.60	40.08	27.47
	(337.54)	(33.14)	(35.11)	(38.16)	(43.92)	(45.04)	(47.40)
	1685	[0.96]	[0.95]	[0.75]	[0.77]	[0.47]	[0.87]
Share of quantity sold over quantity exchanged and sold	0.74	0.05*	-0.02	0.01	-0.06*	-0.03	0.03
	(0.29)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
	1610	[0.15]	[0.95]	[0.75]	[0.24]	[0.47]	[0.87]
Loans:							
1 if borrowed money or rice	0.39	-0.03	0.09**	-0.00	0.12**	0.03	-0.09
	(0.49)	(0.04)	(0.04)	(0.04)	(0.06)	(0.06)	(0.06)
	1703	[0.57]	[0.06]*	[0.96]	[0.06]*	[0.85]	[0.19]
Implicit price of loans per kg of cashews	89.96	-4.59	24.91**	15.45**	29.50**	20.04**	-9.46
	(185.10)	(8.13)	(10.32)	(6.99)	(12.68)	(10.00)	(11.50)
	1645	[0.57]	[0.06]*	[0.11]	[0.04]**	[0.18]	[0.41]
Total quantity paid for loans	68.20	-10.25	12.15	-12.31	22.41*	-2.06	-24.46
	(137.85)	(10.73)	(9.69)	(12.49)	(12.18)	(15.26)	(15.38)
	1687	[0.57]	[0.21]	[0.65]	[0.07]*	[0.89]	[0.19]
Share of quantity sold over quantity exchanged and paid in loans	0.86	0.04**	-0.04	0.02	-0.08**	-0.02	0.06
	(0.26)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
	1603	[0.15]	[0.21]	[0.65]	[0.04]**	[0.85]	[0.19]

Notes: producer-level heterogeneous treatment effect. Column 2 to 4 report the coefficients from interacting the treatment indicator with an indicator for each of the three baseline road distance in km to nearest region capital terciles (where low or medium or high road distance in km to nearest region capital refers to individuals who were in the bottom or middle or highest terciles at baseline). The omitted category represents individuals in the control group from the lowest tercile of road distance in km to nearest region capital. Column 5 reports the difference between column 3 and 2. Column 6 reports the difference between column 4 and 2. Column 7 reports the difference between column 4 and 3. 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; an indicator for being in the spillover group; and two indicators equal to 1 if the individual had respectively medium or high road distance in km to nearest region capital at baseline. 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 and standard deviation for the omitted group in the control group and in the bottom tercile of road distance in km to nearest region capital; and total number of observations across all groups.

Table 35: Heterogenous treatment effects by terciles of wealth index

	Control (1) Mean (SD) Total obs.	(2) Treat. * Low	(3) Treat. * Med.	(4) Treat. * High	(5) (3) - (2)	(6) (4) - (2)	(7) (4) - (3)
Price per sale per kg	278.45	-5.52	2.88	7.69	8.40	13.21*	4.81
	(109.37)	(4.89)	(5.09)	(5.98)	(7.26)	(7.45)	(6.76)
	1587	[0.35]	[0.76]	[0.39]	[0.33]	[0.15]	[0.91]
Value of all sales and exchanges	211712.37	44136.79*	68489.99*	49495.66	24353.20	5358.87	-18994.33
	(212600.51)	(24333.65)	(38897.53)	(46691.76)	(39594.30)	(50032.23)	(56945.67)
	1527	[0.14]	[0.16]	[0.39]	[0.54]	[0.91]	[0.91]
Value of all sales	171035.43	13721.72	69851.65*	45139.45	56129.93	31417.73	-24712.20
	(189269.08)	(21064.00)	(36537.46)	(41444.03)	(36105.40)	(44416.12)	(52253.59)
	1594	[0.51]	[0.16]	[0.39]	[0.24]	[0.64]	[0.91]
Value of exchanges	38399.48	28972.38***	88.19	1779.00	-28884.19**	-27193.38*	1690.81
	(64196.07)	(10469.43)	(11686.07)	(12621.11)	(13907.96)	(14208.25)	(15294.13)
	1639	[0.02]**	[0.99]	[0.89]	[0.15]	[0.15]	[0.91]

Notes: producer-level heterogeneous treatment effect. Column 2 to 4 report the coefficients from interacting the treatment indicator with an indicator for each of the three baseline standardised principal component of wealth terciles (where low or medium or high standardised principal component of wealth refers to individuals who were in the bottom or middle or highest terciles at baseline). The omitted category represents individuals in the control group from the lowest tercile of standardised principal component of wealth. Column 5 reports the difference between column 3 and 2. Column 6 reports the difference between column 4 and 2. Column 7 reports the difference between column 4 and 3. 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; an indicator for being in the spillover group; and two indicators equal to 1 if the individual had respectively medium or high standardised principal component of wealth at baseline. 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 and standard deviation for the omitted group in the control group and in the bottom tercile of standardised principal component of wealth; and total number of observations across all groups.

Table 36: Heterogenous treatment effects by terciles of wealth index

	Control (1) Mean (SD) Total obs.	(2) Treat. * Low	(3) Treat. * Med.	(4) Treat. * High	(5) (3) - (2)	(6) (4) - (2)	(7) (4) - (3)
Total quantity sold	540.50	24.05	112.53	68.17	88.48	44.12	-44.36
	(604.69)	(57.52)	(73.56)	(82.50)	(84.71)	(94.07)	(98.98)
	1625	[0.68]	[0.21]	[0.68]	[0.37]	[0.64]	[0.87]
Number of sales	1.34	0.23**	0.22**	0.15*	-0.02	-0.08	-0.06
	(0.92)	(0.09)	(0.09)	(0.09)	(0.13)	(0.12)	(0.12)
	1693	[0.05]*	[0.10]*	[0.44]	[0.90]	[0.64]	[0.87]
1 if exchanged cashew for rice	0.56	0.06	-0.10**	0.03	-0.15***	-0.03	0.12**
	(0.50)	(0.04)	(0.05)	(0.05)	(0.06)	(0.06)	(0.06)
	1707	[0.28]	[0.10]*	[0.77]	[0.03]**	[0.64]	[0.21]
Total quantity exchanged	163.89	68.11**	-24.43	-11.35	-92.54**	-79.46*	13.08
	(225.97)	(29.12)	(34.95)	(39.82)	(41.89)	(43.83)	(48.69)
	1685	[0.05]*	[0.49]	[0.77]	[0.07]*	[0.33]	[0.87]
Share of quantity sold over quantity exchanged and sold	0.70	-0.03	0.04	0.03	0.06	0.06	-0.01
	(0.33)	(0.04)	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)
	1610	[0.59]	[0.29]	[0.68]	[0.25]	[0.33]	[0.87]
Loans:							
1 if borrowed money or rice	0.32	0.06	0.00	0.00	-0.06	-0.06	0.00
	(0.47)	(0.04)	(0.04)	(0.04)	(0.06)	(0.06)	(0.06)
	1703	[0.38]	[0.98]	[0.94]	[0.58]	[0.30]	[0.97]
Implicit price of loans per kg of cashews	66.52	3.90	5.85	25.83**	1.95	21.93	19.98
	(157.32)	(8.68)	(7.00)	(11.26)	(9.89)	(14.53)	(13.81)
	1645	[0.65]	[0.81]	[0.09]*	[0.84]	[0.26]	[0.60]
Total quantity paid for loans	42.39	13.60	-13.23	-10.26	-26.83*	-23.86	2.97
	(103.94)	(10.29)	(10.75)	(12.52)	(14.05)	(15.41)	(15.85)
	1687	[0.38]	[0.81]	[0.55]	[0.23]	[0.26]	[0.97]
Share of quantity sold over quantity exchanged and paid in loans	0.83	-0.02	0.01	0.03	0.03	0.05	0.02
	(0.32)	(0.03)	(0.03)	(0.02)	(0.04)	(0.04)	(0.03)
	1603	[0.65]	[0.87]	[0.50]	[0.63]	[0.28]	[0.97]

Notes: producer-level heterogeneous treatment effect. Column 2 to 4 report the coefficients from interacting the treatment indicator with an indicator for each of the three baseline standardised principal component of wealth terciles (where low or medium or high standardised principal component of wealth refers to individuals who were in the bottom or middle or highest terciles at baseline). The omitted category represents individuals in the control group from the lowest tercile of standardised principal component of wealth. Column 5 reports the difference between column 3 and 2. Column 6 reports the difference between column 4 and 2. Column 7 reports the difference between column 4 and 3. 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; an indicator for being in the spillover group; and two indicators equal to 1 if the individual had respectively medium or high standardised principal component of wealth at baseline. 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 and standard deviation for the omitted group in the control group and in the bottom tercile of standardised principal component of wealth; and total number of observations across all groups.

Table 37: Heterogenous treatment effects by terciles of the number of producers in the village

	Control (1) Mean (SD) Total obs.	(2) Treat. * Low	(3) Treat. * Med.	(4) Treat. * High	(5) (3) - (2)	(6) (4) - (2)	(7) (4) - (3)
Price per sale per kg	300.26	6.41	4.03	-4.21	-2.38	-10.63*	-8.25
	(92.22)	(4.57)	(6.41)	(4.81)	(7.12)	(6.19)	(7.51)
	1587	[0.21]	[0.53]	[0.51]	[0.74]	[0.35]	[0.88]
Value of all sales and exchanges	285977.20	80644.13**	45371.22	63190.29	-35272.91	-17453.84	17819.07
	(284618.62)	(32028.85)	(30775.74)	(48775.22)	(39306.66)	(54232.80)	(53821.95)
	1527	[0.03]**	[0.43]	[0.43]	[0.74]	[0.81]	[0.88]
Value of all sales	232059.58	68269.26**	34085.25	55872.28	-34184.01	-12396.99	21787.02
	(257550.49)	(27823.94)	(27309.00)	(45176.50)	(34263.34)	(50523.67)	(49474.26)
	1594	[0.03]**	[0.43]	[0.43]	[0.74]	[0.81]	[0.88]
Value of exchanges	50937.01	16670.69	8742.08	6238.30	-7928.61	-10432.39	-2503.78
	(95924.22)	(13335.56)	(12379.33)	(11009.67)	(16554.12)	(15488.47)	(16001.23)
	1639	[0.21]	[0.53]	[0.57]	[0.74]	[0.81]	[0.88]

Notes: producer-level heterogeneous treatment effect. Column 2 to 4 report the coefficients from interacting the treatment indicator with an indicator for each of the three baseline number of cashew producers in the village terciles (where low or medium or high number of cashew producers in the village refers to individuals who were in the bottom or middle or highest terciles at baseline). The omitted category represents individuals in the control group from the lowest tercile of number of cashew producers in the village. Column 5 reports the difference between column 3 and 2. Column 6 reports the difference between column 4 and 2. Column 7 reports the difference between column 4 and 3. 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; an indicator for being in the spillover group; and two indicators equal to 1 if the individual had respectively medium or high number of cashew producers in the village at baseline. 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 and standard deviation for the omitted group in the control group and in the bottom tercile of number of cashew producers in the village; and total number of observations across all groups.

Table 38: Heterogenous treatment effects by terciles of the number of producers in the village

	Control (1) Mean (SD) Total obs.	(2) Treat. * Low	(3) Treat. * Med.	(4) Treat. * High	(5) (3) - (2)	(6) (4) - (2)	(7) (4) - (3)
Total quantity sold	700.63	129.67*	79.24	41.78	-50.43	-87.89	-37.46
	(739.32)	(67.46)	(67.03)	(82.52)	(88.00)	(100.42)	(96.80)
	1625	[0.28]	[0.58]	[0.62]	[0.99]	[0.38]	[0.76]
Number of sales	1.59	0.08	0.26***	0.30***	0.19	0.22*	0.04
	(1.00)	(0.09)	(0.08)	(0.09)	(0.12)	(0.12)	(0.12)
	1693	[0.49]	[0.01]***	[0.00]***	[0.56]	[0.15]	[0.76]
1 if exchanged cashew for rice	0.54	0.05	0.04	-0.11***	-0.01	-0.16***	-0.15**
	(0.50)	(0.05)	(0.05)	(0.04)	(0.06)	(0.06)	(0.06)
	1707	[0.49]	[0.58]	[0.02]**	[0.99]	[0.02]**	[0.09]*
Total quantity exchanged	218.99	31.89	31.51	-17.49	-0.38	-49.38	-49.00
	(320.31)	(37.25)	(33.46)	(34.76)	(45.81)	(47.30)	(44.79)
	1685	[0.49]	[0.58]	[0.62]	[0.99]	[0.37]	[0.46]
Share of quantity sold over quantity exchanged and sold	0.74	-0.00	-0.01	0.06*	-0.00	0.06	0.07*
	(0.32)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
	1610	[0.95]	[0.87]	[0.11]	[0.99]	[0.17]	[0.24]
Loans:							
1 if borrowed money or rice	0.41	-0.02	0.07*	0.00	0.09	0.02	-0.07
	(0.49)	(0.04)	(0.04)	(0.04)	(0.06)	(0.06)	(0.06)
	1703	[0.71]	[0.35]	[0.95]	[0.53]	[0.75]	[0.85]
Implicit price of loans per kg of cashews	67.67	15.94	11.18	10.97	-4.76	-4.97	-0.21
	(155.55)	(9.96)	(9.36)	(6.84)	(13.31)	(10.89)	(11.52)
	1645	[0.43]	[0.47]	[0.33]	[0.72]	[0.75]	[0.99]
Total quantity paid for loans	67.84	-14.50	0.86	3.38	15.36	17.88	2.53
	(144.13)	(11.61)	(12.77)	(9.16)	(17.02)	(12.77)	(15.61)
	1687	[0.43]	[0.95]	[0.95]	[0.72]	[0.35]	[0.99]
Share of quantity sold over quantity exchanged and paid in loans	0.85	-0.01	0.01	0.04	0.02	0.05	0.03
	(0.29)	(0.02)	(0.03)	(0.03)	(0.04)	(0.03)	(0.04)
	1603	[0.71]	[0.95]	[0.33]	[0.72]	[0.35]	[0.85]

Notes: producer-level heterogeneous treatment effect. Column 2 to 4 report the coefficients from interacting the treatment indicator with an indicator for each of the three baseline number of cashew producers in the village terciles (where low or medium or high number of cashew producers in the village refers to individuals who were in the bottom or middle or highest terciles at baseline). The omitted category represents individuals in the control group from the lowest tercile of number of cashew producers in the village. Column 5 reports the difference between column 3 and 2. Column 6 reports the difference between column 4 and 2. Column 7 reports the difference between column 4 and 3. 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; an indicator for being in the spillover group; and two indicators equal to 1 if the individual had respectively medium or high number of cashew producers in the village at baseline. 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 and standard deviation for the omitted group in the control group and in the bottom tercile of number of cashew producers in the village; and total number of observations across all groups.

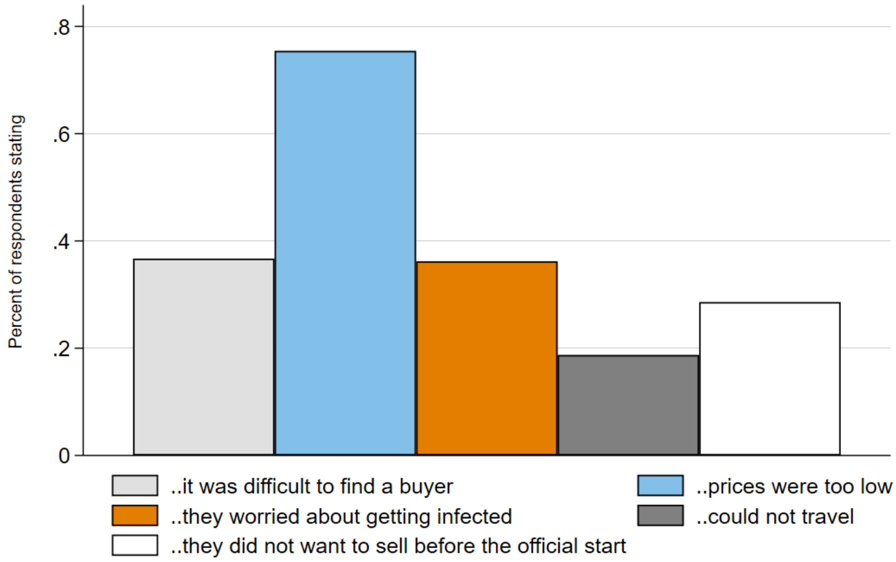

Figure 9: Reasons for delaying cashew sales in 2020

J Changes between the 2019 and 2020 seasons

How did the raw cashew nut market react to the pandemic in 2020?

Usually the cashew trading season in Guinea-Bissau starts in late March. In 2020, due to the restrictions imposed by the government, the official start of the season was postponed until the 27th of May.[49] Due to liquidity needs, little bargaining power and high uncertainty many producers had to start selling their cashews before the official start of the cashew marketing season, at prices as low as 200 XOF per kg, compared to a national average of 474 XOF per kg in 2018 and 351 XOF per kg in 2019.
However, some producers decided to postpone their planned cashew sales due to the COVID-19 implications. Out of the producers that we contacted during a short phone-survey, between July and September 2020, 69% stated that they delayed their cashew sales against their original intentions because of the COVID-19 pandemic. Most reported that prices offers were too low, that they had difficulties in finding a buyer, or that they were worried about getting infected (see figure 9).
The large drops in price and in quantities of cashew nuts sold in 2020 resulted in a dramatic fall in revenue compared to previous years.

A comparison of the 2019 and 2020 trading seasons

In this section, we analysed data from the sample of producers in the 103 control villages in which we did not introduce any new market information system.[50] We report their outcomes collected over two in-person interviews conducted between November and December 2019 and later between April and May of 2021. In those interviews, we asked producers about their cashew sales and other

[49] It is technically unlawful to sell or purchase of raw cashew nuts before the government announces the official start of the trading season.
[50] These villages were randomly chosen to be the comparison group for our impact evaluation of a market information system. We had randomly chosen them to *not* receive this intervention during 2020.

Table 39: Timing of Sales in 2019 and 2020

	(1) 2019	(2) 2020	(3) (1) - (2) p-value	(4) N
1 if sold cashews in April or before	0.20 (0.40)	0.15 (0.35)	0.05 [0.02]	521
1 if sold cashews in May	0.51 (0.50)	0.51 (0.50)	-0.00 [0.90]	521
1 if sold cashews in June	0.55 (0.50)	0.49 (0.50)	0.05 [0.07]	521
1 if sold cashews in July or later	0.19 (0.39)	0.07 (0.25)	0.12 [0.00]	521

Notes: Column 1 reports the mean in 2019. Column 2 reports the mean in 2020. Outcome variables are listed on the left. 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 2019 and 2020 in brackets.

indicators in relation to the 2019 and 2020 trading seasons, respectively.[51]
During the first round of interviews in 2019, we had interviewed 694 producers across 103 villages. Of these, 87 producers could not be reached when we conducted the second round of in-person interviews in 2021, an attrition rate of 12%.

In 2020, a lower percentage of producers sold any cashews in the month of April or earlier, as can be seen in table 39, relative to 2019, because of the delayed start of the official trading season. However, cashew nuts transactions still took place before the official start of the season (27th of May for 2020): 15% of the sample sold in the month of April or earlier, and the percentage of producers who sold in the month of May is the same in 2019 and in 2020. A lower percentage of producers sold any cashews after the month of May compared to the same period in 2019.
The distribution of the average price received by the interviewed producers in 2020 shifted to the left in comparison to the same distribution for 2019, see figure 10. This figures shows that the prices in 2020 were more concentrated and lower on average than in 2019. We also found that the proportion of producers that made no sales at all was higher in 2020 than in 2019, as shown by the bar on the left of the histogram.[52]
In table 40 we see that the reduction in prices during the pandemic also contributed to a reduction in the revenue earned by producers relative to 2019. Average prices per kg fell by 24% on average

[51] The response are unlikely to be affected by seasonality effects, despite the timing of the interviews being across the two rounds, since we asked respondents about their cashew trades during the prior trading season, which occurs around the same time each year. Concretely, during the interviews we conducted in 2021, we asked respondents about their trades in 2020. April and May are usually the first months of the cashew trading season, whereas November and December are pre-harvest months for cashew producers.

[52] We re-coded the price to be zero for producers that did not conduct any sales.

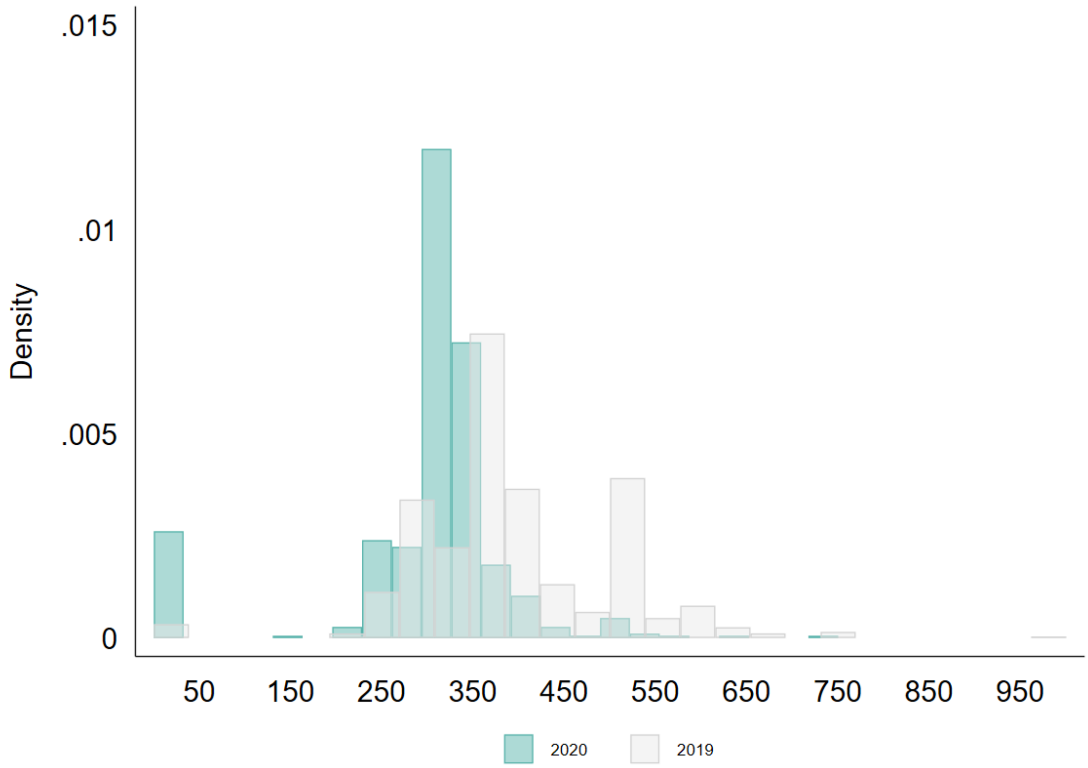

Figure 10: Histogram of average cashew prices in 2019 and 2020

Table 40: Prices and revenue from Sales and Exchanges in 2019 and 2020

	(1) 2019	(2) 2020	(3) (1) - (2) p-value	(4) N
Price per sale per kg	389	295	94	
	(104)	(105)	[0.00]	564
Value of all sales and exchanges	511,949	271,023	240,925	
	(880,506)	(310,374)	[0.00]	594
Value of all sales	474,793	233,494	241,298	
	(869,688)	(292,721)	[0.00]	564
Value of exchanges	37,939	52,325	-14,386	
	(83,559)	(88,037)	[0.00]	589

Notes: Column 1 reports the mean in 2019. Column 2 reports the mean in 2020. Outcome variables are listed on the left. 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 2019 and 2020 in brackets. All values are in West African CFA.

between 2019 and 2020. The fall in revenue earned from cashew transactions declined even further, falling by 47% relative to pre-pandemic levels. The drop in revenue is fully accounted by monetary sales and from changes in the value of cashews bartered for rice.[53] On the contrary, we observe that the value producers obtained from cashew exchanges was higher in 2020 relative to 2019, though far from enough to compensate the lower sales revenue, as the value of sales decreased by 50.8%.

Two patterns emerge from the comparisons across trading seasons of the quantities of cashews nuts produced and used for different purposes. First, there was a much higher quantity produced and sold overall in 2019 relative to 2020. The average production by producer went down by 394 kg, a 27% average reduction from 2019 to 2020, as shown in table 41. The drop in production might be explained by worse agronomic conditions in 2020 relative to the previous year.

Second, a much higher percentage of producers exchanged cashew for rice, both on the extensive and intensive margins. On the extensive one, the percentage of producers bartering their cashew nuts increased from 30% in 2019 to 56% in 2020. On the intensive margin, average quantity exchanged by producers increased by 103 kg, or 91% in 2020 relative to 2019. As a result, the average quantity sold decreased by even more than the total quantity produced, from 1194 kg (or 81% of total production) to 708 kg (or 65% of total production). This 486 kg drop in sales represents 41% of the average quantity sold in 2020.

The last three rows in table 41 help us understand how producers reacted to the downturn in both cashew prices and production through either borrowing or engaging in alternative activities to diversity sources of income.

The percentage of producers receiving credit, either in money or rice, decreased from 37% in 2019 to 31% in 2019. In the same direction, the value of the loans repaid in cashews decreased from 16,294 to 12,994, but this difference is not statistically significant. The reduction in credit between 2019 and 2020 may be due to either supply-side or demand-side factors. On the demand-side, the perspective of a poor trading season reduces the expected value of cashews, which can reduce

[53] It is common for producers to barter cashew nuts for rice, especially when cashew prices fall to levels that are close to the price of rice per kg, which makes the conversion easier to process.

Table 41: Quantities in 2019 and 2020

	(1) 2019	(2) 2020	(3) (1) - (2) p-value	(4) N
Total quantity produced	1473	1079	394	
	(1935)	(1000)	[0.00]	444
Total quantity sold	1194	708	486	
	(1772)	(85)	[0.00]	569
1 if exchanged cashews for rice	0.30	0.56	-0.26	
	(0.46)	(0.50)	[0.00]	604
Total quantity exchanged	114	218	-103	
	(248.78)	(323.48)	[0.00]	599
1 if borrowed money or rice	0.37	0.31	0.06	
	(0.48)	(0.46)	[0.03]	603
Value of all loans repaid in cashews	16,295	12,995	3,300	
	(47,354)	(66,460)	[0.32]	602.00
1 if sold other crops	0.67	0.97	-0.30	
	(0.47)	(0.18)	[0.00]	607

Notes: Column 1 reports the mean in 2019. Column 2 reports the mean in 2020. Outcome variables are listed on the left. 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 2019 and 2020 in brackets. Quantities are in metric kg and the value of loans is in West African CFA.

demand for credit as producers' collateral falls in value. On the supply-side, a worsening market outlook for cashew nuts can reduce the liquidity of informal loan providers in Guinea-Bissau, who are often involved in the cashew nut value chain as either intermediaries or exporters.[54]

A higher share of producers diversified their portfolio, as a likely coping mechanism producers adopted in 2020 to deal with a worse cashew market: the percentage of producers selling crops different from cashew increased from 67% in 2019 to 97% in 2020. This indicates an important substitution effect between cashew revenue and trading of alternative crops, at least on the extensive margin.

[54] Many exporters that finance loans to intermediaries and producers could not reach the country because of the closed international borders. This lack of credit was only partly relaxed by the government, which stepped in by providing a 5 billion XOF loan to national banks to finance trades after the cashew season had started.