Interlinked Transactions and Competition: Experimental Evidence from Cocoa Markets*

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Abstract

Interlinked transactions in which output prices are determined jointly with the terms of a credit contract are an important feature of many business relationships, particularly in developing economies. In these instances, measuring competition requires information on credit as well as prices. We illustrate this idea using a randomized experiment in the Sierra Leone cocoa value chain, which subsidizes traders who purchase output from farmers. Our results show that traders pass value to farmers either through output prices or the provision of credit. A model then shows how the experiment can inform estimation of the market power of traders. Accounting for both the credit and output market transactions, we find limited evidence of market power. Additional results on the pass-through of industry-wide price shocks, market size, and quantity purchased by traders during the experiment confirm that the market for farmers' output is competitive.

JEL Classification: O12, O16, Q13

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1 Introduction

The degree to which intermediaries compete is a long-standing object of interest in the study of trade within countries. A large literature has studied competition by looking at the price response to cost shocks (see, e.g., Goldberg and Knetter, 1997; Weyl and Fabinger, 2013; Atkin and Donaldson, 2015). Where formal financial institutions are absent, however, price may not be the only margin on which intermediaries compete. A tradition in development economics has emphasized that intermediaries frequently serve as a substitute source of credit for consumers and (agricultural) producers, introducing transactions that are *interlinked*: the price at which output is purchased is determined jointly with the terms of the credit contract, and vice versa (Bardhan, 1980; Bell, 1988).[1] In such a context, understanding competition requires an examination of the impact of cost shocks on credit provision, as well as price.

We illustrate this idea using a randomized experiment that directly subsidizes a subset of intermediaries operating in an agricultural value chain. The intermediaries are traders purchasing raw cocoa (cacao) directly from farmers in West Africa, where approximately 75 percent of global supply originates (ICCO, 2019). Here, farm-gate traders may provide farmers credit in the form of advance payments for output. Credit is interlinked, as it is repaid when the farmer accepts a below market price for output at harvest time. The experiment induces variation in the traders' marginal revenue: a random sample of traders are paid a per-unit bonus (approximately 5% of the price traders pay to farmers) for delivering high quality cocoa to five major wholesalers in Sierra Leone's cocoa producing region. Using original data on traders' transactions with farmers, we show that while the bonus causes little difference in the average price paid by treatment and control traders to farmers, treatment traders are substantially more likely to provide credit to farmers (25% vs. 11%).

It is possible to quantify the value of credit for the farmer. Underlying the differences between treatment and control, there is important heterogeneity: in those villages in which the difference in credit outlay is smaller, the difference in prices paid is larger. This suggests that, within the interlinked transaction, there is an indifference curve between credit and price. Estimation of this curve's slope delivers a significant and quantitatively reasonable value of advance payments: a village in which the difference in credit provision between treatment and control traders is equal to

[1] Casaburi and Willis (2018), Casaburi and Macchiavello (2019), Ghani and Reed (2019), and Macchiavello and Morjaria (2019) provide recent empirical contributions on interlinkages in agricultural value chains in Africa. Interlinkages are also important in advanced countries, for instance in the form of trade credit, which is an important source of finance for smaller firms (Petersen and Rajan, 1997), certain industries (Fisman and Love, 2003), and for international trade (Antras and Foley, 2015). In the United States, non-farm enterprises with fewer than 500 employees rely on trade credit for about 60% of their external finance (Mach, 2006).

the average in our sample (i.e. 14%), the difference in price is lower by one-sixth to one-third of the subsidy value, relative to a village with no effect of the bonus on credit. One may combine these effects and report them in terms of a single "effective price", which accounts for both the value of credit as well as the output price, akin to the net present value of the interlinked transaction. Put in these terms, treatment traders pay farmers, on average, an effective price that was higher than control by about one-sixth of the subsidy value.

We use our experimental results on prices, credit, and the value of credit to identify the degree of competition in the market, accounting for interlinkages. Several recent studies have applied an experimental or quasi-experimental approach to study competition by exploiting variation *across markets* (Busso and Galiani, 2019, Jensen and Miller, 2018, Mitra et al., 2018, Rotemberg, forthcoming, Bergquist, 2017).[2] In our setting, like many others, such an approach is not feasible. Traders are highly mobile over a large geographic area, and purchase output in several towns, making it difficult to geographically delineate markets that are truly independent. In such settings, tests for imperfect competition are typically implemented using a time series of prices (and costs) and instrumental variables (Graddy, 1995; Osborne, 2005). We demonstrate that cross-sectional variation in firms' marginal profit, induced by trader-level randomization, can provide additional information that is not available using this approach. In a simple model of oligopsonistic imperfect competition among (potentially) differentiated traders, experimental variation identifies the traders' differentiation parameter, which summarizes a given trader's ability to buy while paying a lower price than competitors.

To identify the differentiation parameter, we make use of the idea that competitive forces may cause a violation of the stable unit treatment value assumption (SUTVA) required for standard experimental analysis (Donaldson, 2015). If they operate in an integrated market, unsubsidized traders may adjust their behavior strategically when some of their competitors receive a subsidy. As a result, differences between prices paid to farmers by treatment and control traders cannot be interpreted as Rubin (1974) average treatment effects. Rather, treatment and control price differences provide a test of the null hypothesis that traders are undifferentiated from the farmer's perspective—the law of one price holds. Randomization is, however, still essential for the analysis, as it ensures subsidies are uncorrelated with trader characteristics and thus that the treatment-control price differences arise only because of the subsidy.

One may estimate the differentiation rate using treatment and control differences in the effective

[2]Other recent empirical work based on experiments featuring market-level variation includes Crépon et al. (2013), Cunha et al. (2018), Lalive et al. (2015), Baird et al. (2018), Hildebrandt et al. (2015), Burke et al. (2018), Mobarak and Rosenzweig (2014), Muralidharan et al. (2016), McKenzie and Puerto (2017), and Breza and Kinnan (2016).

price (i.e. including the value of credit), or differences in the output price alone. The difference between these two estimates illustrates how the presence of interlinkages may affect inference about competition. Though the noise in the estimate of the value of credit unavoidably introduces uncertainty into our estimates, the rate of differentiation estimated when using effective price is higher than when using the output price alone.

Nonetheless, even when accounting for the value of credit, the differentiation parameter is still relatively low in absolute value, suggesting that overall the trading sector is competitive. Several additional results are consistent with this conclusion. First, using the world price as an instrumental variable, we find that the price pass-through rate to a market-level shock is 0.92, nearly complete (and in line with other studies of the cocoa sector in the region (e.g., Gayi and Tsowou, 2015). Second, combining the experimental result with this IV result, our model implies that traders behave as if they competed with about 13 competitors, approximately twice the number of traders operating in a village. This is consistent with the idea that villages are contestable, and that farmers can sell their cocoa outside their village. Third, in our experiment, treatment traders, who pay a slightly higher effective price, purchase almost three times as much cocoa as control traders, consistent again with a low level of market power of each trader. The model's estimated parameter values imply that the difference in quantity purchases comes entirely from market stealing, rather than an increase in aggregate supply. Our conclusion that farm-gate markets are competitive is consistent with a recent review by Dillon and Dambro (2017).

Studies of competition in farm-gate markets have primarily relied on observational, rather than experimental, evidence, analyzing trader price-cost margins (For the case of Sub-Saharan Africa, see, e.g., Fafchamps et al., 2005; Osborne, 2005; Sitko and Jayne, 2014), price dispersion across space (Fackler and Goodwin, 2001; Aker, 2010), or the pass-through of international prices along the supply chain (Fafchamps and Hill, 2008; Dillon and Barrett, 2015).[3] To the best of our knowledge, this is the first experiment that randomized any treatment at the trader level in agricultural markets and that used experimental subsidies to study competition. Two recent papers, Bergquist (2017) and Iacovone and McKenzie (2019), study how *retail vendors* (of maize and fresh produce, respectively) adjust consumer prices in response to subsidies affecting marginal costs. We innovate on existing work by explicitly including into our analysis of output market competition the value of an interlinked credit market transaction. While other work has emphasized that competition may reduce the supply of trade credit by diminishing the market power required to sustain it (McMillan and Woodruff, 1999; Macchiavello and Morjaria, 2019), our results suggest that under competitive

[3]More broadly, recent theoretical and empirical contributions on the role of intermediaries in supply chains include Antràs and Costinot (2011), Bardhan et al. (2013), Chau et al. (2016), Maitra et al. (2017), and Emran et al. (2017).

pressure traders may expand credit provision to secure supply, and further that credit provision may coexist with a highly competitive market structure.

Finally, our estimation approach has broad relevance for policy analysis. An estimate of the differentiation rate allows one to study the impact of subsidy programs that treat a *subset* of firms in the market. Many industrial (agricultural) policies are implemented in this way. For example, while in West Africa governments have broadly preferred universal subsidies for fertilizer use, in East Africa subsidies are only available to poorer farmers (Druilhe and Barreiro-Hurle, 2012). Outside of agriculture, subsidies often target only small or medium-sized firms (Chatzouz et al., 2017; Rotemberg, forthcoming), exporters (Rodrik, 1993; Panagariya, 2000), or the politically connected (Khwaja and Mian, 2005; Faccio, 2006; Rijkers et al., 2015). Intuitively, as we demonstrate through counterfactual analysis, when subsidies are offered only to a subset of firms, the equilibrium effects of the subsidy depends both on the number of firms in the market, and the extent to which they are differentiated.

2 Experimental Design

Study setting. The basic question our experiment sought to answer was how responsive are traders to changes in their marginal revenue from purchasing cocoa of a particular quality, and how do these changes translate into price signals received by farmers. Cocoa is an important crop for Sierra Leone, where it is largest agricultural export by value, comprising 8.6% of exports in 2017, according to COMTRADE. As with many export products, a key policy concern is how to upgrade average quality. The transmission of a quality premium through prices is a necessary condition to do this in a decentralized manner.

Before describing the experiment, a brief schematic of the supply chain is warranted. Wholesalers are based in the three main towns of the cocoa producing region along the Moa river plain in Eastern Province. They source cocoa from a network of traders (intermediaries) with whom they typically have exclusive relations (i.e. a trader almost always delivers cocoa only to one wholesaler). Traders purchase cocoa from farmers in villages near to the towns and within a few days deliver to wholesalers, who sell onwards to exporters in the provincial capital of Kenema or the national capital Freetown. The structure of Sierra Leone's cocoa industry is similar to that of several other countries in West Africa, including Cameroon, Ivory Coast, and Nigeria.

Experimental design. We developed the experiment in partnership with five private wholesalers, and sought to influence the marginal revenue of the traders with which they worked. Ultimately, the experimental sample comprised 80 traders, henceforth *study traders*. As emphasized by Atkin and Donaldson (2015), when studying prices, it is important to focus on narrowly defined homogeneous goods. The quality of cocoa is indeed heterogeneous, and market prices depend on a variety of characteristics including moisture content, mold, germination, lack of fermentation and a discoloration known as slate. Though there is no official measure of quality in the market, wholesalers and traders agree on broad determinants of quality that are consistent with international standards (see Fold, 2005). A quality premium exists to some extent. To implement the experiment, we developed with wholesalers a quantitative quality grade that correlates well with baseline prices. When traders arrive at the warehouse, inspectors hired by the research team sampled 50 beans from each bag, and used them to create an index of quality—grades A, B or C—which was then applied to each bag. Formal grading was explained as a way to make the pricing, which was already based to some extent on inspection of quality, more rigorous. The analysis in this paper focuses on grade A cocoa, the grade targeted by the experimental subsidy, unless otherwise specified. Appendix A provides further details.

The experimental design is as follows. From mid-October to the end of December of 2011, roughly the end of the harvest season, a random subset of 40 traders received a bonus of 150 Leones per pound of cocoa sold —5.6% of the average wholesale price —when selling good quality (grade A) cocoa to the wholesalers. At the beginning of the experiment, traders were informed the bonus was because of increased demand for high quality cocoa, and would last until about the end of the harvest season. Randomization of the bonus treatment occurred at the trader level. We implement a pairwise randomization strategy (Bruhn and McKenzie, 2009): we match traders within wholesalers according to a self-reported estimate of the volume of purchases since the beginning of the cocoa season and then assigned treatment and control within pairs.[4]

Experimental design and SUTVA. In this setting, competitive forces may cause a violation of SUTVA. To see why, note that treatment and control traders operate in the same village. Control traders may respond strategically to behavior of treatment traders induced by the experiment. Importantly, SUTVA could also be violated if randomization was conducted at the *village* level, for instance if the experiment offered the subsidy to all traders in "treatment villages," while offering it to none in "control villages." Each trader on average operates in 4.6 villages, with significant multimarket contact between them, making it difficult to identify independent villages. Traders' mobility (on motorcycles) and the relatively small geographic span of the cocoa producing area

[4]Of the 84 traders identified by wholesalers, four were outliers with respect to baseline quantity relative to other traders (within the same wholesaler), and could not be matched to other traders in our randomization strategy. Thus, the final sample selected for randomization was 80 traders.

imply that, for a given trader, many markets are plausibly contestable. Figure 1 shows Sierra Leone's cocoa producing region, and the locations of the three towns Segbwema, Pendembu, and Kailahun where the five wholesalers operate, as well as the study villages from which study traders procure cocoa.

As a consequence, though the experiment is useful in creating variation in traders' profit function that is uncorrelated with other aspects of the traders' business, any interpretation of the experimental results must account for competitive interaction between treatment and control in the market. Treatment and control cannot be interpreted as treatment effects in the standard potential outcomes framework of Rubin (1974). The model in Section 4 is developed to allow a meaningful interpretation of the treatment and control difference, and to demonstrate its value for inference over the degree of competition in the market, and the analysis of subsidies that treat in particular a subset of firms. We note, for transparency, that we developed the model to account for this issue only once the experiment had been completed.

2.1 Data and Summary Statistics

Baseline data collection. Over the course of the experiment, we collected a variety of original data from traders. At baseline, we interviewed each trader about his experience in the industry, and collected basic demographic indicators. We also asked traders to list each farmer they buy from regularly and all of the villages in which they buy. Table 1, Panel A presents summary statistics and shows that treatment and control groups are balanced on these trader-level covariates. In the baseline listing, traders report purchasing cocoa from 123 villages. The average trader operates in 4.6 villages, and buys from 6 farmers per village. On average, based on the trader survey, there are 7.8 traders operating in a village. However, only 3.2 of these are study traders, suggesting that about 60% of the traders in the market are non-study traders (i.e. working with other wholesalers).

The provision of loans by traders to farmers is an important characteristic of this industry. Traders offer to purchase cocoa in advance before and during the harvesting season. Farmers use the advance payments for production (e.g., hiring workers for harvesting) or for consumption smoothing. Farmers then pay for these advances by selling at a below market price for subsequent sales. These contracts typically define the amount to be deducted from the final payment. Contracts are enforced through customary authorities (see, e.g., Acemoglu et al., 2014 and Sandefur and Siddiqi, 2013) or through relational contracts (see, e.g., Fafchamps, 2003, Blouin and Macchiavello (2019), and Macchiavello and Morjaria, 2015). Study traders report having given at least one loan to about 70% of the suppliers listed at baseline in the previous year.

Follow-up data collection. During the experiment, when traders arrived at the warehouse, inspectors from the research team measured quantity and quality of their shipment. [5] Enumerators then asked traders the price per pound they paid to farmers and the name of the village where the cocoa mostly originated. Traders typically mix cocoa from different farmers in the same bag, and so farmer prices reported are the average per unit purchase price paid by a trader for the cocoa in the bag. In addition to study the impact of trader treatment on advance payment provision, in November and December we asked again the traders if they had given loans in the previous month to the farmers listed at baseline.

Attrition. In the three weeks preceding the intervention, 56 of the 80 traders included in the study visited the warehouses (27 control and 29 treatment). Table 1 Panel B shows that treatment and control groups are balanced along volume purchased and prices paid to farmers. During the experiment, 74 traders visited the warehouse (36 controls and 38 treatment). We include in all regressions randomization pair fixed effects and thus we effectively drop pairs including those traders that did not visit the warehouses.

3 Experimental Results on Interlinked Transactions

We report the experimental treatment-control differences in trader prices (i.e. prices traders pay to farmers) and provision of advance payments during the intervention period. We then show that trader prices and advance payments covary negatively with one another, both in the cross-section across villages and in the treatment-control differences induced by the experiment, suggesting an indifference curve between price and advance payment within the interlinked transaction. The covariance of price and advance payments is used to quantify the slope of this indifference curve, which allows us to calculate the treatment-control difference in an *effective price*, akin to the net present value of the payment. The effective price captures both value transferred to the farmer through the price, and value transferred through the interlinked credit transaction.

3.1 Prices

First, we examine treatment and control differences in prices that traders pay to farmers. Enumerators asked traders the purchase price for each shipment. If the traders made payments at different times (e.g., before and after harvesting), enumerators recorded the total value traders paid

[5] Data collection ran from September 24th, 2011 to December 31st, 2011. The intervention started on October 15th, 2011. Data collection was suspended for approximately two weeks and half between late November and early December because of project budget constraints due to a higher volume of recorded transactions than we had initially budgeted.

for cocoa, not just the harvest one. We denote this variable with $\tilde{p}$, so to differentiate it from the *effective price*, p, we focus on later and in the model.

Figure 2 displays the price results graphically. It shows weekly averages for:[6] i) world prices (right y axis);[7] ii) wholesaler prices; iii) trader prices paid by treatment traders; iv) trader prices paid by control traders. The vertical red line marks the inception of the intervention period. The graph displays two key features. First, trader prices follow closely wholesaler prices and these move with world prices. In particular, domestic prices respond to the sharp decrease in the world price that occurred in November 2011. Second, this preliminary graphical analysis displays no obvious gap in prices that treatment and control traders pay to the farmers.

We estimate the following regression, where an observation is a shipment s delivered by trader i of randomization pair z in week t:

$$\tilde{p}_{sizt} = \eta_z + \eta_t + \pi^{\tilde{p}}\text{Treat}_i + \epsilon_{sit}, \tag{1}$$

where η_z and η_t are randomization pair and week fixed effects, respectively. We cluster standard errors at the unit of treatment, i.e. the trader. Results are similar when allowing for double clustering by trader and village (Cameron et al., 2012).

The term $\pi^{\tilde{p}}$ is the coefficient of interest. Table 2, Column (1) presents a regression without week fixed effects: the coefficient is $\hat{\pi}^{\tilde{p}} = -32.5$ (s.e.=47.2). In Column (2), we introduce week fixed effect and the coefficient becomes -5.5 (s.e. = 14.9). While the two coefficients are not statistically distinguishable from each other or from zero, that the coefficient is higher in absolute value without week effects suggests that selection in when to sell matters. In particular, it appears that the experiment induced treatment traders to stay longer in the market at the end of the season, when prices were lower. We delve into this issue in Section 5.3.

One additional concern is that the treatment may induce selection in which traders make purchases and in which locations traders visit. For this reason, in Columns (3)-(5), we add controls referring to the trader and to the village where the majority of cocoa in the shipment originated (see notes to Table 2 for a list of controls).[8] The coefficient of interest is quite stable when including

[6]In principle, dispersion in prices for transactions on identical cocoa would also be informative of trader differentiation. In practice, cross-sectional variance in prices is also likely to reflect measurement error and variation in transaction characteristics, such as date, exact location, transport costs, or specific product features. In our baseline price data, the coefficient of variation is 0.07.

[7]Specifically, we report the front month price on the Intercontinental Exchange for the physical delivery of 10MT of exchange-grade cocoa from a variety of African, Asian and Central and South American origins to any of five U.S. delivery ports, with trading code NYCC. We convert prices from USD/metric ton to Leones/pound using an exchange rate of 1 USD=4,400 Leones.

[8]Eighty of the 123 villages listed at baseline appear as "main village" in at least one shipment, covering approxi-

these controls. This suggests that the selection concerns described above cannot drive the results.[9]

Overall, the various specifications provide evidence that prices did not differ between treatment and control traders. Given concerns about SUTVA discussed above, this need not imply that treatment traders did not respond to the subsidy, or that they retained all of its value and did not raise prices paid to farmers. Rather, the result may reflect the response of both treatment and control traders to the subsidy. Suggestive evidence in line with this hypothesis comes from the fact that the treatment-control price gap is larger in the first weeks of the experiment (e.g., in the first three weeks of the experiment, the treatment coefficient is 31, p=0.09), and then decreases.

3.2 Advance Payments

Second, we examine treatment-control differences in the provision of advance payments during the intervention period, using the following regression:

$$AdvancePayment_{fiz} = \eta_z + \pi_1^a \text{Treat}_i + \nu_{fi} \tag{2}$$

An observation is a farmer listed as a regular supplier in the trader baseline survey.[10] $AdvancePayment_{fiz}$ is an indicator of whether farmer f received an advance from trader i of randomization pair z during the course of the experiment.

Table 3 presents estimates of π^a, the coefficient of interest. In Column (1), we run a linear probability model where the outcome is a dummy equal to one if credit was provided to a farmer. The difference between treatment and control traders is substantial: farmers reported as regular suppliers by treatment traders in the baseline listing are 14 percentage points more likely to receive credit from these traders during the 2.5 months of the experimental period, relative to a control mean of 11 percent. Columns (2)-(4) show that results are similar when adding trader controls, village controls, and both set of controls.

mately 85% of the suppliers listed at baseline.

[9] In results not presented, we also tested for effects on the prices of B and C grade cocoa. We find a statistically significant difference for grade B prices (the point estimate is 37, which is still very far from the value of the subsidy). Field interviews suggest that treatment traders were somewhat more willing to pay the grade A price for cocoa that had some probability of being grade A.

[10] The fact that we have data only on *regular* suppliers is a limitation of the data. Unfortunately, it is not clear in which direction this selection may bias our estimates of the advance payment treatment-control difference: traders may be less likely to extend advances to irregular suppliers or, on the contrary, they may be using advances particularly to attract irregular suppliers. We however observe that, in the price regressions (which include all farmers), we do not find heterogeneity by the number of regular suppliers in the village. This provides some reassurance that traders did not pass value to regular suppliers differently from how they passed value to other farmers.

3.3 Identifying and Valuing Interlinkages

We next investigate whether responses in prices and advanced payments are interlinked, in the sense that they are co-determined. To answer this question, we posit a simple linear indifference curve, which accounts for both the price paid and advance payments: the two may be combined into a summary *effective price* paid by buyer i given by

$$p_i = \tilde{p}_i + \lambda \cdot ShareAdvances_i, \tag{3}$$

where $\tilde{p}_i$ is the total price paid by the trader and $ShareAdvances_i$ is the share of farmers to whom trader i provides advance payments.[11] Here, λ is the extent to which farmers value advance payments (i.e., the rate of substitution of the indifference curve between prices and advances). If $\lambda = 0$, the transaction is not interlinked in the sense that the effective price is driven only by the price, and not affected by the transaction in the credit market. We will employ two strategies to estimate this parameter.

We observe that the indifference curve approach we adopt above is not informative about why traders choose to pass value to farmers through price or through credit. On this topic, one can only speculate, through in principle the decision should depend on the farmer's demand for credit (relative to price) and also the trader's liquidity. Qualitative discussions with treatment traders suggest that they aggressively used advances to secure supply from farmers shortly before harvest time and that additional revenues from subsidized purchases helped them offering advances.

Approach 1: Cross-Sectional Baseline Correlations

As a first pass, one may infer the value of advance payments from the baseline cross sectional relationship between shipment prices (i.e. the total monetary amount paid by the trader for a given shipment) and advance payments. Since we observe payment amounts at the village level but not at the transaction level, our focus is on village-level average prices and on the share of farmers receiving advance payments in the village.[12] Table 5, column (1) shows that moving from a village where no farmer receives advance payments at baseline to a village where each farmer receives advance

[11] Ideally, our data would include the specific amounts of payments made at different times. However, we only observe shipment-level prices and an indicator of whether a certain farmer receives advance payments.

[12] "Village-level" averages come from aggregating traders' baseline responses on prices, locations of activity, and number of suppliers. Here, we use villages as spatial unit to study the relationship between prices and advance payments. This is not inconsistent with our later discussion that villages may not be the relevant definition of market size. Our goal here is to estimate the slope of total payments with respect to advance payment provision. This requires partitioning farmers and using the partitions as data points. Villages are one of the many possible partitions, but a natural one to use (among other reasons, because we have covariates at the village level).

payments decreases the amount of total payments paid by the trader by 149.6 Leones (s.e. = 74.6). An interpretation of this result is that a farmer is indifferent between a trader paying a certain price and another trader who pays 149 Leones less but provides advance payments. The result is robust to the inclusion of village-level controls (column 2). These results, while based on limited baseline data, provide initial evidence consistent with the fact that farmers value advance payments and are willing to accept lower prices from traders for this service.

Approach 2: Heterogeneity in Treatment-Control Differences

Second, we infer the value of advance payments from the covariance of treatment-control differences in prices and treatment-control differences in advance payments. In principle, if traders respond to the treatment by increasing the *effective price* by a certain amount, the response can come either in the form of higher prices *or* more advance payments. The slope between the two response margins identifies their relative value, or how much less a trader who increases her advance payments needs to adjust her prices.

For this purpose, we modify Equation (2) to allow for heterogeneity in the treatment-control differences across villages and trader characteristics:

$$AdvancePayment_{fizv} = \eta_z + \pi^a(\text{Treat}_i) + (\text{Treat}_i \times X_v')\pi_v^a + X_v'\beta_v + (\text{Treat}_i \times X_i')\pi_i^a + X_i'\beta_i + \nu_{sipv}, \quad (4)$$

where X_v is the vector of village covariates and X_i is a vector of trader covariates. For any trader-village pair iv we then compute the predicted treatment-control difference in advance payment provision using heterogeneity by X_v and X_i: $\widehat{DTC}_{iv}^{a} = X_v'\pi_v^a + X_i'\pi_i^a + \pi^a$. Finally, we run the following specification to test whether village-trader pairs with larger treatment-control differences in advance payments display lower differences in prices:

$$\tilde{p}_{sizvt} = \eta_z + \eta_t + \pi^{\tilde{p}}(\text{Treat}_i) + \pi_a^{\tilde{p}}(\widehat{DTC}_{iv}^{a} \cdot \text{Treat}_i) + X_i'\beta_i + X_v'\beta_v + \epsilon_{kiptv}. \quad (5)$$

If total payments and advance payments are substitutes (i.e., $\tilde{\lambda} > 0$), then $\pi_a^{\tilde{p}} < 0$.[13]

Figure 4 provides some intuition for how this procedure works, and presents initial evidence that there is a negative slope between the treatment-control differences along the two margins. Here we estimate treatment-control differences in prices and advance payments in each of the chiefdoms included in the study, and plot them against each other. Chiefdoms are geographic units of local legal and political administration, and, as discussed in Acemoglu et al. (2014) vary in contract

[13]Since $\widehat{DTC}_{iv}^{a}$, is collinear with the vector of controls, its level is not included in the estimating equation.

enforcement and other institution (unfortunately, our data do not include explicit information on contract enforcement institutions). The scatter displays a negative relation: the regression line has a slope of -271. Table 6 presents estimates of $\pi_a^{\tilde{p}}$. In the different columns we show estimates generated using different sets of controls to predict $\widehat{DTC}_{iv}^a$. Since $\widehat{DTC}_{iv}^a$ is an estimated regressor, we follow Bertrand et al. (2004) and Cameron et al. (2008) and present p-values calculated using bootstrap-t procedure (Efron, 1981). We draw 2,000 bootstrap samples, clustering the bootstrapping by randomization pair.

Our estimates of $\pi_a^{\tilde{p}}$ are negative and statistically significant at 7 to 15 percent across the three specifications. In column (1), $\widehat{DTC}_{iv}^a$ is predicted using only chiefdom dummies. The estimate using these dummies predicts that a village where treatment traders are 14 percentage points more likely to provide advance payments than control traders —the mean coefficient in Table 3—would have a treatment-control difference in prices that is 47.8 Leones lower than a village with no difference in advance payments. This is economically relevant as it accounts for a reduction in the treatment difference of about one-third of the subsidy value. We find similar results in column (2), where the effect on advance payments is predicted using chiefdom dummies and village covariates, and in column (3), where we also add trader covariates. While the magnitude of the coefficients falls across columns, the core result holds: price and advance payment responses are substitutes.

Discussion

One may worry that villages with higher shares of advance provision (in either baseline level or treatment response) may have some other feature that lowers prices and thus that we are capturing a spurious relationship. While we cannot completely rule out this concern, three observations mitigate it. First, both approaches derive significant, qualitatively similar, and quantitatively meaningful results even if they use different variation. In particular, the baseline provision of advances (i.e., the source of variation used for approach 1) is not a major factor in the heterogeneity used in approach 2.[14] Second, we control for a range of village and traders covariates. Third, assuming that the loan covers the entire purchase and that the loan duration is two months (one month), the implied interest rate is around 3% (6%) per month. This is a high rate, but not inconsistent with prevailing interest rates. According to the World Development Indicators, the average lending rate for Sierra Leone over the last fifteen years was between 21% and 25% per year. In the inventory

[14]Estimates of λ derived in the second approach (210-334) are generally larger than the estimates from the cross-sectional analysis in Table 5 (147-150). However, the baseline advance payment variable, which we use in the first approach, captures whether the farmer had received advances in the twelve months before the baseline, a longer time horizon that the one of the experiment (two months). Thus, the two advance payment dummies may capture different intensities of advance payments.

credit evaluation described in Casaburi et al. (2014), rates on subsidized collateralized loans for agricultural smallholders were 22% per year. Rates on uncollateralized agricultural loans and on moneylender loans are likely to be substantially higher.

Treatment-Control Differences in *Effective Prices*

Having obtained estimates of λ, the value of advance payment, it is possible to estimate the treatment-control difference in the effective price, p: $\hat{\pi}^p \equiv \hat{\pi}^{\tilde{p}} + \hat{\lambda} \cdot \hat{\pi}^a$. Based on Tables 2 and 3, we specify $\hat{\pi}^{\tilde{p}} = -6.9$ and $\hat{\pi}^a = .14$. The values of λ are 150 when using the baseline correlations and 210 when using the treatment heterogeneity (we use the specification with all the interactions, as presented in Table 6, col. 3). The two approaches then lead to point estimates for the treatment-control difference in effective prices of 14.1 and 22.7, respectively. That is, once accounting for the value of advance payments, treatment traders pay an effective price higher than the control price by 10% to 15% of the subsidy value (150 Leones).[15]

4 Accounting for Strategic Interaction between Treatment and Control: A Simple Model

The goal of this section is to provide an interpretation of the treatment and control differences when unsubsidized traders strategically respond to subsidies to their competitors (i.e. the SUTVA fails). For this purpose, this section presents a simple model of oligopsonistic competition, in which a buyer differentiation parameter governs the response of market prices to a subsidy offered to a subset of buyers (akin to our experimental treatment). We derive closed form solutions for equilibrium prices paid by treatment and control buyers, and demonstrate how the experiment allows for estimation of the differentiation parameter.

In the model, differentiation among traders (which, to be clear, is distinct from product differentiation on the farmer side) is a reduced form for potential unobserved aspects of a specific buyer-seller relationship that may give the buyer market power. Specifically, the differentiation rate among traders measures the extent to which prices paid by competitors affect the quantity supplied to a trader, relative to the trader's own price sensitivity. Differentiation between traders may emerge

[15] Though noise in the estimation of λ unavoidably add uncertainty to the problem, it is possible to test whether treatment and control differences in effective price are statistically significant. Using Approach 1, based on baseline correlations, we compute a 90% confidence interval by jointly estimating $\hat{\pi}^{\tilde{p}}, \hat{\lambda}, \hat{\pi}^a$. We obtain [-10.93, 37.53] and we can reject the null that the difference in effective price is zero at p=0.23. Using Approach 2, based on treatment heterogeneity, we instead bootstrap since $\pi_a^{\tilde{p}}$ is itself an estimated regressor. We cluster resampling at the randomization pair level. The confidence interval is [-31.11, 61.33].

for range of reasons, for instance search costs, trust, or the relational contract required to sustain interlinkages. Here we do not provide for a full accounting for the determinants of differentiation. Rather, we aim at estimating the differentiation parameter using our experimental design. We then show how estimates of the parameter may vary when comparing treatment-control differences only in prices and in the effective prices defined above.

4.1 Setup

Producers

The economy is composed by V villages. In each village, there are measure one homogeneous producers, each producing a unit of output and there are n buyers who compete for these producers' output. The inverse supply buyer i faces in a village is:[16]

$$p_i = \alpha + \beta q_i + \gamma \sum_{j \neq i} q_j. \tag{6}$$

We adapt the standard model of linear demand and differentiated producers (see, e.g., Vives, 2001) to our setting, which features imperfect competition among buyers.The *differentiation rate* between traders is given by $\Gamma \equiv 1 - \frac{\gamma}{\beta}$. If $\Gamma = 0$, buyers are homogeneous: the slope of the inverse supply to own quantity equals the slope to a competitor's quantity. If $\Gamma = 1$, buyers are local monopsonists: a buyer's inverse supply does not depend on other buyers' quantities.

Buyers

The profit of buyer i in a village is given by

$$\pi_i = q_i(v_i - p_i), \tag{7}$$

where q_i is the quantity purchased, v_i is the (net) resale price, and p_i is the (effective) price the trader pays to producers.[17]

[16] This inverse supply can be microfounded by assuming a representative producer whose cost function features *love for variety*. Specifically, the producer profit function is: $V(p_1, ..., p_n, q_1, ...q_n) = q_0 + \sum_{i=1}^n p_i q_i - C(q_1, ..., q_n) = q_0 + \sum_{i=1}^n p_i q_i - (\alpha \sum_{i=1}^n q_i + \frac{1}{2}\beta \sum_{i=1}^n q_i^2 + \gamma \sum_{j \neq i} q_i q_j)$, where q_0 is the output that is not sold to traders (e.g., consumed, not harvested), p_i is the (effective) price paid by trader i and q_i is the output sold to trader i (the solution presented in this section assumes $q_0 > 0$). A representative agent strategy featuring love for variety may itself be considered a "reduced form" approach that aggregates heterogeneous producers having idiosyncratic preferences for each buyer.

[17] A given buyer can compete in multiple villages. However, the choices she makes across villages are assumed to be independent. Thus, we restrict the analysis to the village level and omit the village index in the equations above.

We assume buyers are ex-ante symmetric in the resale price v. The experiment introduces a subsidy, s, for a share μ of the buyers, who then have a higher resale price. Therefore, $v_i = v + s$ for treatment buyers and $v_i = v$ for control buyers. Below we refer to variables for treatment (control) buyers with subscript T (C).

We also assume *Cournot oligopsonistic competition*: each buyer sets quantities strategically, taking into account competitors' choices. In Section 4.4.1, we provide evidence in support of this choice and discuss alternatives such as Bertrand competition, monopsonistic competition, and trader collusion.

4.2 Equilibrium

We consider a "group-symmetric" equilibrium in which firms in the same treatment group behave similarly. Using standard algebra, we can derive the quantities set by treatment and control buyers:

$$\begin{aligned} q_T &= \frac{\alpha(\gamma-2\beta)+v(2\beta-\gamma)+s(2\beta+\gamma(-\mu n+n-1))}{(2\beta-\gamma)(2\beta+\gamma(n-1))}; \\ q_C &= \frac{(2\beta-\gamma)(v-\alpha)-\gamma\mu n s}{(2\beta-\gamma)(2\beta+\gamma(n-1))}. \end{aligned} \tag{8}$$

From the inverse supply functions in Equation 6, we then obtain equilibrium prices:

$$\begin{aligned} p_T &= \frac{\alpha\beta(2\beta-\gamma)+v(2\beta-\gamma)(\beta+\gamma(n-1))+\beta\gamma\mu n s+s(\beta-\gamma)(2\beta+\gamma(n-1))}{(2\beta-\gamma)(2\beta+\gamma(n-1))}; \\ p_C &= \frac{(2\beta-\gamma)(\beta(\alpha+v)+\gamma v(n-1))+\beta\gamma\mu n s}{(2\beta-\gamma)(2\beta+\gamma(n-1))}. \end{aligned} \tag{9}$$

It can be shown that prices imply variable markdowns, $\frac{p_i}{v_i}$. Treatment (control) quantities are increasing (decreasing) in the subsidy amount s and both are decreasing in the share of treated buyers μ. Both control and treatment prices are increasing in both s and μ. These intuitive comparative statics suggest that treatment changes the behavior of control firms, as well as treatment ones, violating SUTVA.

4.3 Recovering the Differentiation Parameter from the Experimental Results

The difference in equilibrium prices paid by treatment and control traders is

$$\Delta p \equiv p_T - p_C = \frac{s(\gamma-\beta)}{\gamma-2\beta} = \frac{s\Gamma}{1+\Gamma}. \tag{10}$$

Observe that Δp is *increasing* in Γ: if traders are homogeneous (i.e. $\Gamma = 0$), there can be only one price in the market. With higher differentiation, different prices can coexist: control traders can pay a price lower than do treated traders. Price differences do not depend on the share of treatment traders, μ. This is because an additional treatment trader takes away quantity from both control traders and other treatment traders, thus the impact on the difference between the two types of traders is ambiguous. In the case of the specific functional form we adopt, this impact is zero.

To recover Γ, we match Equation (10) to its empirical analog:

$$\Delta p \equiv \frac{s\Gamma}{1+\Gamma} = \hat{\pi}_1^p + \hat{\hat{\lambda}} \cdot \hat{\pi_1}^a \tag{11}$$

To demonstrate the potential effect of interlinkages on the result, we solve Equation (11) for Γ using the treatment-control differences in effective prices that we derived from the two methods in Section 3.3. We obtain the following results for point estimates and 90% confidence intervals: *i)* with $\lambda = 149$, Γ is 0.10 [-.10,.29]; *ii)* with $\lambda = 210$, Γ is 0.176 [-.18,.63]. When we ignore that λ is a generated regressor, we can reject $\Gamma = 0$ at p=0.13. If we ignored advance payments, the point estimate of Γ would be -0.03. The difference between these results demonstrates that, in the presence of interlinkages, looking at value passed through prices alone may affect inference over the degree of competition in the market. Taken together, these results suggest that traders appear fairly undifferentiated, pointing to a competitive market. We reach this conclusion even accounting for value passed through interlinked transactions.

In the spirit of an overidentification test, Appendix B also demonstrates that it is possible to achieve similar results using alternative moments from the model. Specifically, we derive theoretical expressions for the *percent* difference in prices between treatment and control traders and match them to their empirical counterparts (0.007 and 1.88, respectively). This approach yields estimates which are quite similar; for instance, when using $\lambda = 210$, we obtain $\Gamma = 0.181$. Appendix Figures B.1 and B.2 also show that, in the case of model misspecification, estimation using absolute or percent differences would yield considerably different estimates.

4.4 Discussion of Assumptions and Model Validation

There are two sets of concerns one may have regarding this conclusion. The first concerns the assumptions of the model itself, in particular the nature of competition assumed. The second concerns discrepancies between the idealized setting of the model and that of the actual market studied. The fact that we get similar results on the key parameters when using different moments from the model is reassuring. However, it is important to review these concerns and provide additional evidence that supports the use of our specific model in this setting.

4.4.1 Modeling Assumptions

Here, we review the key assumptions of the model.[18] First, we rely on specific functional forms. For instance, we focus on linear supply, rather than working with unrestricted supply elasticity.[19] Similarly, we assume that v and p do not depend on quantities, thus ruling out non-linear pricing and other non-constant trader marginal costs (see, e.g., Attanasio and Pastorino, forthcoming).

Second, agents are assumed to be symmetric, aside from the heterogeneity introduced by the experiment (i.e., the experimental subsidy, s). Appendix C.1 shows how the model can be extended to include heterogeneity in traders' marginal revenue and heterogeneous differentiation rate across different pairs of traders. Arbitrary heterogeneity across locations in market size and differentiation is harder to accommodate. However, in principle, one could estimate Γ separately in each location and then compute the average of the parameters across villages. In practice, for our specific experiment, estimating separate parameters in each location (using information on the main village of provenience of the cocoa in the shipment) delivers results that are too noisy to be useful.[20]

Third, we assume a specific form of competition, namely Cournot quantity setting. Other forms of competition do not appear consistent with the data. Replicating the procedure described above using Bertrand competition (while retaining other assumptions on producers and buyers) delivers unrealistic parameter values (a value of Γ larger than one). This suggests that quantity may be the relevant strategic choice variable in the setting. As it is well known, Cournot outcomes can also be interpreted as reduced-form outcomes for price competition with quantity constraints (Kreps and Scheinkman, 1983).Quantity constraints (arising, for instance, from transport technologies) may be relevant in this setting. Another candidate model of competition could be collusion: the fact that treatment-control differences are small for prices may be consistent with treatment and control buyers forming a cartel to take advantage of the subsidy. However, we note that collusion of this form would require not just an agreement between a treatment and a control trader to game the incentive system, but also collaboration among treatment traders (since otherwise a non-colluding treatment trader could steal the suppliers of the treatment-control pair cartel). The latter is a more demanding form of collusion and it faces the standard enforcement problems of a cartel. Collusion

[18] We observe that a general model of symmetric imperfect competition (e.g., Atkin and Donaldson, 2015) is not readily useful to interpret the response to taxes or subsidies given asymmetrically to firms within the same market.

[19] Among other reasons, the use of linear supply may be a concern because linear supply can be microfounded with a representative agent approach, but not with a discrete choice problem (see, e.g., Jaffe and Weyl, 2010 and Armstrong and Vickers, 2015).

[20] For instance, with few observations per village, the treatment-control difference in effective prices is often either negative or larger than 150 Leones (i.e., the subsidy value), which in both cases implies a negative value of Γ. However, it is reassuring that when we include village fixed effects in the regressions with pooled data, results are very similar to the ones presented in the text.

is also inconsistent with the large differential response of treatment traders in terms of advance payment provision. One might also posit a model of monopsonistic competition (adapted from the more common monopolistic competition case): Dixit and Stiglitz (1977) predicts a markdown on the subsidy equal to the markdown observed in the baseline data; Ottaviano et al. (2002) predicts a difference between treatment and control traders of one-half of the subsidy value. Neither of these predictions finds support in the data. For instance, trader prices are on average 92% of the wholesaler prices. Under constant markdown case, this would imply a difference in effective prices of at least 135 Leones between treatment and control traders.

Fourth, the assumption that farmers accept an *effective price* abstracts from dynamic features of the farmer-trader relationship that may support the interlinked transaction. Accounting for these elements would require a repeated game framework, featuring multiple choice variables for the traders, each dependent on market structure, which does not lend itself easily to closed-form solutions for the treatment-control differences, and also which may feature multiple equilibria, substantially complicating estimation. Therefore, we assume that traders face a separable problem. First, they set their effective price based on the inverse supply curve and competition they face. Second, for a given effective price, they choose the combination of payments to be made at different times. We do not model this second step. When making their sale choices, farmers consider only the effective price (the net present value of the payments), not its composition.

Finally, we assume that credit is the only additional margin through which traders can pass value. Another potential candidate is that traders provide price insurance to farmers. Additional analysis in Section 5.1 uses quasi-experimental variation to show that treatment traders do not seem to provide (differential) price insurance to farmers.

4.4.2 Discrepancies between the Theory Experiment and the Field Experiment

There are also discrepancies between the idealized setting in the model and the market studied, which may affect estimation of the trader differentiation rate.

First, we started the experiment in the middle of the harvest season. It is possible that, by that time, traders had already locked in purchases from some farmers with advance payments. Thus, the degree of differentiation may have been lower if we had started the experiment before the harvest season. Had the wholesalers announced the subsidy earlier, it is possible that treatment traders may have accessed an even larger pool of contestable farmers. In turn, our estimate of the differentiation rate, which was already quite low, may be an upper bound relative to that obtained in a season-long experiment.

Second, the experiment only ran until the end of the harvest season. Traders and farmers may have behaved differently in a multi-season trial. Again, it is plausible to assume that in a longer experiment the degree of differentiation would be even lower. For instance, in a longer experiment, farmers may have been more willing to switch to other buyers. Future research should assess whether varying the duration of the experiment leads to substantially different results. With this caveat in mind, we however believe that running the experiment until the end of the season was a reasonable length. The subsequent harvest season follows seven-eight months of inactivity and new trading relationships may potentially arise during that period. In addition, the high pass-through rate we described in Section 5.1 suggests that traders respond to high-frequency price changes, which are likely to be more transitory than our experimental season-long subsidy. The fact that the experiment lasted until the end of the season also suggests that traders had enough time to learn about the subsidy of their competitors, in line with the assumption of perfect information in the model. We also observe that the variation in prices induced by our experiment is less transitory than daily or weekly price variation used in many studies of pass-through.

Third, a final concern is the presence of non-study traders. These comprise about 60% of the traders operating in the study region. In principle, these could be different from the study traders (control and treatment) at baseline. The model presented in Section 4 is robust to the presence of such traders. Appendix C.2 presents an extension of the model where only a share σ of traders is included in the study, and thus study treatments are a share $\sigma\mu$ of traders. Non-study traders have a resale price, v', that possibly differs from the study traders' one, v. The equilibrium treatment-control price difference Δp (Equation 10) is unchanged.

5 Additional Results on Competition

Our estimate of the differentiation rate is suggestive of a market that is fairly competitive, even when accounting for value transferred through interlinkages. In this section we provide three additional pieces of evidence that support this conclusion.

5.1 Pass-Through of Industry-Wide Price Shocks

First, we examine how trader prices respond to common changes in wholesaler prices (v in the model), which are driven by changes in the world price. Figure 2, discussed in Section 3.1, showed a stark reduction in prices paid to farmers (around 22%) in the final month of the experiment, following a reduction in wholesaler prices, and a decline in the world price. Table 7 quantifies pass-through using an OLS regression, where standard errors are clustered by date (we obtain similar results when clustering by week). Column (1) reports results of a regression of the trader price on the wholesaler price and trader fixed effects. The coefficient estimate is 0.89, implying a high level of price pass-through from traders to farmers.

Of course, the change in wholesaler prices observed here may be correlated with local supply shocks. To address this concern, we instrument wholesaler prices with the international price of cocoa, as reported by the *Intercontinental Exchange*. Given that Sierra Leone has a small share of the global production, it is plausible that changes in international prices are exogenous to supply conditions in Sierra Leone. The instrument leads a very strong first stage (Kleibergen-Paap F-stat=14,024). Column (2) in Table 7 shows that the pass-through rate estimate is 0.92. In Column (3), we also add month fixed effects. The coefficient is stable. Finally, Column (4) shows that the coefficient is robust to including village fixed effects (though the first stage becomes substantially weaker). We also obtain similar results when using a lag of international prices (of a day or a week) in our regression. Overall, these results suggest a high level of pass-through of changes in the wholesaler price to prices received by farmers. Our results are consistent with the findings of Gayi and Tsowou (2015), who show that cocoa farmer prices in several West African countries have been very responsive to world prices in the last two decades, with a pass-through of around 90%.

One caveat to these results is that, given the results on interlinked credit above, this estimate of the pass-through rate may be under-estimated in terms of an effective price. Unfortunately, given the relatively low frequency of international price movements, measuring pass-through in these terms would require collecting data on credit transactions over a much longer period of time. Therefore, the estimated pass-through rate, which is already quite high, is likely to be a lower bound of the pass-through one would measure when also including credit adjustment.

Finally, additional analysis shows that the pass-through of industry-wide price shocks does not vary between treatment and control (p-value=0.43). This suggests that treatment traders do not provide (additional) price insurance to farmers relative to control ones. This finding supports our approach in Section 3.3, where we calculated the effective price using only total value and credit provision.

5.2 Effective Number of Traders in the Market

In our model, *for a given number of traders in the market*, competitiveness is summarized by the differentiation rate. Under Cournot competition of course the number of traders in the market also moderates competition. As discussed previously, a challenge in our setting is that, given ambiguous market boundaries, the number of traders competing in the market is difficult to observe.

It is possible however to use additional information from the data to estimate the number of traders in the market, rather than take it as given. Specifically, the model delivers a solution for how buyer prices respond to a market-level shock in the resale price, v, common for all traders, or the pass-through rate, which we estimated above. In the model, this is given by

$$\rho \equiv \frac{\partial p_C}{\partial v} = \frac{\partial p_T}{\partial v} = 1 - \frac{1}{1 + \Gamma + n(1 - \Gamma)}, \tag{12}$$

which is decreasing in Γ and increasing in n. Equation 12 shows that, for given Γ, the pass-through rate allows us to recover the number of traders in the market, n (in practice, Γ and n are estimated jointly, not sequentially).

In the average village, we observed 7.8 traders operating, already perhaps a large number. Using our estimate of $\hat{\rho} = 0.92$, and solving for n using the two values of the treatment-control differences in effective prices yields: *i)* with $\lambda = 149$, n, is 12.7 [7.9,17.1]; *ii)* with $\lambda = 149$, n is 13.75 [90% C.I. is 7.6,20.1]. These estimates of n imply that, according to the model, traders behave as if the number of their competitors were sixty to eighty percent more than observed in the average village. This result confirms our intuition that village markets operate as if they are highly contestable: the option to sell to other traders shapes competition, not the actual number of traders actually purchasing from each farmer. Anecdotally, we find that sales outside the village may be indirect. For instance, a farmer may give her product to a local aggregator who then makes sales outside the village.

Consistent with the idea that villages do not necessarily match the relevant market size, we do not detect statistically significant impacts of the number of treated traders in the village on the treatment-control differences in prices, advance payments, and quantities. Alternative specifications that use the level, the inverse hyperbolic sine transformation, or dummies for the number of treated traders and of study traders give similar results. However, as a caveat, we note that these estimates suffer from low power.

5.3 Treatment and Control Differences in Quantities

In a competitive market, even a very small increase in price offered by one trader should allow the trader to increase their market share substantially. We can test this prediction directly by examining treatment-control difference in quantities purchased during the experiment. Figure 3 shows the weekly amount purchased by the study traders and then by treatment and control groups separately. Several patterns emerge. First, purchases of treatment and control are balanced in the three weeks before the intervention. Second, throughout the intervention, treatment traders purchase substantially higher volumes than control ones. Third, total quantity purchased by study-traders increases after the beginning of the experiment. This observation is consistent with the idea that treatment traders may have gained market shares at the expense of non-study traders, as well as of control traders.[21] Finally, toward the end of the experiment, there is a stark reduction in total quantities purchased, consistent with field reports that the season was essentially over by that time.

Table 4 presents these results more rigorously using the following regression models:

$$Quantity_{izt} = \eta_t + \eta_z + \pi^q \text{Treat}_i + \zeta_{it} \tag{13}$$

where an observation captures the total purchases of cocoa trader i of randomization pair z in week t (including zeros). During the experiment, treatment traders on average purchase 527 pounds per week more than control traders, roughly a 188% difference.[22] The results are robust when including trader controls in Column (2). Overall, this is a large impact of the treatment. Given that farmers had limited opportunities to increase production by the time the intervention started, it seems likely that market-stealing effects may drive the results. Treatment traders could steal from both control traders (20% of the market) and non-study traders (60% of the market).

It is possible to use the model's estimated parameters (Γ, n) to assess how much of this increase comes from market stealing vs. increases in aggregate supply. A priori, we expect the majority of the increase to have come from market stealing, given that the experiment was implemented at harvest time and farmers had limited options to increase their supply in response to the price changes (e.g. reducing processing losses). In the model, the *direct* supply function (which again depends on Γ and n), provides a mapping from the aggregate price impacts of the experiment—effects on both treatment and control prices, accounting for strategic interaction—to the aggregate impact on quantity. We derive this impact in two steps, which we sketch here, while Appendix D provides details.

First, we calculate the effect of the subsidy on (effective) prices paid by both treatment and control traders, relative to a countefactual without the experiment. The derivative of the prices of treatment and control traders are pinned down by Γ, n, and μ (the share of treatment traders in the market). In what follows, we use results based on treatment-control differences in the effective price,

[21] The large differences in quantity purchased may also be consistent with treatment and control buyers forming a cartel to take advantage of the subsidy. The discussion in Section 4.4.1 presented a number of findings against this interpretation.

[22] Consistent with the large difference in quantities purchases, treatment traders were more than three times as likely to visit the warehouse during the experimental period than control ones. Throughout the experiment, we did not receive any complaint from either wholesalers or traders suggesting that control traders were switching to different wholesalers. This is consistent with the fact that the experiment did not change the wholesaler price for control traders.

assuming the value $\lambda = 210$, based off our experimental results. Figure 5 shows, for the estimated values of the competition parameters (i.e., $\Gamma = .176, n = 13.75$), the increase in the treatment and control prices in response to a unit subsidy, relative to the scenario without the experiment, as a function of the share of treated traders, $\mu \in (0, 1)$.

At $\mu = 0.2$—the share of treated traders in the market once accounting for the non-study traders—the derivatives imply that treatment (control) traders raise their effective prices by 0.30 (0.15) per unit of subsidy and thus by 46 (23) Leones overall in response to the experimental subsidy of 150 Leones.

Second, we apply these increases in prices to the supply function to estimate an upper bound on the *aggregate* quantity of 0.9%.[23] We can compare this result to the increase in aggregate quantity that would occur if all of the quantity results came from increases in aggregate supply (as opposed to market stealing), 38%. This suggests indeed that the difference in quantities between treatment and control traders during the experiment arises almost entirely from market stealing.

Combining these results, the percent increase in farmer revenues is pinned down by the percent changes in prices and quantities for treatment and control, weighted by their market shares. Using again the lower (upper) bound on changes in aggregate supply, we find that farmer revenues increase by 1.1% (1.2%), more than aggregate quantity.

6 Counterfactual Experiments Treating Different Shares of Differentiated Traders

Estimates of the differentiation rate allow for the analysis of the impacts of subsidies that target *subsets* of traders in the market, a feature of many industrial policies. This has broad relevance for policy analysis. Many industrial (agricultural) policies are implemented in this way. For example, while in West Africa governments have broadly preferred universal subsidies for fertilizer use, in East Africa subsidies are only available to poorer farmers (Druilhe and Barreiro-Hurle, 2012). Outside of agriculture, subsidies often target only small or medium sized firms (Chatzouz et al., 2017; Rotemberg, forthcoming), exporters (Rodrik, 1993; Panagariya, 2000), or the politically connected (Khwaja and Mian, 2005; Faccio, 2006; Rijkers et al., 2015). When subsidies are offered only to a subset of firms, they have direct effects through changes in the prices paid by treatment traders, and indirect effects through the strategic response of control traders. The equilibrium effects thus

[23] The lower bound is, trivially, zero. We can also assess the impact of the experiment on quantities purchased by treatment and control traders separately. Using the upper (lower) bound on the increase in aggregate quantity, we find that treatment traders increased their purchases by 111% (109%), relative to the counterfactual without the experiment, and control traders decreased their purchases by 27% (27.5%)

depend both on the number of firms in the market and on the extent to which they are differentiated. Here, we discuss how the model can be used to simulate the equilibrium impact of this transfers, for different *shares* of treated traders. In particular, we are interested in comparing different subsidy interventions along their "return on investment:" the ratio of benefits in terms of incremental farmer revenues, and their cost (the total subsidy value).

The continuous blue curve in Figure 6 shows values of this return implied by our estimates $\Gamma = .18, n = 13.75$, under the assumption of no impact of the experiment on aggregate quantity (as discussed above, the upper bound for the increase in quantity was 0.9%). There are three remarks. First, once shutting down the aggregate quantity impact, the return is obviously always less than one: the subsidy value is passed only imperfectly to farmers. Second, the return is increasing in the share of treated traders: the additional benefits in terms of farmer revenues when increasing μ are larger than the extra costs. Third, the return on investment is quite flat in the share of treated traders μ. In particular, under the estimated market structure parameters, the return when subsidizing 20% of the traders is about two-thirds of the return when targeting all the traders.

These counterfactuals highlight the importance of estimating the two parameters governing competition in our model separately. For a given level of the pass-through rate, different pairs (Γ, n) lead to different returns on investment when the share of treated traders is less than one. For instance, in Figure 6, the dashed orange curve captures the return for $\Gamma = 0.75$ and $n = 43$. The continuous and dashed curve take, by construction, equal value for a subsidy that targets all traders (i.e., $\mu = 1$). However, the dashed line is above the continuous line for interventions that only target a subset of traders. In particular, at $\mu = 0.2$, the return is about fifteen percentage points higher than before.

These counterfactual results may be useful to inform subsidy policies. For instance, a government may wish to transfer income directly to farmers, but to do so may be costly, given the absence of financial infrastructure. If the cost of physically going out to transfer cash to farmers is more than the loss incurred by passing the subsidy through traders on to farmers, the government may find that paying a subsidy to traders is a (second-best) efficient way to transfer income to farmers. A helpful benchmark is the unconditional cash transfer (UCT) program of *Give Directly*, studied by Haushofer and Shapiro (2016). This program, which relies on mobile money technology to disburse payments, achieves a ratio of recipient benefits to costs of 94.7% in Kenya and 93.2% in Uganda.[24] These programs have a higher ratio of recipient benefits to total costs than trader subsidies for any

[24]These numbers are based on costs reported for 2014 by *Give Directly*, and calculated as ROI = 1 – (Cost of identifying recipients, transferring money, and following up)/(Total Cost – Overhead), to be comparable to our ROI measure, which includes only direct costs of the subsidy, and not overhead.

level of μ. However, in Sierra Leone, only 8% of the rural population over 15 years has a mobile money account (Demirgüç-Kunt et al., 2017), and so such returns may not be obtainable.

7 Concluding Remarks

Interlinked transactions in which output prices are determined jointly with the terms of a credit contract are an important feature of many business relationships, particularly in developing economies. The presence of interlinkages must be accounted for in the analysis of competition and market structure. In an experimental design in the Sierra Leone cocoa value chain, where we provide subsidies to a random subset of traders purchasing output from farmers, we show that traders may pass value to farmers either through output prices or the provision of credit and that these two margins covary negatively.

A standard model of imperfect competition demonstrates the implications of this result for the degree of market competition. The analysis accounts explicitly for the violation of SUTVA that is implied when an experiment takes place in a market setting. In this case, the treatment-control price difference informs an intuitive test of the degree of differentiation across firms: only if firms are differentiated can there be systematic price differences between subsidized and non-subsidized firms. Our results suggest that low differentiation and a large number of effective buyers may exist in the presence of an interlinked credit market.

While our findings are suggestive of high competition at the farm gate, downstream segments in the value chain (wholesalers, exporters) may be substantially less competitive. In Sierra Leone, where exportation is mainly private, Figure 2 showed that, though wholesaler prices respond somewhat to changes in the international price, pass-through much lower than when looking at prices paid by traders to farmers. Identifying whether this low price pass-through may be explained by weak competition among wholesalers or exporters an important area for future research.

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Figures

Figure 1: Map of study villages

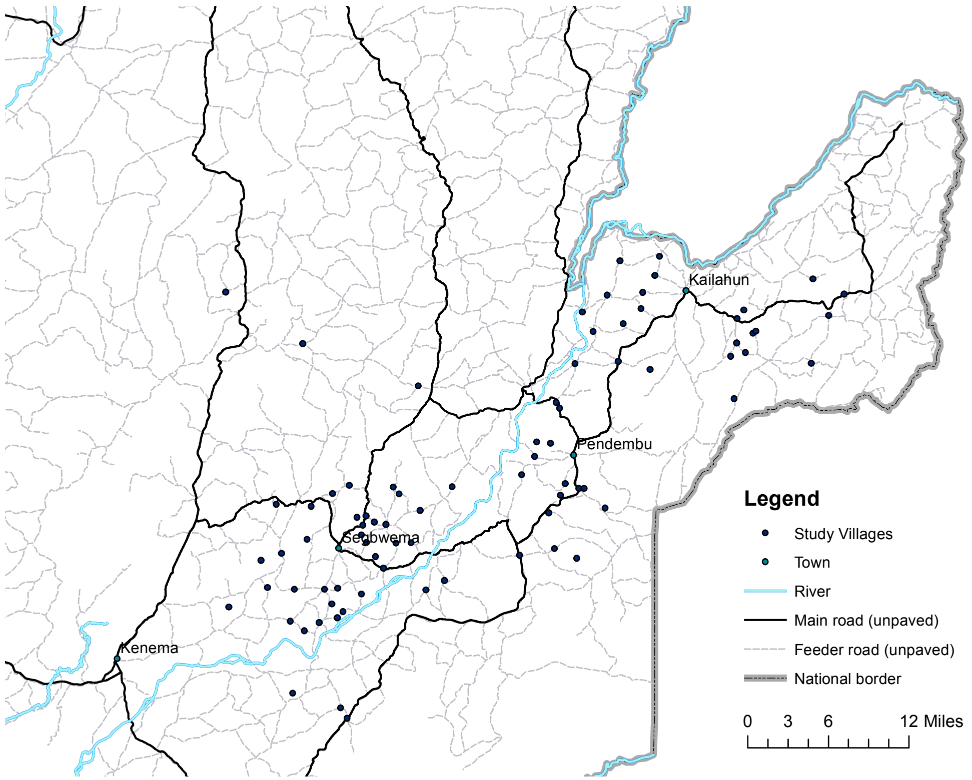
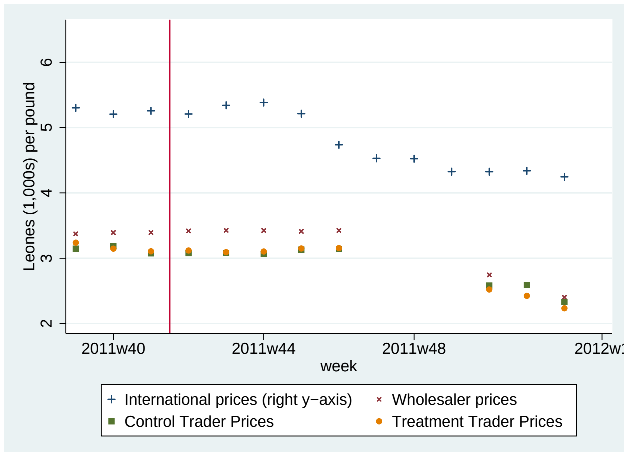
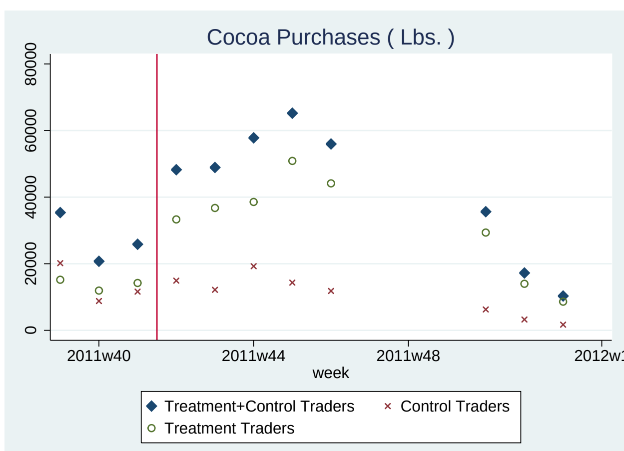


Figure 2: Cocoa Prices



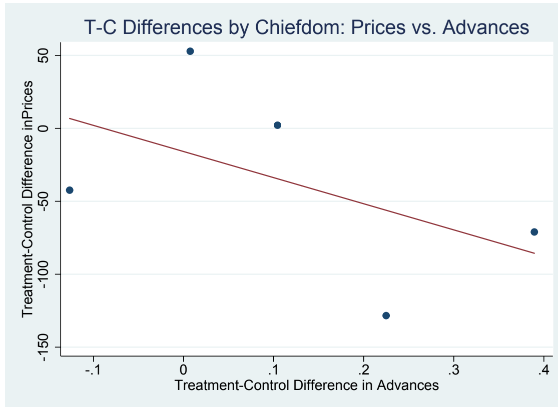
*N*otes: The figure presents average weekly prices for: *i)* international cocoa prices; *ii)* prices the study wholesalers pay to the traders; *iii)* prices control traders pay to farmers; *iv)* prices treatment traders pay to farmers. All prices are in Leones (1,000s) per pound. Wholesaler and trader prices data collection was suspended for most of three weeks (w47-w49). The vertical line marks the beginning of the intervention period.

Figure 3: Purchases of Cocoa



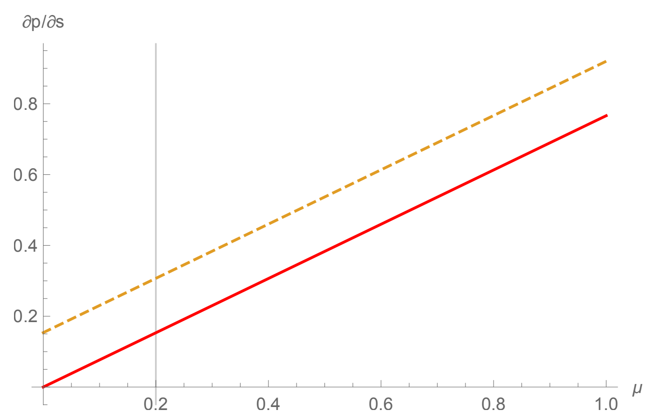
*N*otes: The figure shows the total amount of cocoa purchases by study traders (i.e., control and treatment traders), control traders, and treatment traders. The vertical line marks the beginning of the intervention period. Data collection was suspended for most of three weeks (w47-w49).

Figure 4: Estimating λ: Treatment-Control Differences by Chiefdom, Prices vs. Advance Payments



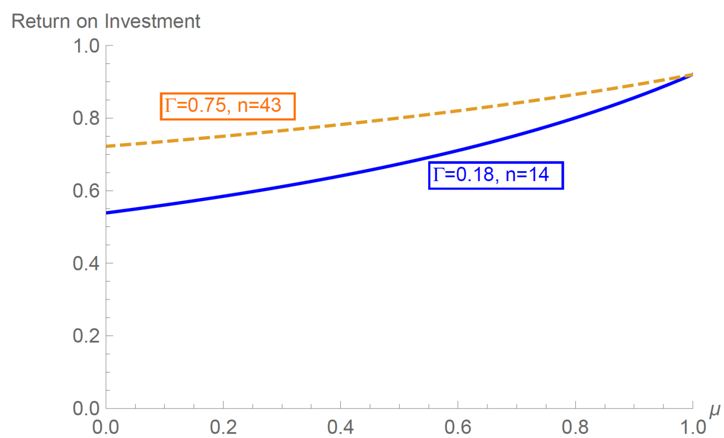
*N*otes: The scatter reports the correlation across price and advance payments treatment-control differences, estimated separately across the five chiefdoms included in the study. The regression line has a slope of -271.

Figure 5: Counterfactual Experiments: Trader Prices



*N*otes: The graph shows the impact of counterfactual experiments on (effective) prices paid by control traders (continuous line) and treatment traders (dashed line). Specifically, it reports the increase in prices in response to a unit-subsidy as a function of the share of treated traders, μ. The vertical line reports the share of traders treated in our experiment, $\mu = 0.2$. At this value of the treatment share, control (treatment) traders increase their prices by 0.15 (0.30) per unit of subsidy, respectively, relatively to a scenario without the experiment. For $\mu \to 1$, the response of treatment traders tends to the pass-through rate, 0.92.

Figure 6: Counterfactual Experiments: Return on Investment



Notes: The graph reports the return to investment (y-axis) for experiments that target a share μ (x-axis) of traders with a unitary per-unit subsidy. The return to investment is defined as the ratio between the additional farmer revenues generated by the intervention and the cost of the subsidy. We conduct the simulations assuming no response in aggregate supply. Section 6 describes the procedure to recover these values. The continuous curve describes the results using the market structure parameters estimated in the paper ($\Gamma = .18, n = 14$). The dashed curve describes the results using an alternative pair ($\Gamma = .75, n = 43$) that gives the same pass-through rate, $\rho = .92$, than the pair of values estimated in the paper.

Tables

Table 1: Baseline Trader Summary Statistics

Covariate	Treatment	Control	Treatment - Control
Panel A: Baseline Interview			
Self-estimate bags sold in 2011	20.0	18.6	1.5
	(28.3)	(18.5)	(2.23)
Age, years	38.2	36.9	1.4
	(8.2)	(10.2)	(1.91)
Years trading cocoa	8.1	8.9	-0.8
	(5.4)	(5.5)	(1.2)
Years selling to study wholesaler	5.7	7.3	-1.6
	(4.8)	(4.9)	(0.86)*
Cement or tile floor in house $\in \{0,1\}$	0.53	0.63	-0.1
	(0.51)	(0.49)	(0.1)
Mobile phone owner $\in \{0,1\}$	0.90	0.93	-0.03
	(0.30)	(0.27)	(0.06)
Access to storage facility $\in \{0,1\}$	0.88	0.78	0.10
	(0.33)	(0.42)	(0.09)
Villages operating in	4.25	4.87	-0.62
	(1.64)	(2.02)	(0.39)
Number of suppliers per village	5.8	6.2	-0.35
	(3.3)	(3.6)	(0.84)
Share of suppliers given credit since March	0.72	0.68	0.04
	(0.32)	(0.28)	(0.05)
Panel B: Pre-treatment shipment data			
Price Paid to Farmer (shipment-level)	3,137	3,136	1.2
	(154)	(151)	(41.9)
Pounds sold during pre-treatment (weekly)	345	339	6.2
	(694)	(762)	(96.5)

Notes: Panel A presents balancing for the variables defined in the baseline survey. Some baseline survey variables are missing for one trader. The column "Treatment-Control" presents results from a regression on treatment and randomization pairs. Panel B presents balancing for variables from pre-experiment shipment data. Prices are defined only for the subset of traders that delivers at least one shipment during this period (56 traders). Quantities are defined for all traders and are equal to zero for traders who don not make any delivery in the pre-experimental period. Standard errors are clustered by trader. ***p<0.01, **p<0.05, *p<0.1.

Table 2: Treatment-Control Differences in Prices

	(1)	(2)	(3)	(4)	(5)
Bonus $\in \{0,1\}$	-32.52	-5.47	-5.92	-12.87	-6.86
	(47.16)	(14.95)	(16.99)	(13.21)	(15.41)
Control Group Mean	2987	2987	2987	2987	2987
Week FE		X	X	X	X
Trader Controls			X		X
Village Controls				X	X
Observations	1079	1079	1060	1079	1060

Notes: The table reports the difference between the prices paid by treatment and control traders to farmers during the experiment, measured in Leone per pound. The subsidy to treatment traders was Le. 150. per pound. An observation is a shipment delivered by the trader to a wholesaler. Trader controls are baseline values of pounds of cocoa sold, number of villages operating in, number of suppliers buying from, share of clients given credit in baseline, age, years of working with wholesaler, and dummies for ownership of a cement or tile floor, mobile phone and access to a storage facility. Village controls are baseline share of suppliers begin given credit, number of other bonus traders and number of study traders, miles to nearest town, and number of clients across all traders. Data on some trader controls are missing for one trader and thus the number of observations falls in Columns (3) and (5). Standard errors are clustered at the level of the trader. ***p<0.01, **p<0.05, *p<0.1.

Table 3: Treatment-Control Differences in Advance Payments

	(1)	(2)	(3)	(4)
Treatment Trader	0.14***	0.14***	0.13***	0.14***
	(0.03)	(0.02)	(0.03)	(0.02)
Control Group Mean	0.11	0.11	0.11	0.11
Trader Controls		X		X
Village Controls			X	X
Observations	1837	1825	1837	1825

Notes: The table reports the difference between treatment and control in the share of regular suppliers that receive advance payments (binary indicator) during the experimental period. An observation is a farmer a trader listed as regular supplier in the baseline survey. Trader controls are baseline values of pounds of cocoa sold, number of villages operating in, number of suppliers buying from, share of clients given credit in baseline, age, years of working with wholesaler, and dummies for ownership of a cement or tile floor, mobile phone and access to a storage facility. Village controls are baseline share of suppliers begin given credit, number of other bonus traders and number of study traders, miles to nearest town, and number of clients across all traders. Data on some trader controls are missing for one trader and thus the number of observations falls in Columns (2) and (4). Standard errors are clustered at the level of the trader. ***p<0.01, **p<0.05, *p<0.1.

Table 4: Treatment-Control Differences in Quantities

	(1)	(2)
Treatment Trader	537.12***	527.72***
	(54.0)	(54.2)
Control Group Mean	282.5	282.5
Trader Controls		X
Observations	640	632

Notes: The table reports the difference between the quantities of cocoa purchased by treatment and control traders during the experimental period. An observation is a week*trader (8*80). Trader controls are baseline values of pounds of cocoa sold, number of villages operating in, number of suppliers buying from, share of clients given credit in baseline, age, years of working with wholesaler, and dummies for ownership of a cement or tile floor, mobile phone and access to a storage facility. Data on some trader controls are missing for one trader and thus the number of observations falls in Column (2). Standard errors are clustered at the level of the trader. ***p<0.01, **p<0.05, *p<0.1.

Table 5: The Value of Advance Payments: Baseline Correlations

	(1)	(2)
Share of Farmers Receiving Advance Payments	-149.65*	-147.19*
	(74.66)	(75.47)
Dependent Variable Mean	3138	3138
Village Controls		X
Observations	43	43

Notes: The table presents correlation between baseline value of the average village cocoa price and the share of farmers receiving advance payments in the village. The sample includes 44 villages for which we have baseline cocoa shipment data. Village controls include: number of traders in the village, distance from the wholesaler warehouse, and number of farmers in the village. Standard errors allow for heteroskedasticity. ***p<0.01, **p<0.05, *p<0.1.

Table 6: The Value of Advance Payments: Heterogeneity in Treatment-Control Differences

	(1)	(2)	(3)
Treat* Estimated Treament Effect on Credit	-341.79	-300.13	-209.87
p-values from boostrapped t-stats	[.10]	[.15]	[.07]
Chiefdoms	X	X	X
Village Controls		X	X
Trader Controls			X
Observations	1060	1060	1060

Notes: The dependent variable is the price paid by the trader for the shipment of cocoa. Each column presents estimates of $\pi_a^{\tilde{p}}$ from equation 5. P-values in brackets are derived from pairs cluster bootstrap-t at the randomization pair level using 1,000 replications. Trader controls are baseline values of pounds of cocoa sold, number of villages operating in, number of suppliers buying from, share of clients given credit in baseline, age, years of working with wholesaler, and dummies for ownership of a cement or tile floor, mobile phone and access to a storage facility. Village controls are baseline share of suppliers begin given credit, number of other bonus traders and number of study traders, miles to nearest town, and number of clients across all traders.

Table 7: Pass-Through from Wholesaler to Trader Prices

	OLS	IV		
	(1)	(2)	(3)	(4)
Wholesaler Price	0.89***	0.92***	0.93***	0.90***
	(0.02)	(0.01)	(0.01)	(0.09)
Control Group Mean	3007	3007	3007	3007
Kleibergen-Paap First Stage F-stat		1408.2	471.6	5.5
Trader FE	X	X	X	X
Month FE			X	X
Village FE				X
Observations	1254	1254	1254	1254

Notes: The table reports the pass-through from wholesaler prices (i.e. paid to traders) to trader prices (i.e., paid to farmers). Both are measured in Leones per pound. An observation is a shipment delivered by the trader to a wholesaler before or during the intervention. In Columns (2)-(4), wholesaler prices are instrumented with the front-month prices for liquid cocoa futures, obtained by the *Intercontinental Exchange*. Standard errors are clustered by day. ***p<0.01, **p<0.05, *p<0.1.

Appendix

A Measuring Cocoa Quality

Both international and local cocoa prices vary with quality. Factors contributing to poor quality cocoa are high moisture content, mold, germination, a lack of fermentation and slate, a discoloration signaling poor flavor. There is wide agreement on these standards internationally. For a discussion, refer to CAOBISCO/ECA/FCC (2015) and, for the specific case of West Africa, David (2005). Other dimensions of quality affecting price on the international market are various fair-trade and environmental certifications. Such certification generally requires that beans can be verifiably traced to individual producers. In our market, there is not yet the infrastructure to do such tracing, and so this quality dimension does not apply.

In our grading system, inspectors from our research team with local language skills stayed in the warehouses of wholesalers and tested a sample of 50 beans from each bag of cocoa as it arrived. Moisture was measured using Dickey John MiniGAC moisture meters, two of which were generously donated by the manufacturer. Other defects were spotted by eye, after cracking beans open with a knife. Grade A beans have no more than average 11.5% moisture, no more than 2% mold (1 bean of 50), and no less than 72% beans with no defect (36 beans of 50). Grade B beans have no more than 22% moisture, 4% mold (2 beans of 50) and no less than 52% good beans (27 beans of 50). Grade C applies to any bean failing to be grade A or B.

B Alternative Estimation Moments

This Appendix presents details about the alternative approach to recover Γ and n. Our goal is to identify alternative moments and to compare the results we obtain from these moments to the ones of the main approach presented in the paper. Showing that different moments deliver similar estimates would provide support for the specific model we use.

B.1 Methodology

In the paper, we showed how they two parameters could be identified, relying on two moments: the *level* difference in treatment and control prices (Equation 10) and the pass-through rate of changes in wholesaler prices (Equation 12). In this section, we show how the key parameters Γ and n, and also the intercept parameter α, can be recovered from the *percent* differences between treatment and control in prices *and quantities*, combined again with the pass-through rate.

First, we derive theoretical expressions for the percent differences between treatment and control in prices and quantities:

$$\%\Delta p \equiv \frac{p^T - p^C}{p_C} = \frac{s\Gamma(1 + (1-\Gamma)(n-1))}{(1-\Gamma)\mu ns + (1+\Gamma)((1-\Gamma)(n-1)v + (\alpha + v))} \tag{B.1}$$

and

$$\%\Delta q \equiv \frac{q_T - q_C}{q_C} = \frac{s(-2 - (1-\Gamma)(n-1))}{(1-\Gamma)\mu ns - (1+\Gamma)(v - \alpha)} \tag{B.2}$$

For a given value of the subsidy s, these expressions depend on additional parameters, i.e., μ, v, α, as well as on those we aim to recover, i.e., Γ and n. We calibrate the value of μ and v. We set the former at $1/5$, the share of treatment traders out of the total number of traders (study and non-study). Assigning a value to the latter requires some additional assumption. The (average) value of the wholesaler price (i.e. the price at which traders resell), is Le. 3,260. The average price at which traders purchase is Le. 2,987, 91% of the wholesaler price. However, in the model, v is the *net* resale price, net of other costs the traders may incur and that we do not observe, such as transport and storage costs. We set $v = 3,145$, which implies a 5% markdown.[25]

B.2 Results

Having assigned values to μ and v, we have a system of three equations—Equations B.1, and B.2 defined above and the pass-through formula (Equation 12)—, in three unknowns, Γ, n, and α. We note that the intercept term α is identified only up to the currency unit choice.

During the experiment, control traders pay an average price of 2,987. The average likelihood of advance provision for control traders is 0.11. Therefore, given $\lambda = 209$, the average control effective price is 3,010. This implies that the percent price difference between treatment and control traders during the experiment is 0.7%. The average quantity purchased by control traders is 282.5 kilograms. Thus the percent different between treatment and control traders is 188%.

Solving the equation system with these values for $\%\Delta p$ and $\%\Delta q$, we obtain the following estimates for the three parameters of interests: $\Gamma = 0.181$, $n = 13.8$, and $\alpha = 2,015$. The results for Γ and n are thus very close to the ones obtained when using the more parsimonious methodology described in the main text. We see this as evidence in support of the specific competition model chosen for the analysis.

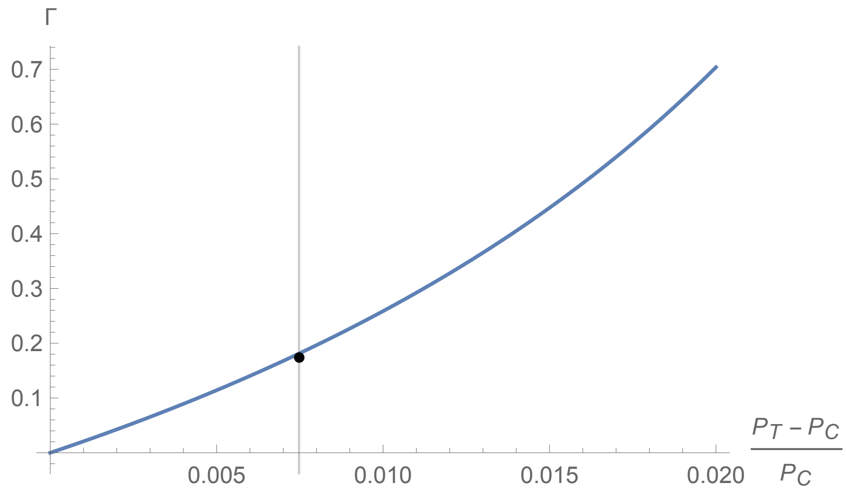
Finally, we emphasize that the similarity of the results between the two approaches is not a mechanical result since one uses the *level* of the difference between treatment and control prices, while the other uses the *percent* differences between treatment and control in both prices and quantities. Figure B.1 and B.2 confirm this point: the

[25] Results are quite stable when using other values of v, spanning between 2,987 (the average trader price) and 3,260 (the average wholesaler price).

two graphs show, respectively, how the estimated values of Γ and n would vary with different values of the percent treatment-control difference in prices, $\frac{p_T - p_C}{p_C}$, in a neighborhood of the real value, 0.007 (represented by the vertical gray line). In each graph, the large dot reports the estimate from the main estimation presented in the text. The key point is that, while the estimates derived when using the real value $\frac{p_T - p_C}{p_C}$ are close to those in the main text, they would be quite different when using arbitrary values of $\frac{p_T - p_C}{p_C}$ (i.e. if the treatment-control difference in the level of prices were equivalent to a different value of the difference in percent terms.).

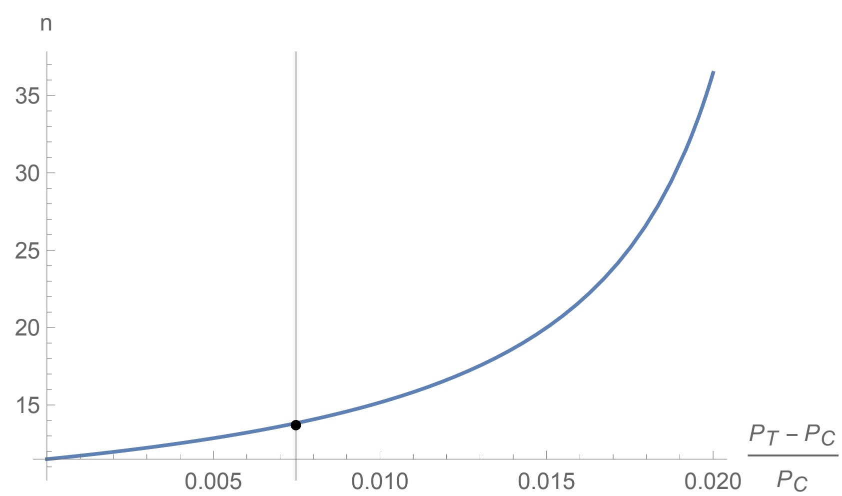
B.3 Figures

Figure B.1: Sensitivity of Γ to $\frac{p_T - p_C}{p_C}$



*N*otes: The graph reports sensitivity of the estimate of Γ obtained from the method described in Appendix B to the value of the percent treatment-control price difference. The dot represent the estimate from the main method presented in Section 4.3.

Figure B.2: Sensitivity of n to $\frac{p_T - p_C}{p_C}$



*N*otes: The graph reports sensitivity of the estimate of Γ obtained from the method described in Appendix B to the value of the percent treatment-control price difference. The dot represent the estimate from the main method presented in Section 5.2.

C Model Extensions

C.1 Trader Heterogeneity

The baseline model presented in Section 4 assumes that traders are symmetric at baseline and that the experimental subsidy is the only source of heterogeneity. The key results of the model, and thus the empirical strategy to recover the competition parameters, are robust to extensions that account for different forms of heterogeneity.

First, we allow baseline differences across traders in their resale prices.[26] For simplicity, we consider a case with two types of traders. Absent the experiment, a share σ of traders has resale price v, and a share $1-\sigma$ has resale price $v' = v + w$. With the experiment, a share μ of traders in each group receives a per unit subsidy s. In equilibrium, firms with higher resale prices purchase larger quantities and pay higher prices (unless $\Gamma = 0$). By randomization, treatment is uncorrelated with firm characteristics. This orthogonality is the key benefit of randomization even if, as we discuss in the paper, the SUTVA is violated.

Within each group of traders (v and v'), the difference in equilibrium prices between treatment (subsidized) and control (unsubsidized) firms is $\Delta p = \frac{s\Gamma}{1+\Gamma}$. Therefore, trivially, this is the value for the expected price difference: $E[\Delta p] \equiv E[p_T - p_C] = \frac{s\Gamma}{1+\Gamma}$. Similarly, it can be shown that $E[\Delta q] \equiv E[q_T - q_C] = \frac{s}{\beta(1+\Gamma)}$. Finally, the linear inverse supply implies constant pass-through: For each type of firm, $\rho \equiv \frac{\partial p}{\partial v} = 1 - \frac{1}{1+\Gamma+n(1-\Gamma)}$, and thus $E[\rho]$ takes the same value. Therefore, the key moments presented in Equations (10) and (12) are unchanged.

Second, we allow for multiple differentiation rates across traders. We consider again a simple case with two groups of competitors. In a symmetric environment with n traders, each trader has $\frac{n}{2} - 1$ "close" competitors, with substitution rate γ, and $\frac{n}{2}$ "far" competitors with substitution rate $\kappa\gamma, 0 < \kappa < 1$. Therefore, the inverse supply for each trader i is $p_i = \alpha + \beta q_i + \gamma(\sum_{j\in C} p_j + \kappa \sum_{j \in F} p_j)$, where C and F represent close and far competitors, respectively.

It can be shown that the equilibrium differences between treatment and control are unchanged: $\Delta p = \frac{s\Gamma}{1+\Gamma}$ (where Γ is still $1 - \frac{\gamma}{\beta}$ and $\Delta q = \frac{s}{\beta(1+\Gamma)}$. In addition, the pass-through rate is $\rho = 1 - \frac{1}{1+\Gamma+\tilde{n}(1-\Gamma)}$, where $\tilde{n} \equiv \frac{n}{2}(1+\kappa)$ can be again defined as the "effective market size", the number of competitors weighted by their (relative) substitution parameter κ. In this case, the estimation procedure presented in the paper therefore recovers Γ and $\tilde{n}$.[27]

C.2 Non-study Traders

As discussed above, the model presented in Section 4 features symmetric traders. From this pool of identical traders, a share μ receives the experimental subsidy. In our field experiment setting, about 60% of the traders are not included in the study (and we do not collect data on them). These traders may be fundamentally different than the ones we include in the study. We present an extension of the model that accounts for this issue.

There is a share σ of study traders (S) and a share $1-\sigma$ of non-study traders (NS). We allow the two types of farmers to vary in their resale prices: $v_S = v$ and $v_{NS} = v + w, w \neq 0$. Inverse supply for trader i is again $p_i = \alpha + \beta q_i + \gamma \sum_{j \neq i} q_j$.[28] A share μ of the study traders, and thus a share $\mu\sigma$ of all traders, receives the subsidy.

Our experimental estimates only compare prices of the study traders. The main object of interest is $p_{ST} - p_{SC}$, where the subscript S refers to the share σ of study traders. The moments derived in Section 4 are robust to the presence of non-study traders. It can be shown that $\Delta p_S \equiv p_{ST} - p_{SC} = \frac{s\Gamma}{1+\Gamma}$. This is the same value we obtained in the baseline model, where we assumed that all traders were part of the experiment (Equation 10). A similar result is obtained for Δq_S. Finally, the pass-through rate is also unchanged (again, this is due to the common pass-through functional form).

[26] This is equivalent to varying producer costs in an oligopoly model.

[27] The result extends to the general case of $m = 1, .., M$ groups of traders, with differentiation $\Gamma_m = \kappa^{m-1}\Gamma$. In this case, Δp, Δq, and ρ are as above and $\tilde{n} = \frac{n}{M}\frac{1-\kappa^M}{1-\kappa}$.

[28] That is, we assume a common degree of differentiation across study and non-study traders.

D The Impact of the Experiment on Prices, Quantities, and Farmer Revenues

This Appendix provides details of the steps to assess the impact of the experiment on prices, quantities, and farmer revenues (Section 6).

Setup

We use the superscript 0 to refer to the pre-intervention period and 1 to refer to the intervention period. At baseline, traders are homogeneous and pay p^0.[29] Each trader thus faces the direct supply $q_i^0 = a + bp_i^0 - c\sum_{i\neq j} p_j^0$.[30] Symmetry implies $q^0 = a + (b - c(n-1))p^0$. Aggregate supply is thus $Q^0 = nq^0 = n[a + (b - c(n-1))p^0]$. Throughout this section, we assume that non-study traders are equal to control ones.

Impact on Prices

To assess the impact of the experiment on the prices of control and treatment traders, we first compute the derivative of equilibrium prices with respect to the subsidy: $\frac{\partial p_T}{\partial s} = \frac{\Gamma - \frac{(\Gamma-1)\mu n}{\Gamma - \Gamma n + n + 1}}{\Gamma+1}$, $\frac{\partial p_C}{\partial s} = \frac{(1-\Gamma)\mu n}{(\Gamma+1)(1+\Gamma+n(1-\Gamma))}$.

The impact of the experiment on prices is then given by $dp_g = \frac{\partial p_g}{\partial s}s$, for $g = \{T, C\}$. Given our estimates of Γ and n, we can compute $dp_T = p_T^1 - p^0 = 46$ and $dp_C = p_C^1 - p^0 = 23$.[31] Using a baseline price of 2,964 (mean of the effective price for control traders during the experiment minus dp_C), we obtain $p_T^1/p^0 = 1.015$ and $p_C^1/p^0 = 1.008$. The experimental subsidy, which was worth about 5% of the baseline price, increased treatment (control) prices by around 1.5% (0.8%).

Impact on Quantities

Given dp_T and dp_C, we can write: $q_T^1 = a + b(p^0 + dp_T) - c\Big((\mu n - 1)(p^0 + dp_T) + (1-\mu)n(p^0 + dp_C)\Big)$. With some algebra, we obtain $q_T^1 = q^0 + \underbrace{\Big(bdp_T - c\big((\mu n - 1)dp_T + (1-\mu)ndp_C\big)\Big)}_{dq_T}$. Similarly, for the control group, $q_C^1 = q^0 + \underbrace{\Big(bdp_C - c(\mu ndp_T + ((1-\mu)n - 1)dp_C)\Big)}_{dq_C}$. The aggregate quantity during the experimental period, Q^1, is then $Q^1 = n\big(\mu q_T^1 + (1-\mu)q_C^1\big) = n\big(\mu(q^0 + dq_T) + (1-\mu)(q^0 + dq_C)\big) = Q_0 + \underbrace{n\big(\mu dq_T + (1-\mu)dq_C\big)}_{dQ^1}$. The increase in aggregate quantity induced by the experiment can the be written as: $dQ^1 \equiv Q^1 - Q^0 = n\big(b - c(n-1)\big)\big(\mu dp_T + (1-\mu)dp_C\big)$. In turn, the *percent* impact is: $\frac{dQ^1}{Q^0} = \frac{n}{n}\frac{\big(b-c(n-1)\big)\big(\mu dp_T + (1-\mu)dp_C\big)}{a + (b - c(n-1))p^0} = \frac{\mu dp_T + (1-\mu)dp_C}{\frac{a}{b-c(n-1)} + p^0}$. Since we do not estimate β and γ separately, but only their ratio, we cannot quantify $\frac{dQ^1}{Q^0}$. However, assuming $a \geq 0$ (which holds in our estimates) and noticing that $b - c(n-1) > 0$ (since $\beta > \gamma$), then $\frac{\mu dp_T + (1-\mu)dp_C}{p^0}$ is an *upper bound* on $\frac{dQ^1}{Q^0}$.

From the percent impact on aggregate quantity, we can now compute the impact for treatment and control quantities. We do this in a four steps

1. Aggregate quantity is $Q^1 = n\Big(\mu q_T^1 + (1-\mu)q_C^1\Big) = nq_0(1 + \frac{dQ^1}{Q^0})$

[29] Thus, throughout the exercise, we assume non-study traders and study traders are homogeneous before the experiment.

[30] The direct supply function is $q_i = a + bp_i - c\sum_{j\neq i} p_j$, with $a \equiv \frac{\alpha}{\beta + \gamma(n-1)}$, $b \equiv \frac{\beta + \gamma(n-2)}{(\beta + \gamma(n-1))(\beta - \gamma)}$, $c \equiv \frac{\gamma}{(\beta + \gamma(n-1))(\beta - \gamma)}$.

[31] This is, by construction, consistent with our estimate of the difference in (effective) prices between treatment and control traders.

2. We define the ratio of treatment to control quantities during the experimental period: $R \equiv q_T^1/q_C^1$. Then,

$$Q^1 = n\Big(\mu R q_C^1 + (1-\mu) q_C^1\Big)$$

3. We can now solve for q_C^1 (relative to q^0):

$$\frac{q_C^1}{q^0} = \frac{1 + \frac{dQ^1}{Q^0}}{\mu R + (1-\mu)}$$

We note that we can measure the ratio R in the data

4. Finally, we can easily derive $\frac{q_T^1}{q^0} = R\frac{q_C^1}{q^0} = R\frac{1+\frac{dQ^1}{Q^0}}{\mu R+(1-\mu)}$

In our experiment, $\mu = .2, 1-\mu = .8, dp_T = 46, dp_C = 23$. $p^0 = 2,964$. Therefore we compute the upper bound on the percent change in aggregate quantity as

$$\frac{\widehat{dQ^1}}{Q^0} = \frac{27}{\frac{a}{b-c(n-1)} + 2987} \leq \frac{27.6}{2987} = 0.009$$

In response to the experiment, aggregate quantity raises by *at most* 0.9%. Also, we have a trivial lower bound, which is zero (i.e., the aggregate quantity is constant). As a benchmark, we can quantify the increase in aggregate quantity we would observe without any market stealing (i.e., the control quantities were unchanged). In this case aggregate quantity would go up by $\mu * 188\% + (1-\mu) * 0$, that is by 38%.[32] This suggests that most of the difference between treatment and control in quantity purchases comes indeed from market stealing. Finally, given the upper bound of 0.009, we can compute that, relative to a world without experiment, controls reduce their purchases by 27% and treatment increase their purchaes by 111%. At the lower bound of 0, controls reduce their purchases by 27.5% and treatment traders increase their purchaes by 109%.

Impact on Farmer Revenues

In the pre-experiment period, farmer revenues are simply $r^0 = p^0 Q^0 = p^0 n q^0$. In the experimental period, these become $r_1 = n\Big(\mu p_T^1 q_T^1 + (1-\mu) p_C^1 q_C^1\Big)$. Therefore, the ratio between these two values is a function of the quantities we derived above:

$$\frac{r_1}{r_0} = \frac{\mu p_T^1 q_C^1 + (1-\mu) p_C^1 q_C^1)}{p^0 n q^0}$$
$$= \mu \frac{p_T^1}{p^0}\frac{q_T^1}{q^0} + (1-\mu)\frac{p_C^1}{p^0}\frac{q_C^1}{q^0}$$

In our experiment, taking the upper bound $dQ^1/Q^0 = 0.009$, we obtain:

$$\frac{r_1}{r_0} = \mu \frac{p_T^1}{p^0}\frac{q_T^1}{q^0} + (1-\mu)\frac{p_C^1}{p^0}\frac{q_C^1}{q^0} = .2 * 1.015 * 2.11 + .8 * 1.007 * .73 = 1.02$$

At the lower bound of no change in aggregate quantity, $\frac{r_1}{r_0} = 1.01$.

Returns on Investment and Counterfactual Experiments

Finally, we consider the return on investment (ROI) on experiments that treat a share μ of traders. We focus on a social planner whose welfare is linear in farmer revenues (and does not depend on trader revenues). Therefore, the ROI is the ratio between the increase in farmer revenues and the cost of the program. The former is $r^1 - r^0 =$

[32] 188% is the percent difference in treatment and control quantities during the experiment.

$\left(\underbrace{\left(\mu \frac{p_T^1}{p^0} \frac{q_T^1}{q^0} + (1-\mu) \frac{p_C^1}{p^0} \frac{q_C^1}{q^0}\right)}_{r^1/r^0} - 1\right) \underbrace{np^0 q^0}_{r^0}$. The cost of the intervention is $C = \mu n s q_T^1$. The derivations in the paper consider the case in which the experiments do not induce an increase in aggregate quantity. We focus first on variations in μ given our estimates of (Γ, n) and then consider returns for alternative values of these parameters. Section 6 presents the results.