

IMPACT OF THIRD-PARTY ENFORCEMENT OF CONTRACTS IN AGRICULTURAL MARKETS—A FIELD EXPERIMENT IN VIETNAM

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Introduction

Over the past two decades, many smallholder farmers in developing countries have benefitted from closer integration into global value chains, but constraints in accessing high-value markets remain (Reardon et al. 2009). Contract farming, which has recently become more widespread, links farmers to output markets for high-value foods such as fruits, vegetables, meat, or milk (Birthal, Joshi, and Gulati 2005; Swinnen 2009; Reardon et al. 2009; Mergenthaler, Weinberger, and Qaim 2009; Bellemare 2012). When agreements between buying companies and selling farmers are complemented by schemes to provide inputs, credit, or training, contract farming can also help to improve access to technology and overcome factor market inefficiencies (Masakure and Henson 2005; Minten, Randrianarison, and Swinnen 2009; Rao, Brümmer, and Qaim 2012).

While in developed countries there are strong institutions to enforce contracts, in developing countries this is rarely the case (Key and Runsten 1999; Kirsten and Sartorius 2002). An environment of weak institutions can negatively affect both buyers and sellers of farm output. For example, buying companies that provide finance or inputs as part of a contract lose if farmers renege on the agreement by diverting inputs to other crops or side-selling their output on the spot market (Gow and Swinnen 1998; Bellemare 2010). On the other hand, farmers may lose if the buying company has a nontransparent system of quality grading and thus the ability to manipulate prices. In this chapter, we focus on small-scale contract farmers who are negatively affected by information asymmetry with moral hazard resulting from weak institutions. Since Akerlof's (1970) seminal work, the economics of information has received considerable attention. A relatively new and important application is the study of emerging markets for high-value agricultural products. In these

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markets, product quality is an important factor in determining farmers' pay. As costly technology is required to assess nonvisible quality attributes, a harmful asymmetry of information may occur. The buyer can accrue information rents from reporting lower than actual quality levels, thus downgrading the price paid to the seller. Rational sellers forming the belief that the buyer cheats will factor in the buyer's opportunistic behavior, lowering their expectations about the product price. In this situation, contracts are characterized by additional price risk from the farmers' perspective. This can result in underinvestment in inputs and productive assets, leading to lower output (Gow, Streeter, and Swinnen 2000; Vukina and Leegomonchai 2006; Cungu et al. 2008). Such underinvestment by farmers can also increase the per-unit transaction costs of procurement for the buying company. Hence, weak contract enforcement may prevent whole industries from attaining their full potential, which is especially important in economies where the agricultural sector plays an important role.

Here, we investigate whether improved institutions for contract enforcement can break the information asymmetry and lead to higher investment and output produced. Specifically, we test the impact of an independent agency that can verify product quality resulting from the existing farmer production intensity and output levels. We also explore whether this type of contract enforcement can improve farm-household welfare.

Previous research indicates that more transparency in the supply chain is one possible solution to overcome harmful information asymmetry (Balbach 1998; Sykuta and Cook 2001; Young and Hobbs 2002). In a laboratory experiment, Wu and Roe (2007) have shown that third-party contract enforcement can be one way to successfully mitigate underinvestment and enhance social efficiency. But as the laboratory systematically differs from natural environments, external validity of this type of studies may be limited (Levitt and List 2007). Over the past decade, randomized controlled trials (RCTs), in which subjects take decisions in their natural environment, have been used extensively. Only recently, RCTs have been carried out in the field of agriculture (Duflo, Kremer, and Robinson 2008, 2011; Ashraf, Giné, and Karlan 2009; Cole, Giné, and Vickery 2013a).

We contribute to the literature on RCTs in agriculture and information asymmetry in contracts through a randomized field experiment, using the example of the fast-growing Vietnamese dairy industry, in which crucial institutions to support contract enforcement are missing. The industry is characterized by a great number of small-scale dairy farmers who are contracted by a large milk-processing company. This is a typical situation for emerging

markets for high-value agricultural products in developing countries (Reardon et al. 2009; Rao, Brümmer, and Qaim 2012; Bellemare 2012). In this field experiment, the contract of a randomly chosen subsample of farmers, the treatment group, is altered such that it becomes enforced; previously unobservable quality attributes are now measured and verified by an independent and certified laboratory. Control-group farmers continue to produce under the initial contract. For the field experiment, we collaborated with a private dairy company that provided access to weekly farm-level output data. This information is complemented with data that we obtained through household surveys.

We find that our intervention leads to higher input use and increased dairy output. There is also a positive treatment effect with respect to household consumption expenditures for a specific subgroup of the sample. We are able to attribute observed differences in output to a behavioral change of farmers rather than changes in the reporting strategy of the buying company. Hence, in this specific case, the buying company did not behave opportunistically, but the supply-chain architecture did not allow the buyer to signal its fair type to farmers. Third-party enforcement of contracts in an environment of weak institutions can move the supply chain to a first-best scenario, in which both smallholders and buying companies benefit from increased farm productivity.

The Vietnamese Dairy Industry

In Vietnam, much like in other countries of Asia, milk is becoming an increasingly popular food item, leading to high growth rates in the dairy sector. For example, only two decades ago the consumption levels of milk and dairy products were almost nil due to cultural practices and low incomes. But with increasing income, urbanization, and the spread of Western lifestyle, the demand for milk has increased tremendously (Saenger et al. 2013). Today's per capita consumption of milk in Vietnam has reached 15 kg per year, which is about 8 percent of the amount being consumed in the United States or Europe. Currently, the Vietnamese dairy sector is dominated by local processing companies, importing large quantities of powdered milk from overseas to satisfy domestic demand. However, more and more milk is produced domestically, especially by small-scale farmers. Fresh-milk production in Vietnam has tripled between 2003 and 2009, but it still meets only one-fifth of domestic consumption (USDA 2011).

The leader in the dynamic dairy industry—and the cooperation partner in this field experiment—is the formerly state-owned milk processor Vinamilk.

This company collects the major share of milk produced in Vietnam and is also a main importer of powdered milk. Currently, the company has contracted more than 5,000 small-scale dairy farmers, most of them located around Vietnam's largest city and commercial capital Ho Chi Minh City.

Supply-Chain Architecture and the Standard Contract

In Vietnam, milk is produced mainly on specialized small-scale farms. Crossbreed dairy cattle are held in sheds all year round. The major input is fodder; rations usually consist of forage produced on farms, complemented with purchased fodder, primarily concentrate. Farmers usually sell the entire milk output to one dairy company. Alternative sales options are limited. Informal channels exist, but can absorb only small quantities due to low demand for highly perishable raw milk in rural areas. Hence, small-scale dairy farmers, who have undertaken relationship-specific investment, have little bargaining power vis-à-vis large monopsonistic dairy companies.

The raw milk is channeled through milk-collection centers (MCCs) located in the vicinity of the dairy farms. An average MCC is supplied by about 100 farmers and is operated by a private entrepreneur working on commission for the dairy processor, in our case Vinamilk. Each MCC carries out the following tasks: collection and handling of the milk twice daily, sampling of the milk, initial testing of quality (through staff deployed by the dairy processor), and daily transport of raw milk to company processing plants, which are located in urban centers. The MCCs also administer the weekly payments to farmers.

The production contract between Vinamilk and the dairy farmers is a country-wide standardized written agreement, determining how much milk of what quality is purchased at what price. The output price per unit of milk p received by farmers is a function of milk quality θ :

$$p = f(\theta) \quad (1)$$

Quality is a composite measure of several parameters, most importantly milk fat and total solid content, as well as bacterial contamination, all of which depend on input use x (for example, type and amount of fodder, level of effort):

$$\theta = g(x) \quad (2)$$

Various factors, such as limited access to inputs, finance, and lack of skills, constrain farmers in developing and transition countries to produce high-quality milk (Dries and Swinnen 2004, 2010; Dries et al. 2009).

Vinamilk tries to address these limitations by supporting farmers through the provision of training to overcome technical gaps, while the MCCs provide farmers with prefinancing for inputs (concentrate). But incentive pricing remains the main instrument at Vinamilk's disposal to promote the delivery of raw milk with high milk fat and total solid content, which is desired as raw material in the high-value segment. At the same time, the company uses price signals to discourage the supply of raw milk with high contents of psychrotrophic bacteria or antibiotics, which are known to increase processing costs (Claypool 1984). Worse, adulteration of milk along the supply chain can even have adverse health effects for consumers, as the recent case of melamine-tainted milk in China has shown (Jia et al. 2012).

In the early stages of dairy value-chain development in Vietnam, it was prohibitively costly to assess the quality of milk supplied by individual farmers. Instead, the milk was pooled at MCC level. Only one sample per MCC was taken to be analyzed at the dairy plant, and one common price was paid to all farmers delivering to the same MCC. Hence, the company lacked the ability to trace back the milk to its origin and thus to attribute milk quality to individual farmers. As a result, the company was hardly able to incentivize quality milk production. Today, cheaper quality-testing devices allow dairy companies to assess quality individually for each farmer, which is a key requirement for incentive pay. This also reduces the risk of deliberate milk adulteration in the supply chain. Vinamilk employs tests to detect undesired substances; suspect batches of potentially tainted milk are not accepted, and farmers are banned from further supplying milk to the company. The high probability of being caught is a strong incentive for farmers to refrain from adulterating.

To assess milk composition (milk fat and total solid content), Vinamilk staff take milk samples individually from the daily delivery of each farmer to the MCC. One sample per week from each farmer is randomly selected for further analysis with sophisticated laboratory methods in the dairy plant. Producers have unique identification numbers and are paid individually according to their own output (quantity q and quality θ). The base price for top-quality milk is subject to harsh deductions if one or more of the quality parameters falls short of the requirements set by the company. One-tenth of a gram of milk fat below the threshold—a deviation far too small to be visually detected even by experienced farmers—can trigger financial penalties. As milk analyses are carried out in the company's own laboratory and cannot be observed by farmers, milk quality remains private information of the dairy company. Currently, smallholders cannot overcome the information asymmetry regarding milk quality by systematically cross-checking the results

provided by the company, because individual milk testing is prohibitively costly, and collective action fails.

It should be noted that Vinamilk's technical capacity to assess milk quality individually for each farmer has shifted the informational advantage. Before individual testing was possible, farmers had an informational advantage about the quality of their milk; now, the company has an advantage. These dynamics also have important implications for the distribution of gains in contract farming. This first step of supply-chain development in Vietnam—from the assessment of pooled milk to individual milk testing—may have led to a distribution of gains in favor of the company. This is in line with a model on rent distribution in global value chains developed by Swinnen and Vandeplass (2011). The subsequent step that restores the symmetry of information between farmers and the company, as intended by our intervention, could increase market efficiency, triggering a further round of adjustments in the distribution of gains—this time in favor of farmers.

A Simple Model of Underinvestment

The information asymmetry in the Vietnamese dairy contracts, where farmers do not know the exact price that the buying company will pay, can be understood as a form of price risk. The effects of price risk on household behavior and welfare have received considerable attention in the theoretical literature. Baron (1970) and Sandmo (1971) analyzed the impact of price uncertainty on producer output and profit. This analysis was extended to consumers (for example, Deschamps 1973) and farm households that can be both producers and consumers (for example, Finkelshtain and Chalfant 1991; Barrett 1996). Recently, Bellemare, Barrett, and Just (2013) further developed the framework to cover price volatility of multiple commodities for producing and consuming farm households and also used this framework for empirical estimates.

In our analysis of dairy contracts in Vietnam, we only focus on milk for which farm households are pure producers.¹ Hence, we build on the model for producers described by Sandmo (1971) to formally derive how the described asymmetric information on relevant quality attributes leads to lower output and input use as compared to a situation with symmetric information. It is assumed that farmers maximize expected utility of profits from milk

1 Dairy farmers in Vietnam do not produce milk for home consumption. Overall, milk consumption in rural areas of Vietnam is very low.

production. The utility function is a concave, continuous, and differentiable function of profits. The farmer's cost function is defined as:

$$T(q) = V(q) + F \quad (3)$$

where $V(q)$ is the variable cost function, which depends on output quantity q , and F represents the fixed cost. Further, we assume that the cost function has the following properties:

$$V(0) = 0, V'(q) > 0, V''(q) > 0 \quad (4)$$

In a contract with symmetric information, the profit function can be defined as:

$$\pi(q) = pq - [V(q) + F] \quad (5)$$

where the product of output price p and quantity q is total revenue. Without price risk, farmers maximize profits where marginal cost equals marginal revenue according to:

$$V'(q) = p \quad (6)$$

We now take this as the baseline and analyze how optimal output and thus input use change when the buying company has private information about product quality θ . Exploiting its informational advantage, the company may report to the farmer a quality level that is lower than the one actually assessed in its own laboratory. According to equation (1), such underreporting of quality would negatively affect the output price, while increasing company profit. For dairy companies in the Vietnamese market this would be a tempting practice, given that they face competition not only from domestic processors but also from imports.

Such information asymmetry leads to price uncertainty for farmers. Output price becomes a random variable with an underlying density function $h(p)$ and an expected value $E[p] = \mu$. The expected utility of farmer profit becomes:

$$E\{U[pq - V(q) - F]\} \quad (7)$$

Hence, the first-order condition for a profit maximum is:

$$E\{U'(\pi) [p - V'(q)]\} = 0 \quad (8)$$

which can also be written as:

$$E[U'(\pi)p] = E[U'(\pi)V'(q)] \quad (9)$$

If we subtract $E[U'(\pi)\mu]$ on both sides of equation (9), we get:

$$E[U'(\pi)(p - \mu)] = E[U'(\pi)(V'(q) - \mu)] \quad (10)$$

Sandmo (1971) showed that the left-hand side of equation (10) is negative if $p \geq \mu$. This can be assumed in our case, because the price uncertainty comes from asymmetric information. Farmers who believe that the company underreports output quality will have a price expectation μ that is lower than p . In that case, the right-hand side of equation (10) must be negative as well, and it can be written as:

$$E[U'(\pi)] (V'(q) - \mu) < 0 \quad (11)$$

Since marginal utility is always positive, this implies:

$$V'(q) < \mu \quad (12)$$

Equation (12) shows that profit-maximizing output is less than the expected price, and, since $\mu < p$, optimal output quantity with information asymmetry is clearly lower than without. This also implies lower input use.

The introduction of independent quality assessment to enforce the contract would mitigate the negative effect on the expected price level, because formerly unobservable quality attributes would become verifiable for farmers. This would force the dairy company to report the actual level of quality, leading to a situation where $\mu = p$. Hence, we hypothesize that independent testing will induce farmers to increase their output.

How can dairy farmers practically respond to higher expected output prices? Generally, they can raise the output of milk fat and total solid—the value-defining parts of the raw milk—in three ways: (1) increase the quality (milk composition) while keeping the milk quantity constant, (2) keep the quality constant while increasing the quantity, or (3) simultaneously increase quality and quantity. At the farm level, the goal of increasing the absolute quantity of milk fat and total solid can be achieved in different ways. In the short run, the main instrument would be to increase the amount of purchased fodder (that is, concentrate) to make the ration more nutritious. Hence, we hypothesize an increase in concentrate use through independent quality assessment. All other inputs are quasi-fixed in the short run. The supply of forage produced on the farm can only be increased in the medium or long run, as additional land would have to be acquired. Likewise, the herd size can only be increased in the medium or long run, as this requires significant investments for buying cattle or raising own female calves. In the long run, selective breeding may also improve the herd's overall genetic potential for milk production.

Experimental Design and Implementation

After having outlined the theoretical framework of third-party contract enforcement, we now describe the design and practical implementation of our intervention in which randomly selected dairy farmers were provided with the opportunity to verify milk-testing results provided by Vinamilk.

Every treatment farmer received three nontransferable vouchers, each valid for one independent analysis of milk quality (milk fat and total solid). Vouchers were meant to be executed whenever eligible farmers challenged the testing results reported by Vinamilk. Providing farmers with third-party quality verification involved complex transport and testing logistics. For each milk sample obtained at the MCC under the original contract (hereafter A-sample), an additional identical sample (hereafter B-sample) had to be taken for each treatment farmer. The B-sample was sent to an independent and certified laboratory in Ho Chi Minh City, where it was stored. If a farmer executed a voucher, the B-sample was analyzed by the third-party laboratory, and the testing results were reported by mail to the farmer. This allowed the farmer to compare if the results based on the A-sample reported by Vinamilk were identical to the results of the corresponding B-sample provided by the independent laboratory.

While Vinamilk knew the identity of the treatment farmers, the actual execution of vouchers could not be observed, that is, the company did not know when an individual farmer in the treatment group executed her voucher. Hence, there was a constant threat to the company that any of the farmers in the treatment group could in any given week verify their testing result and detect potential opportunistic behavior. The combination of a constant threat to be caught and the associated high reputational costs,² should effectively discourage Vinamilk from behaving opportunistically. This is a central assumption in our study and crucial for the intervention to work; we will substantiate this assumption further below.

Compared to validating the results of every sample analyzed by Vinamilk, the voucher mechanism enabled us to systematically overcome the information asymmetry on milk quality attributes at relatively low cost. All outlays arising from setting up a parallel testing infrastructure for the B-samples and milk analyses were borne by the project, ruling out that farmers would not request independent milk testing for reasons of monetary costs.

2 Before the intervention started there was substantial coverage in Vietnamese newspapers, alleging that Vinamilk mistreated farmers by paying too low and unfair milk prices. While Vinamilk was publicly denying these allegations, the reports caused some reputational damage (*Viet Nam News* 2008).

The logistics of the voucher treatment are complex. Thus, it was especially important that both the treatment farmers delivering milk and the Vinamilk staff taking the B-samples thoroughly understood the procedure. During a compulsory half-day workshop, treatment farmers were informed about the independent milk testing and learned how to use the vouchers. Every treatment farmer received written instructions, supplementing the information presented during the workshop, and was provided with a phone number of trained field staff. To assure that farmers regarded the third-party testing as credible and independent, we had identified a certified laboratory that both farmers and Vinamilk explicitly agreed on. Further, to ensure the comparability of the A- and B-samples, we calibrated the third-party laboratory and Vinamilk's in-house laboratory using imported reference milk. By employing the same cooling technology we also assured that during transport and storage the A- and B-samples were kept in identical environments.

To avoid contamination in the sense that control-group farmers get access to the third-party milk testing and thus effectively become treated, the emergence of a secondary market for vouchers had to be prevented. We handed out personalized vouchers tagged with a unique identification number. Vouchers passed on to other farmers (also outside the treatment group) automatically became invalid. A scenario in which control farmers sell their milk through treatment farmers to benefit indirectly from the independent quality verification and higher expected milk prices is possible but seems unlikely. First, to maintain traceability within the milk supply chain, Vinamilk's procurement policy includes a mechanism to control the quantity delivered by individual farmers, strongly discouraging milk producers from accepting milk from other sources. Specifically, milk producers are required to register their herd size with Vinamilk. This information, which is regularly verified through field visits, enables the company to estimate the production potential of each farmer. Thus, Vinamilk would notice if a farmer were to accept milk from others and thus increase their delivery beyond expected levels. Second, if a treatment farmer accepts milk from a control farmer (or any other source), she would take the risk of mixing her own milk with milk of unknown quality, potentially leading to financial loss.

If participation in field experiments is voluntary, individuals who are assigned to the treatment group may refuse to participate. This may lead to low compliance rates which can be a challenge for impact analysis. Cole et al. (2013b) found that uptake rates for innovative crop insurances in India were as low as 5–10 percent despite high potential benefits. Hill and Viceisza (2012) overcame the problem of low uptake in a framed field experiment by

imposing mandatory insurance. Our intervention is special with respect to compliance, as a high voucher-execution rate is not a necessary condition for the voucher treatment to be effective. The specific design of the third-party contract enforcement does not depend on an individual farmer's decision to execute a voucher to build a direct threat to Vinamilk. Instead, it is sufficient if farmer A believes that farmer B or C may request an analysis. This belief—from farmer A's point of view—would be an indirect but sufficiently powerful threat to the dairy company being monitored, ruling out underreporting. Ultimately, this would imply that all farmers in the treatment group can be regarded as treated, regardless of their actual voucher execution. This is a further necessary assumption for our experimental design to be effective, which we will also substantiate further below.

It should be noted that when designing the voucher treatment, we were interested in isolating the effect of third-party contract enforcement in general, rather than evaluating a particular way of providing farmers with independent testing of quality attributes. Our voucher-based approach is too costly to be easily scaled up. In a nonexperimental setting, complete outsourcing of milk testing to an independent laboratory would be more efficient than establishing a parallel structure for B-sample analyses.

Study Area, Sample, and Randomization

Almost 70 percent of the domestically produced milk in Vietnam stems from the region around Ho Chi Minh City. The study area is located in Long An and Tien Giang, two provinces south of Ho Chi Minh City where Vinamilk has contracted 402 dairy farmers. The milk supply is channeled through four MCCs.

At MCC level, differences with respect to average dairy output (quantity, quality) can be observed (Appendix Table 11.A1). Three out of the four collection centers (MCCs B, C, and D) are spatially clustered, so it is unlikely that agroecological factors cause the performance differential. As farmers can choose freely which MCC to supply their milk to, we suppose that selection based on unobservables may cause the farmer population of one MCC to systematically differ from farmers at other MCCs. For example, dairy producers choose an MCC not only on the basis of the distance to their farm but also based on soft factors such as trust toward the management of the MCC. Beside the three clustered MCCs, there is also one more isolated collection center (MCC A) where farmers do not have the option to choose between different Vinamilk MCCs. However, a competitor of Vinamilk sources raw milk in the area of MCC A. Hence, farmers could switch to the competing dairy

company, if they were dissatisfied with Vinamilk, the contract, or the collection center management. We reckon that farmers who deliver to Vinamilk despite having an alternative may be systematically different from Vinamilk farmers without such an outside option. Such possible differences are accounted for in our analysis through MCC dummies.

Given the limited number of MCCs and significant mean differences in observable characteristics, a randomization of treatment status over MCCs—even though easier to manage—might have confounded our results. Hence, we randomized over the entire population of 402 dairy farmers. In May 2009, all farmers attended a public lottery in which 102 farmers were randomly assigned to the treatment group. Another 100 farmers were randomly assigned to the control group, continuing to produce under the original contract without third-party enforcement. Farmers were informed that due to a budget constraint and for the sake of a clear evaluation of the experiment only a limited number of slots would be available in the treatment group. Owing to the complexity of the treatment design, the implementation had to be delayed several times. The intervention eventually started in May 2010 when the first batch of B-samples was obtained. It was continued for a period of 12 months.

Data

We collected detailed information for all farmers participating in the experiment. Through two rounds of structured household surveys we generated a dataset comprising socioeconomic details on dairy production, income from agricultural and nonagricultural activities, household expenditures, and assets owned. Additionally, questions measuring social capital, trust, time, and risk preferences were included in the questionnaire. The first round of interviews, the baseline survey, took place in May 2009 before the experiment started. In May and June 2011, all farmers were revisited for the follow-up survey when the experiment was completed.

The household data were complemented with farm-level output data for each producer in the sample provided by the company. Vinamilk provided these data for the period from May 2008 to May 2011, covering 24 months before the intervention and the time period of the intervention. On the one hand, it can be assumed that these data are of higher quality than self-reported recall data on output obtained through household surveys, as this weekly reported information—disaggregated by milk quantity and three quality parameters—is the basis for farmers' payment. On the other hand, the dairy company may have an incentive to strategically release

information, providing manipulated data to mask underreporting of milk quality and price in case farmers were cheated before the intervention. This would clearly undermine the internal validity of the results. If the company had underreported output quality before the independent quality verification was implemented, we would not be able to easily attribute observed effects to changes in either farmer or company behavior. In an extreme case, higher output could be entirely the result of Vinamilk ceasing to underreport quality. We carefully address this issue in the discussion section.

Identification Strategy and Econometric Estimation

The impact of third-party quality verification is assessed in three dimensions: (1) input use in milk production, (2) output generation in milk production, and (3) welfare of the farming household. While (1) is measured by the amount of purchased fodder (concentrate) used per cow and day reported by farmers, (2) is captured by three variables, namely total amount of milk fat and total solid produced during the 12 months when the experiment was ongoing, as well as revenues from dairy farming for the same time period. Data on these output variables are provided by the company. For (3) we use total annual household consumption expenditures on food (own-produced food items were valued at the market price), other consumer goods, and durables obtained through the household surveys.

We seek to identify two types of treatment effects: first, the average treatment effect on the treated (ATT), which is estimated according to:

$$ATT = E(y_1 - y_0 | v = 1) \quad (13)$$

where ATT is the difference between y_1 , the average outcome of the treated, and y_0 , the counterfactual outcome of the untreated, conditioned on the treatment status $v = 1$, which means being treated. In view of the random assignment of v , the control group constitutes an adequate counterfactual of the treatment group.

Second, we would like to assess the ATT conditional on specific baseline covariates x . To estimate this heterogeneous treatment effect, we condition ATT on x according to:

$$ATT(x) = E(y_1 - y_0 | x, v = 1) \quad (14)$$

Given that the voucher use was voluntary (we did not use an encouragement design), one might argue that the intention to treat (ITT) analysis would

be a more suitable approach. In our view, the ATT analysis seems appropriate because all farmers in the treatment group can be regarded as treated. As explained above, the effectiveness of the third-party quality verification scheme does not depend on an individual farmer's decision to execute the voucher.

In order to estimate ATT econometrically, we first specify an ordinary least squares (OLS) according to:

$$y = \alpha + \beta v + \varepsilon \quad (15)$$

where the dependent variable y is the outcome variable of interest measured at the end of the experiment, and v is the treatment dummy.

To estimate average treatment effect on the treated, ATT(10), and thus to explore treatment heterogeneity, we modify the model by including a vector of variables, indicating baseline characteristics at time t_0 , and an interaction term between treatment and baseline characteristics:

$$y = \alpha + \beta v + \gamma x + \delta vx + \varepsilon \quad (16)$$

This interaction term allows for testing whether the relationship between baseline characteristics and outcome variables is different conditional on treatment status. One specification for ATT(10) includes the variable *trust toward Vinamilk*, which is a dummy variable taking the value 1 if farmers agreed with the statement "Vinamilk is a trustworthy business partner" in the baseline survey, and 0 otherwise.³ We suppose that initial trust levels may affect the impact intensity of the voucher. For example, farmers already trustful in the baseline may be less affected by an intervention that aims at ruling out potential opportunistic behavior by Vinamilk. Another specification for ATT(10) includes dummies indicating the farmers' delivery to milk-collection centers (MCC B, MCC C, and MCC D; MCC A was chosen as benchmark). These dummies capture the effect of unobserved characteristics that make farmers select a specific MCC.

Randomization

Before the impact analysis, we verified that treatment and control groups are similar statistically with respect to the large number of observables available from the baseline survey, including the outcome variables (Table 11.1). The

3 In the baseline survey, interviewees had to rate this statement on a four-point Likert scale ("very much agree," "agree," "disagree," "very much disagree"; the option "I don't know" was also included). We collapsed the responses into a dummy taking the value 1 if farmers opted for "agree" or "fully agree," and 0 otherwise. It should be noted that the results are sensitive to the specification; when including dummies for each response level, the coefficients for these trust variables are insignificant.

TABLE 11.1 Mean difference for baseline variables in treatment and control groups

Variable	Control – voucher	Standard error
Basic household (HH) characteristics		
Age of HH head (years)	1.233	1.558
Education of HH head (years of schooling)	0.556	0.442
Number of HH members	0.073	0.183
Total land size (m ²)	893	783
Distance to paved road (km)	0.270*	0.122
If agree to postpone at interest rate \leq 3.5 percent (1 = y)	-0.183**	0.069
Dairy enterprise		
Delivers milk to MCC A (1 = y)	0.033	0.063
Delivers milk to MCC B (1 = y)	-0.098	0.064
Delivers milk to MCC C (1 = y)	0.065	0.065
Delivers milk to MCC D (1 = y)	-0.000	0.065
Daily concentrate per cow (kg)	1.626	1.826
Absolute milk fat (kg)	-53.519	59.996
Absolute total solid (kg)	-173.342	194.658
Annual revenue from dairy (US\$)	-432.499	550.234
Household expenditure		
Annual HH expenditure (US\$)	36.410	111.463

Source: Authors' estimates.

Notes: HH = household; MCC = milk-collection center; probability = * < .1, ** < .05.

only statistically significant differences (at 10 percent level) are for the variables capturing road infrastructure and time preferences,⁴ indicating that treatment farmers are located slightly further away from paved roads and are more patient than their peers in the control group. It should be noted that given the random assignment of the treatment status, the observed differences are not systematic, that is, worse infrastructure and lower time preferences did not make a household more likely to be assigned to the treatment group.⁵

Attrition

Between the baseline survey in May 2009 and the implementation of the treatment in May 2010, a number of milk farmers ceased production or switched from Vinamilk to a competing dairy processor. The number of households in the treatment and control groups decreased from 102 and 100

4 In the baseline survey, we elicited—through a battery of choices between hypothetical pay-offs—the discount rates at which farmers accepted to wait for one month to receive a significant lump-sum payment. The variable was converted into a dummy which takes the value 1 if farmers agreed to wait for one month if a monthly interest rate of up to 3.5 percent is paid. These farmers have a lower time preference and can be considered more patient.

5 As robustness checks, both variables were included in the regression models, but their inclusion led neither to significant coefficients for these variables nor to notable changes in the treatment effects.

to 94 and 90, respectively. Those producers dropping out of the sample have significantly (at 10 percent level) smaller baseline herd sizes, are less productive, and have lower revenues from milk. The attrition rate is balanced between treatment and control groups.

Compliance

As discussed above, the intervention did not require high compliance rates, that is, it was not necessary for a large number of treated farmers to actually execute their vouchers, for the treatment to be effective. Nevertheless, from a treatment farmers' perspective a certain minimum level of compliance in the treatment group might be (psychologically) desirable to credibly build up the threat of effective monitoring vis-à-vis the dairy company.

We find that only seven farmers (out of 94) had actually requested independent verification of milk-testing results despite it being easy, cheap, and safe to execute. Those farmers who had executed vouchers had larger herd sizes with more productive dairy cattle on average. A possible explanation could be that these larger farmers had greater interest in verifying the milk-testing results, because even little underreporting of quality by the company would lead to substantial losses due to larger quantities involved. We systematically evaluated the voucher treatment in the follow-up survey to identify reasons for low execution rates. The survey results suggest that the majority of farmers who had not executed a voucher agreed that third-party quality assessment was useful and easy to request, and that the independent laboratory is trustworthy. Around 50 percent of all treated farmers stated they had not executed a voucher because they were satisfied with the milk-quality results provided by Vinamilk. A significant proportion (almost 40 percent) indicated that they would feel uneasy secretly checking up on Vinamilk. While we assured them that they would face no monetary cost for executing vouchers, these findings suggest that some might still consider it risky to double-check the testing results in terms of jeopardizing the relationship with the company.

It is likely that this subjectively felt risk could foster free-riding among treatment farmers, meaning here that individuals take advantage of the third-party enforcement while not actively contributing to the scheme through execution of a voucher. Such behavior does not necessarily undermine the scheme's overall effectiveness (as perceived by individual farmers) in providing protection against opportunistic behavior by the company. Also, in other contexts, it is not uncommon that individuals choose to free-ride, while still believing in the effectiveness of the system as a whole. For example, in the area of public health

it can be observed that some free-ride on the protection offered by a vaccination scheme. Individuals may want to avoid possible risks of getting vaccinated, expecting that a sufficient number of other people will get vaccinated to ensure the desired level of protection (Bauch, Bhattacharyya, and Ball 2010).

We are confident that the low execution rate of vouchers in our case does not undermine the effectiveness of the intervention and does not pose a major problem for impact analysis. Based on the above discussion, we argue that all individuals assigned to the treatment group (except for dropouts) can be regarded as treated. Nevertheless, we acknowledge that for future research an encouragement design would be a good way to overcome this potential limitation.

Estimation Results

At first we investigate how the treatment affects self-reported fodder usage (concentrate fed per cow and day in kilograms). Results are presented in Table 11.2, columns (1) to (3). We find a significant positive treatment effect, which is robust across specifications. Farmers in the treatment group on average fed their cattle 0.83 kg more purchased concentrate than their peers in the control group, which implies an increase of 12 percent. The coefficients of the additional control variables, baseline trust toward Vinamilk, and the affiliation to a specific milk collection center are mostly insignificant. As we do not find significant effects for the interaction terms, the treatment effect seems to be homogeneous. That is, we find a significantly positive impact of third-party enforcement on input use that does not differ across treatment farmers.

Beside the amount of purchased concentrate, which makes up the largest share of total input costs, we also analyzed treatment effects for labor input, veterinary services, and artificial insemination. For these other inputs we did not find significant differences between treatment and control groups.

The regression results for dairy output are also presented in Table 11.2. Without controlling for other covariates, the ATT for absolute milk fat and solid produced is positive but insignificant. Also the level of baseline trust does not seem to have an impact. But once we control for milk-collection center affiliation, we find significant treatment effects for both output measures (columns 6 and 9). Apparently, third-party enforcement of the contract increases not only concentrate use but also output quantity, as hypothesized. Yet the treatment effects for output quantity are not homogeneous across milk-collection centers. Considering the interaction terms in columns (6) and (9) of Table 11.2, we find significant effects for farmers delivering to MCCs

TABLE 11.2 Estimation results for input use and output produced

	Input			Output					
	Daily concentrate per cow (kg)			Absolute milk fat (kg)			Absolute total solid (kg)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Voucher treatment (1 = y)	0.826** [0.365]	0.869** [0.414]	0.973* [0.512]	71.3 [80.4]	119.1 [111.1]	289.7** [136.9]	227.6 [249.5]	387.8 [344.1]	913.9** [433.4]
Trust toward Vinamilk (1 = y)		-0.020 [0.369]			157.6 [141.5]			505.5 [448.0]	
Vinamilk trust* Voucher		-0.033 [0.566]			-82.0 [165.8]			-279.7 [515.8]	
MCC B (1 = y)			-0.935 [0.504]*			212.8 [134.5]			683.3 [425.7]
MCC C (1 = y)			-0.847 [0.541]			29.5 [151.7]			105.6 [480.1]
MCC D (1 = y)			0.136 [0.541]			110.3 [144.9]			367.7 [458.6]
MCC B * Voucher			0.271 [0.701]			-154.8 [193.4]			-522.8 [611.9]
MCC C * Voucher			-1.059 [0.709]			-271.9 [198.5]			-829.2 [628.3]
MCC D * Voucher			0.088 [0.720]			-363.7* [197.0]			-1,139* [623.5]
Constant	6.905*** [0.284]	6.915*** [0.399]	7.375*** [0.393]	515.2*** [53.3]	443.2*** [54.98]	415.2*** [101.1]	1,630*** [170.3]	1,399*** [167.9]	1,304*** [320.1]
Observations ^a	164	162	164	172	170	172	172	170	172
R-squared	0.056	0.060	0.221	0.006	0.025	0.071	0.006	0.026	0.066

Source: Authors' own estimates.

Notes: MCC = milk-collection center; Robust standard errors, clustered at MCC level, in brackets; probability = * < .1, ** < .05, *** < .01. The number of observations varies across models because of missing values for some of the variables. We also ran alternative estimates with equal number of observations across models, excluding farmers with missing values throughout. The results are similar, although with somewhat larger standard errors in some cases (see Table S1 in the supplementary material online at <http://dx.doi.org/10.1093/ajae/aaud21>).

A, B, and C, but not for those delivering to MCC D. The treatment effects for farmers delivering to MCC A are particularly large. The coefficients for the treatment dummy imply an increase of approximately 40 percent for milk fat and total solid. We already explained above that farmers who deliver to MCC A may be systematically different. For farmers in MCCs B and C, the treatment effects are smaller but remain positive and significant.

We also ran regressions using average fat and solid content per kilogram of milk as dependent variables,⁶ without finding significant treatment effects (results not presented here). Looking at quality, the relative composition of milk remained constant, as a comparison of fat and total solid content per kilogram of milk before and after treatment shows. This might seem surprising given that the aim of the intervention was to break the asymmetry of information with respect to milk-quality attributes. A possible explanation for the observed increase in milk quantity instead of quality can be found in the physiology of dairy cattle. To produce large quantities of milk, the dairy cow requires a nutritious and balanced fodder ration, especially with respect to protein and energy. If the ration is unbalanced, for example if it contains too little protein relative to energy, milk yields will drop (Roth, Schwarz, and Stangl 2011). The concentrate purchased by farmers in Vietnam is rich in protein. It is therefore plausible that an increase in concentrate use, as observed among treated farmers, contributes to relaxing a protein constraint in the fodder ration, leading to higher milk quantity produced per cow. The same increase in protein-rich concentrate alone does not necessarily lead to higher fat and solid content per kilogram of milk.

Higher output leads to more revenues from dairy production, as shown in Table 11.3. The positive and significant coefficient of the treatment dummy in column (3), in which baseline characteristics are controlled for, points to a heterogeneous treatment effect, especially with respect to MCC affiliation. As milk quality was not affected by the treatment and thus the average price received remained unchanged, the increment in revenue can entirely be attributed to increased production quantity.

Finally, we look at the intervention's impact on total household consumption expenditures, a commonly used measure of living standard and welfare. We do not find a significant ATT (Table 11.3, columns 4 to 6). This is not surprising, because households tend to adjust their consumption expenditures only slowly, that is, an increase in revenue or profit may not immediately be

⁶ In dairy farming, the output is usually quantified using weight measures such as pounds or kilograms.

TABLE 11.3 Estimation results for revenue and household welfare

Parameter	Revenue Annual from dairy (US\$)			Welfare Annual HH expenditure (US\$)		
	(1)	(2)	(3)	(4)	(5)	(6)
Voucher treatment (1 = y)	812.8 [830.7]	1,446 [1,179]	3,164* [1,659]	82.95 [375.0]	-733.6 [358.9]	153.5 [931.4]
Trust toward Vinamilk (1 = y)		1,940 [1,725]			-661.5 [390.0]	
Vinamilk trust * Voucher		-1,119 [1,855]			1,731** [363.4]	
MCC B (1 = y)			3,173* [1,629]			-63.94 [907.6]
MCC C (1 = y)			874.4 [1,837]			-344.6 [972.8]
MCC D (1 = y)			1,931 [1,755]			-778.6 [940.5]
MCC B * Voucher			-1,825 [2,342]			-1,185 [1,310]
MCC C * Voucher			-2,704 [2,404]			658.8 [1,300]
MCC D * Voucher			-3,859 [2,386]			-17.20 [1,294]
Constant	6,118*** [762.3]	5,232*** [693.1]	4,474*** [1,225]	4,106*** [181.2]	4,400*** [316.6]	4,401*** [687.9]
Observations ^a	172	170	172	184	182	184
R-squared	0.005	0.025	0.060	0.000	0.021	0.026

Source: Authors' own estimates.

Notes: HH = household; MCC = milk-collection center; Robust standard errors, clustered at MCC level, in brackets; probability = * <.1, ** <.05, *** <.01; ^a The number of observations varies across models because of missing values for some of the variables. We also ran alternative estimates with equal number of observations across models, excluding farmers with missing values throughout. The results are similar, although with somewhat larger standard errors in some cases (see Table S2 in the supplementary material online at <http://dx.doi.org/10.1093/ajae/aau021>).

reflected in changed consumption behavior. To measure impacts on consumption expenditure, the duration of the experiment may have simply been too short.⁷

But we observe a significant welfare increase for those treatment farmers who were more trustful toward the company before the intervention. This

7 In a recent impact assessment for a new agricultural technology in India based on observational panel data, Kathage and Qaim (2012) also found that technology adoption did not raise household expenditure in the beginning, in spite of sizeable profits gains, but significantly contributed to increased consumption after some time. In general, household consumption levels tend to change less rapidly than income levels.

can be inferred from the positive coefficient of the trust–voucher interaction term in column (5) of Table 11.3. Interestingly, neither the trust variable itself nor the interaction term was significant in any of the previous models. The results here appear counterintuitive on first sight, as one would expect stronger impacts for farmers who do not trust the company much. Yet, it should be noted that our trust variable may capture trust toward the company in multiple dimensions, also beyond quality reporting. The statement “Vinamilk is a trustworthy business partner” that farmers were asked to rate may also involve expectations regarding the timing of payment, or beliefs about the company’s long-term commitment to the contractual relationship. Hence, farmers with lower levels of trust may perceive the relationship with Vinamilk as riskier, and thus act more cautiously, for instance by saving additional revenues instead of spending more on consumption. While we did not explicitly collect data on savings, higher profits without higher consumption expenditures imply higher savings.

Discussion

Our findings confirm the hypothesis that third-party enforcement of contracts mitigates underinvestment, and hence are in line with Wu and Roe’s (2007) results from laboratory experiments with college students. Furthermore, our study shows that, under real-world conditions, higher input levels observed under the enforced contract actually translate into higher output, a result that we hypothesized based on the theory but that would be impossible to obtain in the laboratory. The findings also suggest that specific subgroups are affected to differing degrees by the intervention. Differences occur especially between farmers delivering to different collection centers. Given data limitations, we are not able to further analyze possible mechanisms that may explain these differences in the treatment effects. In part, they may be due to unobserved factors that determine farmers’ self-selection into specific MCCs.

Contamination

As pointed out before, by issuing personalized vouchers for treatment to farmers we avoided the possibility of control farmers getting direct access to third-party quality assessment. However, the random assignment of the treatment may still have led to contamination more indirectly through trust spillovers. For example, it is possible that a control farmer updated her belief about Vinamilk’s type from “unfair” to “fair” after communicating with a neighboring treatment

farmer. We evaluated this possibility through specific questions in the follow-up survey.⁸ Trust levels of both treatment and control farmers significantly increased (though more so for treatment farmers), pointing to the existence of positive spillovers. Hence, we conclude that the treatment effects that we measured possibly underestimate the real impact of third-party contract enforcement.

A cleaner design, less susceptible to spillovers, would have required us to strictly separate treatment and control farmers, to avoid communication between groups. However, choosing the MCC as unit of randomization, as one possible way of separating treatment and control farmers, would have been much more costly due to the large number of collection centers needed for proper randomization. With only a small number of MCCs, as in our case, randomization among MCCs could have led to biased treatment effects due to systematic differences, as discussed above.

Data Provision and Incentive Compatibility

We attribute the entire treatment effects to a behavioral change of treatment farmers, not to a change in Vinamilk's reporting behavior. This is justified, but deserves further explanation. We distinguish between the output (quantity and quality) *reported* by Vinamilk and the *true* output obtained using laboratory methods. In the baseline scenario before the intervention, milk quality was private information of Vinamilk. If the company had exploited this informational advantage, reported output levels would have been lower than true output levels. If instead Vinamilk played fair, reported and true output levels would have been identical.

We have shown that independent verification of quality attributes made farmers produce more milk fat and total solid during the intervention compared to the baseline. This was a result of an increase in milk quantity q , while milk quality θ remained unchanged. It is important to note that the quantity of milk delivered has been observable to both farmers and the company at any point in time, before and during the intervention, because milk is weighed at the MCC under the eyes of the farmers. This implies that there has never been information asymmetry with respect to quantity. It follows that reported and true output must be identical. Thus, the observed treatment effect with respect to q can unambiguously be attributed to a change in farmers' input use.

⁸ Trust levels were measured before and after the treatment. The variable is constructed in the same way as baseline trust.

While q increased, θ was not affected by the intervention. During the intervention, when quality was verifiable through the independent laboratory, we know with certainty that reported θ and true θ must be identical. If Vinamilk had cheated before the intervention and stopped doing so when the third-party testing started, we would have been able to identify a discontinuity (jump) in the reported average quality. We do not observe such a discontinuity in the data.

But before we can infer that Vinamilk did not underreport in the period before the intervention, we need to rule out a possible alternative explanation for the missing discontinuity: Vinamilk could have stopped cheating the farmers in the run-up to the intervention, as soon as they learned about its design and the fact that it would include third-party verification of the company's testing results. In this case, Vinamilk would have ceased underreporting at a much earlier point in time (that is, not covered by our dataset) to avoid providing evidence of cheating. This possibility, however, can be ruled out, because Vinamilk had already started providing production data (quantity and quality) at a very early stage of our cooperation, before we actually discussed the nature of the specific intervention. Hence, we had already received data at a time when Vinamilk could not anticipate that we were planning to look into independent quality verification. This precludes the possibility that the company provided us with "tailored" data to mask strategic underreporting of quality. Also the mere fact that the company agreed to this intervention can be interpreted as a sign that Vinamilk did not cheat on quality reporting prior to the experiment.

Moving Toward a First-Best Scenario in the Supply Chain

Putting these pieces of evidence together, we conclude that the company has not been deliberately underreporting milk quality and price, neither before nor during the intervention. Apparently, the company played fair but the supply-chain architecture prevented it from sending a credible signal of its fairness to the farmers, who in turn reacted with distrust. The finding that Vinamilk had not cheated the farmers has strong implications for the distribution of gains from third-party monitoring. As the company did not behave opportunistically in the first place, it also did not accrue any information rents. Thus, in a situation in which the principal plays "fair" but is unable to send a credible signal, third-party verification can restore a first-best scenario, increasing the welfare of both actors in the supply chain, sellers and buyer—while farmers benefit from unlocked productivity reserves, the company's per-unit transaction costs decrease if procuring from farmers who are more productive in a situation with symmetric information.

The distribution of gains is also driven by the stage of development of the supply chain. In the baseline scenario, without third-party contract enforcement, the company holds an informational advantage that can be exploited to capture additional gains. Moving toward a more efficient market with symmetric information shifts the distribution of gains in favor of farmers and hence—from their point of view—a desirable outcome.

Generally, for a rational buyer to refrain from cheating, expected benefits from underreporting quality should be lower than expected costs. Such costs could arise from two sources: first, in the form of forgone operational profits from farmers' suboptimal milk output (as we could show), and second, from the expected damage when cheating is detected. Reputational damage in particular could be severe for Vinamilk, given the company's size and the fact that it has established several high-profile brands in the national market. But this may not be so obvious for farmers. While company decisionmakers have more information to assess expected costs of cheating, contracted smallholder farmers may underestimate Vinamilk's risk and damage of being caught. For example, farmers may find it unlikely that Vinamilk would be convicted for fraudulent behavior. Anecdotal evidence suggests that Vinamilk is perceived as a powerful and politically well-connected player, due to its history as a state-owned company.

Based on this argumentation, we cautiously conclude that for Vinamilk it is the dominant strategy to play fair, but that farmers may nevertheless form the belief that the company behaves opportunistically. It remains to be discussed why in this situation the company itself has not established an independent system of quality verification to signal its fair type, even though this could be profitable. One explanation could be that the credibility of any processor-driven initiative to increase transparency may be low. Duflo et al. (2012) showed for industrial pollution in India that—if incentives are not aligned—firms employ auditors who write favorable reports, actually understating pollution caused by the company. The current equilibrium of distrust that we find could probably be broken by a credible intervention from outside, such as by public research institutions—like in our experiment—or more generally by the government. A study by Olken (2007) found top-down monitoring to be relatively effective, even in an environment notorious for corruption. But governments do not necessarily need to undertake controls by themselves. As Yang (2008) has shown for import-tax fraud, governments can “hire integrity” from private firms, which is comparable to the independent laboratory in our case. The fact that no such attempt has yet been undertaken by the Vietnamese government may not surprise us, given that the local dairy industry is still emerging. Also in other sectors of developing countries,

such as health and education, market and policy failures are widespread phenomena (World Bank 2003).

Conclusion

Contracting has become a widely embraced approach to facilitate supply-chain relations between selling farmers and buying companies, especially in emerging markets for high-value agricultural products. Smallholders entering contractual relations with buyers of high-value products such as fruits, vegetables, meat, or milk often become highly specialized and derive a considerable income share from the output sold under contract. However, a harmful asymmetry of information occurs if product-quality attributes are observable to the buyer but not to the selling farmer. If buyers behave opportunistically and exploit this information asymmetry to increase their profit, output prices for producers are subject to risk, and expected prices are lower than in a situation with symmetric information. Farmers taking this into account will underinvest, that is, they may use suboptimal levels of input, which translates into lower output levels. This is a nondesirable outcome for both farmers and buyers.

In this study, we have shown that third-party contract enforcement can be one way to mitigate the adverse effects of information asymmetry. Conducting a field experiment with dairy farmers in Vietnam we found that the provision of third-party contract enforcement had a positive impact on input use (purchased fodder) and output levels (quantity of milk fat and total solid), ultimately translating into higher revenue and also higher household welfare for specific subgroups of the sample. While we carefully designed the intervention to retain the internal validity of the results, we are also facing some limitations. Given the design of our intervention, we cannot fully avoid positive contamination of the control group, and thus may actually underestimate the treatment effects. While the postexperiment survey suggests that the intervention (which relied on a threat of selective double-checking rather than a comprehensive surveillance scheme) provided effective protection against cheating on the side of the buying company, residual doubts remain in view of the surprisingly low uptake rate (execution of vouchers for independent milk-quality testing). An encouragement design, with double-checking of milk quality being compulsory for treatment farmers, would be one option to consider in follow-up research.

From the available data, we infer that the observed treatment effects can be fully attributed to a behavioral change of farmers, instead of a change in the

company's reporting strategy. It can also be concluded that, in this specific case, the company had not exploited the informational advantage when the contract was not yet enforced through third-party testing. Instead, the company was playing fair but could not credibly signal its type to the farmers due to the specific architecture of the supply chain. Hence, not only farmers can benefit from more transparency regarding quality assessment. If more output per farmer is generated, the per-unit transaction costs for the buying company are reduced.

Our results were obtained in an environment that is representative of the fast-growing Vietnamese dairy sector. The findings may also be transferable to other agricultural sectors, especially those where competition between buyers is low and information asymmetry exists. If quality attributes determine output price but testing requires costly equipment, independent monitoring helps to overcome problems associated with information asymmetry, whether this is for fat content in milk, sugar concentration in cane, or protein content in grains—in Vietnam and beyond. The impact of third-party enforcement on the distribution of gains from contract farming depends on the stage of market development.

Appendix

TABLE 11.A1 Summary statistics of selected variables by milk-collection center

Characteristic	MCC A (n = 113)	MCC B (n = 103)	MCC C (n = 86)	MCC D (n = 83)
HH characteristics				
No. of HH members	4.513 [1.536]	4.641 [1.514]	4.244 [1.255]	4.341 [1.399]
Age of HH head	45.46 [11.53]	44.66 [9.161]	47.61 [11.74]	47.38 [11.39]
Total HH income (VND)	74,192,179 [49,567,765]	82,514,741 [69,491,153]	67,618,558 [58,362,681]	73,970,047 [53,442,489]
Dairy income (VND)	45,968,059 [35,675,422]	53,551,420 [55,525,486]	44,313,419 [53,633,181]	52,171,927 [47,796,603]
Dairy production				
Herd size (heads)	7.611 [5.417]	8.194 [5.369]	7.744 [4.587]	6.398 [3.751]
Productivity per cow (kg)	4,051.6 [2,888.4]	4,925.9* [2,229.7]	4,477.3 [2,472.7]	n.a.
Average milk price (VND)	6,850.0 [275.6]	6,730.9** [294.7]	6,542.4*** [416.7]	6,671.4* [772.3]
Total solid (%)	12.63 [0.520]	12.50 [0.496]	12.35*** [0.427]	12.61 [0.641]
Milk fat (%)	3.980 [0.280]	3.907* [0.245]	3.862** [0.221]	4.074 [0.482]
Milk hygiene score	3.572 [0.368]	3.642 [0.205]	3.686** [0.162]	3.578 [0.465]

Source: Authors' own estimates.

Notes: HH = household; MCC = milk-collection center; VND = Vietnamese dong.

Mean values are shown with standard deviations in brackets; mean differences are tested for MCC B–MCC A, MCC C–MCC A, and MCC D–MCC C; probability = * <.1, ** <.05, *** <.01.

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