Online Appendix

"Using individual-level randomized treatment to learn about market structure"

Lorenzo Casaburi **Tristan Reed** *

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.

*lorenzo.casaburi@econ.uzh.ch, treed@worldbank.org.

B Computing Effective Prices

B.1 Defining the Effective Price

A farmer can sell cocoa at price p_1 at harvest time or at $p_0 = \lambda p_1$ in advance, where λ depends on the market interest rate, r: $\lambda = \frac{1}{1+r} < 1$. The farmer will prefer the advance if $\frac{\lambda}{\delta} p_1 > p_1$, where δ is the farmer's discount factor between the two periods. Intuitively, if the discount factor is low (i.e. the farmer is impatient) or the penalty for getting early payment is low (i.e. λ is high), then the farmer prefers the advance.

A trader who pays a share s of the cocoa in advance, will thus pay the average price:

$$\tilde{p} = sp_0 + (1-s)p_1 = p_1(s\lambda + (1-s)) = p_1(1 - s(1-\lambda)).$$

We then denote with p the *effective price* paid by each trader, *as valued by the farmer* (at harvest time):

$$p = s\frac{p_0}{\delta} + (1-s)p_1 = p_1(s\frac{\lambda}{\delta} + (1-s)) = p_1(1 - s(1-\frac{\lambda}{\delta})) = \frac{\tilde{p}}{1-s(1-\lambda)}(1-s(1-\frac{\lambda}{\delta}))$$

.

The effective price p shapes farmers' supply and it is our object of interest.

B.2 Computing the Effective Price

In the data, we observe the average price paid by traders in each treatment group, $\tilde{p}$. We proxy s with the share of farmers who receives credit. We compute the effective price (by treatment group) following several approaches. In Section IV.A.1, we described our baseline approach based on cross-sectional variation in prices and credit. Here, we present details on our alternative strategies.

Treatment Heterogeneity in prices and credit. We infer the value of advance payments from the covariance of treatment-control differences in prices and treatment-control differences in advance payments. Similarly to our baseline approach, we assume again that $\lambda = \delta$. The slope between the price and credit responses thus identifies their relative value, or how much less a trader who increases his advance payments needs to adjust his prices.

For this purpose, we modify Equation (11) to allow for heterogeneity in the treatment-control differences across villages and trader characteristics:

$$AdvancePayment_{fizv} = \eta_z + \pi^a(\text{Treat}_i) + (\text{Treat}_i \times X_v')\pi_v^a + X_v'\beta_v + (\text{Treat}_i \times X_i')\pi_i^a + X_i'\beta_i + \nu_{sipv}, \tag{B.1}$$

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: $\widehat{DTC}^a_{iv} = X_v'\pi^a_v + X_i'\pi^a_i + \pi^a$. 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:

$$\tilde{p}_{sizvt} = \eta_z + \eta_t + \pi^{\tilde{p}}(\text{Treat}_i) + \pi^{\tilde{p}}_a(\widehat{DTC}^a_{iv} \cdot \text{Treat}_i) + X_i'\beta_i + X_v'\beta_v + \epsilon_{kiptv}. \tag{B.2}$$

If total payments and advance payments are substitutes (i.e., $\lambda > 0$), then $\pi^{\tilde{p}}_a < 0$.[1]

Appendix Figure B.1 provides 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 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 -191.

Appendix Table B.2 presents estimates of $\pi^{\tilde{p}}_a$. In the different columns we show estimates generated using different sets of controls to predict $\widehat{DTC}^a_{iv}$. Since $\widehat{DTC}^a_{iv}$ is an estimated regressor, we follow Bertrand et al. (2004) and Cameron et al. (2008) and present p-values calculated using the bootstrap-t procedure (Efron, 1981). We draw 2,000 bootstrap samples, clustering the bootstrapping by randomization pair. Our estimates of $\pi^{\tilde{p}}_a$ are negative and statistically significant at 8 to 15 percent across the three specifications. In column (1) of Table B.2, $\widehat{DTC}^a_{iv}$ is predicted using only chiefdom dummies. The estimate using these dummies predicts that a village where treatment traders are 12 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 40 Leones lower than a village with no difference in advance payments. This is economically meaningful as it accounts for a reduction in the treatment difference of about one-quarter of the subsidy value. In Appendix Table B.2, 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. According to column (3), when treatment traders provide credit but control traders do not, shipment prices would fall by 222 Leones, from a baseline of 3,138 Leones. Thus, $\lambda = 0.93$.

Interest Rate Calibration 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 credit evaluation described by Casaburi et al. (2014), rates on subsidized collateralized loans for agricultural smallholders were 22%. One would expect rates on unsecured agricultural loans to be higher (IFAD, 2018). As a lower bound, we consider a rate of 2% per month. We assume a loan

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

duration of one month, which is reasonable because we focus only on credit provided during the intervention (2.5 months) and most transactions happen in the first half of the intervention. Thus, $\lambda = 0.98$ and the difference in effective prices between treatment and control is 0.01.

Experimental Elicitation of Farmers' Subjective Rates of Returns. We conducted a lab-in-the-field experiment with cocoa farmers. Similarly to the original field experiment, the study took place in the harvest season (in November). It targeted three villages included in the original experiment. In each village, we listed approximately thirty cocoa farmers. Farmers were then asked to choose between a payment in three days and one in 24 or 45 days, i.e. three weeks or six weeks after the first payment).[2] We follow a *Multiple Price List* design (Andersen et al., 2008). For each time interval, farmers made 15 binary choices between the earlier and the later payment, with the latter increasing across choices. The earlier amount was always Leones 300,000 (approximately USD 30). This is a large amount, worth about 7% of the median self-report cocoa revenues from the entire harvest season. Future amounts spanned between Leones 300,000 and 700,000.

We then use the switching points in each set of of choices to compute a farmer's monthly *Required Rate of Return, RRR*. As highlighted in Cohen et al. (2020), the *RRR* is not necessarily a time-preference parameter because a number of other factors may affect its determination (e.g. risk aversion and liquidity constraints). Rather, it is simply an indifference point in the experimental data (we define the indifference point as the mid point between the highest future amount to which the subject prefers the (fixed) present amount and the lowest future amount the subject prefers to the present amount). Nevertheless, the *RRR* is still the relevant object to understand how much farmers may value traders' advance payments. Pooling our data, we compute a median monthly *RRR* of 0.0935, which implies $\delta = 0.914$.

Scaling Credit by the Share of Grade-A Transactions. In the final sensitivity test, we focus on the observation that some of the advance payments may concern non-grade A transactions (as per our discussion in Section IV.A.2). First, we first scale down the village-level credit share by the share of grade-A transactions in that village. Moving from a village where no farmer receives advance payments at baseline to a village where each farmer receives advance payments decreases shipment prices paid by the trader by approximately 254 Leones from an average of 3,138 , i.e. $\lambda = 0.92$. Intuitively, this value of λ is lower than when we use the unadjusted credit share. Second, we multiply the share of advance payments by the share of grade-A transactions in each treatment group, thus obtaining $\sigma_C = 0.15 * 0.45 = 0.068, \sigma_T = 0.27 * 0.63 = 0.17$. This gives a difference in effective prices between treatment and control traders equal to 19.6 Leones [-11,54.7].

B.3 Figures

[2] To avoid differences in transaction costs and perceived risk, we add a front-end delay in the earlier. This design also advance provision, as traders often return with the cash few days after agreeing on the advance transaction. We also elicited time-preferences between 24 and 45 days.

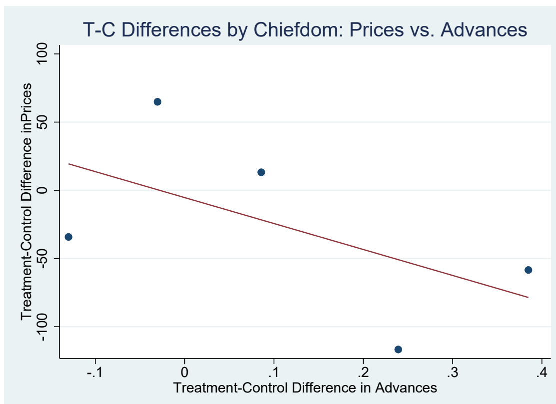


Figure B.1: Estimating λ: Treatment-Control Differences by Chiefdom, Prices vs. Advance Payments

*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 -199.

B.4 Tables

Table B.1: The Value of Advance Payments: Baseline Correlations

	(1)	(2)
Share of Farmers Receiving Advance Payments	-151.23*	-149.60*
	(77.04)	(76.81)
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: baseline share of suppliers receiving credit, 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 B.2: The Value of Advance Payments: Heterogeneity in Treatment-Control Differences

	(1)	(2)	(3)
Treat* Estimated Treament Effect on Credit	-327	-274	-222
p-values from boostrapped t-stats	[.08]	[.15]	[.14]
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 $\pi_a^{\bar{p}}$ from equation B.2. P-values in brackets are derived from pairs cluster bootstrap-t at the randomization pair level using 2,000 replications. Trader controls are baseline values of pounds of cocoa sold, number of villages operating in, number of suppliers buying from, share of suppliers receiving credit from the trader at 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 number of other bonus traders and number of study traders, miles to nearest town, and number of clients across all traders.

C 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.

C.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 5) and the pass-through rate of changes in wholesaler prices (Equation 9). 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:

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

and

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

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 effective price at which traders purchase cocoa from farmers is Le. 3,010, 92% 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.[3]

C.2 Results

Having assigned values to μ and v, we have a system of three equations—Equations C.1, and C.2 defined above and the pass-through formula (Equation 9)—, 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 effective price of 3,010 Leones and, in our preferred specification with $\lambda = 0.95$, treatment traders pay 3,022.8. This implies that the percent price difference between treatment and control traders during the experiment is 0.4%. The average quantity purchased by control traders is 114.6 kilograms and the treatment-control difference is 395 kg. Thus, the percent difference between treatment and control traders is 344%.

Solving the equation system with these values for $\%\Delta p$ and $\%\Delta q$, we obtain the following estimates for the three parameters of interests: $\Gamma = 0.096$, $n = 12.12$, and $\alpha = 2,368$. 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.

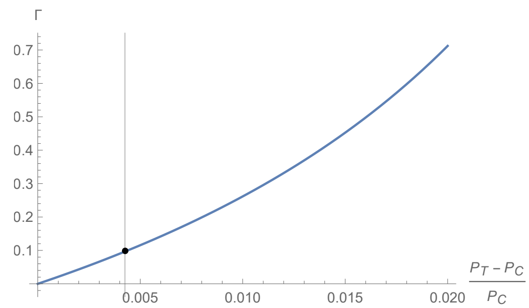
Finally, we emphasize that the similarity of the results between the two approaches is not a mechanical result. This is because one set of results uses the *level* of the difference between treatment and control prices, while the other uses the *percent* differences between treatment and control in both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

[3] Results are quite stable when using other values of v, spanning between 3,010 (the average effective price paid to the farmer) and 3,260 (the average wholesaler price).

this point: the 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.).

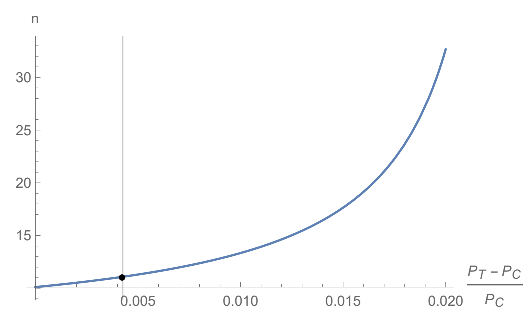
C.3 Figures

Figure C.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 C to the value of the percent treatment-control price difference. The dot represent the estimate from the main method presented in Section IV.

Figure C.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 C to the value of the percent treatment-control price difference. The dot represent the estimate from the main method presented in Section IV.B.

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

This Appendix provides details on the steps to assess the impact of the experiment on prices and quantities with respect to a counterfactual without the experiment (Section IV.C.2).

Setup

We use the superscript 0 to refer to the scenario without the intervention and superscript 1 for the intervention. Without the experiment, traders are homogeneous and pay p^0.[4] Each trader thus faces the direct supply $q_i^0 = a + bp_i^0 - c\sum_{i\neq j} p_j^0$.[5] 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 the (rounded) $dp_T = p_T^1 - p^0 = 37.75$ and $dp_C = p_C^1 - p^0 = 24.95$.[6] Using a baseline price of $p^0 = 2,985$ (mean of the effective price for control traders during the experiment minus dp_C), we obtain $p_T^1/p^0 = 1.0126$ and $p_C^1/p^0 = 1.0083$. The experimental subsidy, which was worth about 5% of the baseline price, increased treatment (control) prices by around 1.26% (0.83%).

Impact on Quantities

Given dp_T and dp_C, we can write: $q_T^1 = a + b(p^0 + dp_T) - c\Big((\mu n - 1)(p^0 + dp_T) + (1-\mu)n(p^0 + dp_C)\Big)$. The derivative of equilibrium quantities with respect to the subsidy are therefore: $\frac{\partial q_T}{\partial s} = -\frac{(1-\Gamma)\mu n}{\beta(1+\Gamma)(1+\Gamma+(1-\Gamma)n}$ and $\frac{\partial q_T}{\partial s} = \frac{\Gamma+(1-\Gamma)(1-\mu)n+1}{\beta(1+\Gamma)(1+\Gamma+(1-\Gamma)n}$.

The impact of the experiment on prices is then given by $dq_g = \frac{\partial p_g}{\partial s}s$, for $g = \{T, C\}$. Given our estimates of Γ and n, we can compute the (rounded) $dq_T = q_T^1 - q^0 = 323.7$ and $dq_C = q_C^1 - q^0 = -71.9$. Using a baseline quantity of $q^0 = 185.9$ (average quantity for control traders during the experiment minus dq_C), we obtain $q_T^1/q^0 = 2.74$ and $q_C^1/q^0 = 0.62$. The change in aggregate quantity is given by $\mu dq_T + (1-\mu)dq_C$, which corresponds to 3.8% of the baseline quantity.

Return on Investment

We consider the return on investment (ROI) on interventions 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.

In the pre-experiment period, farmer revenues are: $r^0 = p^0 Q^0 = p^0 nq^0$. In the experimental period, these become $r_1 = n\Big(\mu p_T^1 q_T^1 + (1-\mu)p_C^1 q_C^1\Big)$. The cost of the intervention is $C = sn\mu q_T^1$. The ROI is thus $\frac{r_1 - r_0}{C}$.

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

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

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

E Model Extensions

E.1 Trader Heterogeneity

The model presented in Section I 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.[7] 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 (5) and (9) 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 $\tilde{n}$.[8]

E.2 Non-study Traders

As discussed above, the model presented in Section II 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 $p_i = \alpha + \beta q_i + \gamma \sum_{j \neq i} q_j$.[9] 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 II 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 5). 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).

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

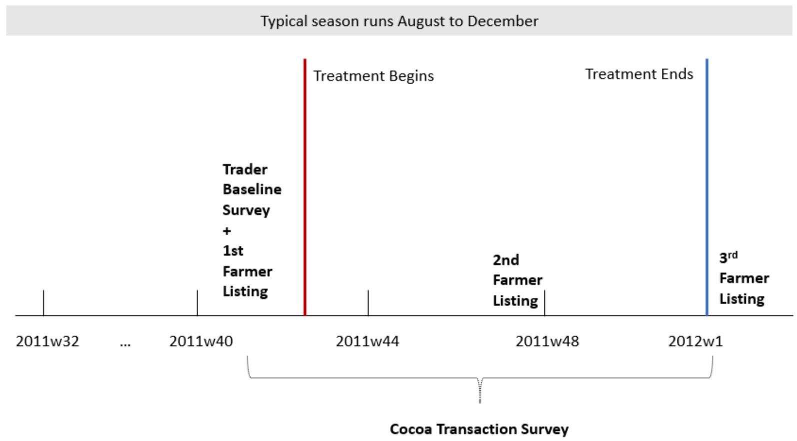
[8] The result extends to the general case of $m = 1, .., M$ groups of traders, with differentiation $\Gamma_m = \kappa^{m-1}\Gamma$. In this case, Δp, Δq, and ρ are as above and $\tilde{n} = \frac{n}{M}\frac{1-\kappa^M}{1-\kappa}$.

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

F Additional Figures and Tables

F.1 Timeline

Figure F.1: Timing of Experiment and Fielding of Survey Instruments



Notes: The figure shows the timeline of the intervention, the baseline survey, and the farmer listings. Traders were informed of the subsidy when the treatment began. Credit is offered and prices are negotiated throughout the crop harvest, and so we cannot indicate these moments on the timeline with accuracy. Our analysis of credit focuses only on credit offered during the 2.5 months of the experiment.

F.2 Price Results for Grade B and C

Table F.1: Treatment-Control Differences in Prices (Grade B and C)

	Grade B	Grade C	Grade B&C
	(1)	(2)	(3)
Treatment Trader	40.07***	-0.36	31.06**
	(13.22)	(18.24)	(13.72)
Control Group Mean	3025.82	3050.47	3035.91
Observations	532	231	763

Notes: The dependent variables in Columns 1, 2, 3 are the price per pound for grade B , grade C , and grade B&C, cocoa respectively. The subsidy to treatment traders for grade A was Le. 150. per pound. An observation is a shipment delivered by the trader to a wholesaler. All the regressions include week fixed effects. Standard errors are clustered at the level of the trader. ***p<0.01, **p<0.05, *p<0.1.

F.3 Traders' Costs

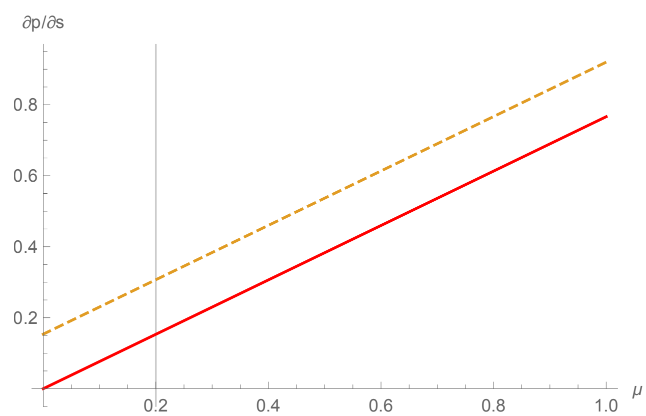
Table F.2: Treatment-Control Differences in Costs

	Transport Costs	Truck Dummy
	(1)	(2)
Treatment Trader	-13.88***	0.19***
	(4.02)	(0.06)
Control Group Mean	49.12	0.40
Observations	1079	1079

Notes: In Column (1), the dependent variable is the transport cost per pound, defined as the ratio of the transport cost per bag reported by the trader and the weight of the bag (measured in Leones per pound) . In Column (3), the dependent variable is a dummy equal to one if the trader reports using a truck to transport that bag. The subsidy to treatment traders was Le. 150. per pound. An observation is a shipment delivered by the trader to a wholesaler. All the regressions include week fixed effects. Standard errors are clustered at the level of the trader. ***p<0.01, **p<0.05, *p<0.1.

F.4 Price responses to subsidies to different share of traders

Figure F.2: Price responses to subsidies to different share of traders



Notes: 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, $\mu = 0.2$. For $\mu \to 1$, the response of treatment traders tends to the pass-through rate, 0.92.

References

Acemoglu, Daron, Tristan Reed, and James A Robinson. 2014. "Chiefs: Economic development and elite control of civil society in Sierra Leone." *Journal of Political Economy*, 122(2): 319–368.

Andersen, Steffen, Glenn W Harrison, Morten I Lau, and E Elisabet Rutström. 2008. "Eliciting risk and time preferences." *Econometrica*, 76(3): 583–618.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. "How Much Should We Trust Differences-in-Differences Estimates?" *Quarterly Journal of Economics*, 119(1): 249–275.

Cameron, A Colin, Jonah B Gelbach, and Douglas L Miller. 2008. "Bootstrap-based improvements for inference with clustered errors." *The Review of Economics and Statistics*, 90(3): 414–427.

CAOBISCO/ECA/FCC. 2015. "Cocoa Beans: Chocolate and Cocoa Industry Quality Requirements." *Memorandum.*

Casaburi, Lorenzo, Rachel Glennerster, Tavneet Suri, and Sullay Kamara. 2014. "Providing collateral and improving product market access for smallholder farmers. A randomised evaluation of inventory credit in Sierra Leone." *3ie Impact Evaluation Report*, 14.

Cohen, Jonathan, Keith Marzilli Ericson, David Laibson, and John Myles White. 2020. "Measuring time preferences." *Journal of Economic Literature*, 58(2): 299–347.

David, Sonii. 2005. "Learning about Sustainable Cocoa Production: A Guide for Participatory Farmer Training 1. Integrated Crop and Pest Management." *Sustainable Tree Crops Program, International Institute of Tropical Agriculture, Yaounde, Cameroon.*

Efron, Bradley. 1981. "Nonparametric standard errors and confidence intervals." *Canadian Journal of Statistics*, 9(2): 139–158.

IFAD. 2018. *Project Completion Report Validation - Rural Finance and Community Improvement Programme, Republic of Sierra Leone.*