Contract farming and smallholder incentives to produce high quality: experimental evidence from the Vietnamese dairy sector

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Abstract

In emerging markets for high-value food products in developing countries, processing companies search for efficient ways to source raw material of consistent quality. One widely embraced approach is contract farming. But relatively little is known about the appropriate design of contracts, especially in a small farm context. We use the example of the Vietnamese dairy sector to analyze the effectiveness of existing contracts between a processor and smallholder farmers in terms of incentivizing the production of high quality milk. A framed field experiment is conducted to evaluate the impact of two incentive instruments, a price penalty for low quality and a bonus for consistent high quality milk, on farmers' investment in quality-improving inputs. Statistical analysis suggests that the penalty drives farmers into higher input use, resulting in better output quality. The bonus payment generates even higher quality milk. We also find that input choice levels depend on farmers' socio-economic characteristics such as wealth, while individual risk preferences seem to be less important. Implications for the design of contracts with smallholders are discussed.

1. Introduction

The rapidly increasing demand for high-value food products in developing countries is triggering important changes in traditional supply chains, which often involve smallholders (Reardon et al., 2009). Processors and wholesalers, who are looking for new and efficient ways to source high quality raw material, have widely embraced contract farming as one approach to coordinate supply chain relations (Birthal et al., 2005; Swinnen, 2009; Schipmann and Qaim, 2011). Production contracts can entail a broad variety of incentive instruments, such as input control, field visits, quality assessment, and incentive pay, all of which aim at maintaining high output quality (Hueth et al., 1999; Bellemare, 2010).

Empirical evidence on the degree and impact of smallholder participation in high-value markets is mixed. Some studies find that buyers prefer to contract larger farmers because of lower transaction costs (Key and Runsten, 1999). However, there are also examples where smallholders benefit from contract farming through better access to inputs and technology as well as higher and more stable prices (for a comprehensive review, see Minten et al., 2009). Small-scale farmers can have a comparative advantage in the production of labor-intensive goods. Yet, they may struggle to meet strict quality standards, especially when these require the use of special inputs or new production techniques (Swinnen, 2009). Given widespread constraints, smallholders may underinvest into their production, which can result in suboptimal quality from the point of view of buyers in high-value markets. Improved contracts could potentially help reduce transaction costs and provide new incentives for high-quality production. However, there is very little empirical evidence available on issues of contract design in a small farm context.

The available literature on contracts in agriculture focuses mainly on two questions: first, what determines contract choice (Ghatak and Pandey, 2000; Goodhue et al., 2004; Masakure

and Henson, 2005) and second, how do specific contract designs affect farmers' response once they have been contracted by a buyer (Hueth et al., 1999; Goodhue et al., 2010)? The second question has mostly been addressed in developed countries. For example, studies in the markets for processing tomatoes and wine grapes in the US have found that financial incentives can successfully influence production decisions and increase quality (Goodhue et al., 2004; Alexander et al., 2007). However, the empirical analysis of both, contract choice and performance under a specific contract type, can be confounded by selection bias, as most farmers choose only one type of contract, and this choice may be endogenous (Alexander et al., 2007). One way to avoid the endogeneity problem is the use of experimental methods to observe behavior under controlled conditions. In an early study, Bull et al. (1987) have experimentally tested various contracts. Wu and Roe (2005) have investigated different incentive schemes employed in contract agriculture using laboratory experiments with college students in the US.

We contribute to this literature through a framed field experiment carried out with farmers in a developing country. In particular, we are interested in the relationship between price incentives, input use, and output quality in contract arrangements. The experiment was conducted with a subject pool of smallholder dairy farmers in Vietnam. The Vietnamese dairy sector is a typical example of fast growing high-value markets in developing countries, where the quality of the raw material becomes increasingly important. For example, high content of psychrotrophic bacteria or antibiotics in the raw milk caused by poor herd management can increase processing costs (Claypool, 1984), while adulteration of milk along the supply chain can have adverse health effects for consumers, as the recent case of melamin-tainted milk in China has shown (Gale and Hu, 2009).

The Vietnamese dairy farmers that participated in our experiment produce milk under a contract with a large processing company. We have designed three input decision games with

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varying financial penalties and a bonus to investigate (i) whether the incentive structure in the existing contract effectively incentivizes input use to boost output quality and (ii) whether, additionally to the financial incentive, risk preferences and wealth levels drive farmers' input decisions. Based on the findings we discuss ways to improve existing production contracts to the benefit of both smallholders and processing companies.

The remainder of this article is organized as follows. After presenting some background information on contract farming in the Vietnamese dairy sector in Section 2, the experimental approach is described in Section 3. The empirical strategy and descriptive statistics are outlined in Section 4, followed by the presentation and discussion of regression results in Section 5. Section 6 concludes with some policy and research implications.

2. Background

The market for dairy products in Vietnam has a couple of features that are typical for emerging high-value markets in developing countries. First, it has high growth rates. Only two decades ago, the consumption of milk and dairy products was almost nil in Vietnam (and other Asian countries) due to cultural practices and low income levels. But economic growth, urbanization, and the spread of Western lifestyles went along with a change in food consumption patterns, causing a surge in the demand for milk. Today's per-capita consumption of milk has reached 15 kg per annum in Vietnam, which is still only about 8 percent of what is consumed in the US or Europe (USDA, 2011).

Second, the Vietnamese dairy sector is dominated by local processing companies, which currently import large quantities of powdered milk from overseas to satisfy local demand. However, increasing quantities are produced domestically, especially by small-scale farmers. Fresh milk production in Vietnam has more than quadrupled between 2001 and 2009, now

meeting about 20 percent of domestic consumption (USDA, 2011). Third, the quality of the raw material is very important for processing companies that mainly sell drinking milk, yoghurt, ice cream, and infant formula. Powdered milk from the world market is a standardized product, which is purchased in large batches with known and predictable quality. In contrast, local farmers produce small quantities of raw milk, which is subject to fluctuation in quality, depending on various factors.

To ensure a constant supply of raw material, dairy processors in developed and developing countries do not source raw milk from spot markets but through contract farming arrangements (e.g., Royer, 2011). Until recently, it was quite costly to assess milk quality for each farmer, especially when only small quantities are involved. Today, cheaper quality testing devices allow dairy processors to assess quality individually for each farmer, which is a key requirement for traceability, quality management, and incentive pay. The question is as to how farmer-specific quality data can be used to design incentive-compatible contracts. In Vietnam, the largest dairy processing company utilizes the data to employ financial penalties, punishing the delivery of poor quality. A base price is paid for milk of the highest quality. For lower quality, the company adjusts the price downwards. Milk quality is a function of farmers' input use and environmental factors. Hence, dairy farmers face the challenge to maximize profit by choosing the right input mix to produce a specific quality. This decision involves some degree of risk, because environmental factors are not perfectly predictable. This situation is also the starting point for our framed field experiment.

The current design of the contract in Vietnam has evolved over time. The instrument of financial penalty has to be seen in the context of the existing market structure. For most dairy farmers, the processing company is the only realistic marketing option. The raw milk is perishable, and production involves a high degree of asset specificity, so that farmers' bargaining power is limited. Here, we are not primarily interested in analyzing whether or not

the pricing scheme in the existing contract is fair. Rather, we want to understand how it affects farmers' input use and their incentive to produce high quality. Various studies on the consequences of oligopsony power suggest that biased pricing can affect farmers' investment behavior (Gow and Swinnen, 1998; Young and Hobbs, 2002; Vukina and Leegomonchai, 2006; Swinnen and Vandeplas, 2010). This can include both short-term investments into variable inputs and also longer-term investments into technological upgrading.

3. Experimental approach

3.1 Experimental design

We have designed a framed field experiment, which involves five repeated costly choices between three gambles. Specifically, the subjects (dairy farmers) choose input levels mimicking risky day-to-day production decisions familiar to them from their own farm. In the game, each subject hypothetically owned one cow that produced a fixed quantity of milk (10 kg per day) with varying quality. Milk quality is graded in five levels, A to E, each yielding a different price. The base price chosen for the experiment was 7,000 Vietnamese Dong (VND) per kg for quality A. Lower quality grades were associated with severe price deductions, as shown in Figure 1. The lowest grade, E, only fetched a price of 2,000 VND per kg.[1]

The payoff depended on the subjects' choice of input quantity and a subsequent stochastic move of nature, which could take two states, *good* or *bad*, representing benign or malign production conditions. Production conditions affect quality. For instance, under malign conditions, output quality is lower than under benign conditions at the same level of input.

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[1] The prices for the quality grades resemble those that farmers actually received from the dairy processor at the time when the experiment was conducted (July 2009). All prices, costs, and revenues in the game are in VND. The official exchange rate in July 2009 was 1 USD = 17,522 VND.

Likewise, input quantity affects quality. The input, which subjects could purchase, has risk-reducing characteristics such that it dampens the negative effect of malign production conditions. This is a realistic assumption for many inputs used in dairy farming. For example, if adverse weather conditions affect farmers' own forage production, purchased fodder can help to reduce negative impacts on milk output. Purchased mineral fodder and vaccinations can help to reduce or avoid negative effects of animal disease. More broadly, the draw of nature represents a stochastic component affecting potential outcome, a feature inherent in most agricultural production processes. Since the dairy processing company wants to buy milk of high quality, it has an interest in farmers using sufficient quantities of input. It is important to note that the purchased input is sold by a separate, specialized company, not by the dairy processor, so that there is no conflict of interest. The experiment comprises three treatments, which are described in the following.

Baseline treatment

The baseline treatment is called such, because it reflects the existing contract between dairy farmers and the processing company. The protocol comprised the following steps:

1. At the beginning of the game ($t = 0$), before the first decisions were made, each subject received a random initial endowment ϑ_i, with three possible levels $\vartheta_i =$ (25,000, 30,000, 35,000).

2. Subjects had to take a costly production decision, namely choose how many bags of input to purchase using the initial endowment ϑ_i. The input, framed as a special type of mineral fodder, could be purchased in quantities of either zero, one, or two bags $q = (0, 1, 2)$ at unit price $p = 10,000$. While the costs ($C_{var} = pq_i$) associated with the choice of bags mimic variable costs of production, subjects also faced fixed costs

$C_{fix} = 20{,}000$ for other types of fodder, veterinary service etc. Accordingly, the total

cost of production was $TC_i = (pq_i) + C_{fix}$.

3. Nature, which can take two possible states, $v = (good, bad)$, was randomly determined

    by a draw from an urn. The probability of nature taking the state *good* was $p = 0.75$,

    while the probability of state *bad* was $(1 - p)$.

4. Each subject i realized a payoff (profit) Π_i, which depended on the individual input

    decision and the subsequent stochastic move of nature. Π_i in the first round was

    determined according to

$$\Pi_{i1} = \vartheta_i + (TR_i - TC_i) = \vartheta_i + (\text{TR}_i - (pq_i) - C_{fix}) \tag{1}$$

where TR_i is the total revenue realized, which is a function of input choice q and the

state of nature v, $TR_i = f(q, v)$. The possible profits for each input choice are depicted

in Table 1.

As the game lasted $k = 5$ rounds, steps 2, 3, and 4 were repeated five times leading to

total payoff Π_{iK} as follows:

$$\Pi_{iK} = \vartheta_i + \sum_{ik}^{K}[\text{TR}_{ik} - (p * q_{ik}) - C_{fix_k}] \tag{2}$$

The two gambles that are reflected by choice 1 and 2 stochastically dominate the

gamble behind choice 0 (Table 1). This implies that the relatively small revenue due to poor

output quality under choice 0 cannot be overcompensated by low initial input costs. In other

words, some minimum use of input is necessary for profit maximization. Assuming that

subjects maximize expected profit, the stochastic dominance effectively narrowed down the

decision problem to a choice between two gambles (1 or 2 bags of input). The payoff

distributions of choice 1 and 2 in the baseline treatment have the same expected value (EV =

27.5) but different standard deviations (SD). For choice 1, the SD is three times higher (12.99)

than for choice 2 (4.33). We expect risk-averse subjects to purchase two bags of fodder, in

order to avoid the risk of a low payoff when the state of nature is *bad*. Accordingly, the incentive effect underlying the pricing scheme stems from farmers' potential preference for a lower SD of payoffs.

We now focus on the two additional treatments, in which the incentive structures were changed. Specifically, we altered the underlying pricing scheme that defines the relation between milk quality and price.

Counterfactual treatment

The counterfactual treatment was not designed as a ready-to-implement alternative to the pricing scheme currently used in the Vietnamese dairy industry. Rather, it aims at pinning down the effectiveness of the baseline incentives by showing what the outcome would look like under a modified pricing scheme. That is, we want to identify whether the financial penalty currently observed and reflected in the baseline treatment works effectively to increase input use and thus milk quality. As the company uses a country-wide standardized contract, there is no real-world variation in pricing schemes, so that this analysis would not be possible based on observational data.

The pricing scheme we chose for the counterfactual treatment resembles the one in the baseline, with the only difference that the price penalty for poor quality is less harsh. Specifically, the deduction in price for quality level D is smaller than in the baseline treatment (Figure 1). As a result, the relative moments of the payoff distributions change. While in the baseline, the EV was the same with 1 or 2 bags of input, in the counterfactual treatment, the EV is higher with 1 bag (Table 1). Hence, choosing 2 bags over 1 bag to keep the SD lower now requires giving up some EV. With this modification, any differences in input choice

between the baseline and counterfactual treatments can be attributed to the stronger price penalty in the baseline scheme.

Bonus treatment

Next, we introduce an additional positive financial incentive. In the bonus treatment, a reward was paid for constantly high input use and resulting excellent output quality. The positive incentive (bonus payment) used here addresses the dissatisfaction with the existing pricing system, which farmers expressed during interviews carried out before the experiment. Farmers consider it imbalanced that there are harsh deductions for poor quality, but no rewards for excellent quality. In the bonus treatment, we used the baseline pricing scheme, but announced and paid an extra 10,000 VDN when A quality milk was delivered in two consecutive rounds. This changed the incentives fundamentally. While in the baseline scenario only a stick in the form of price deductions was employed, in the bonus treatment we added a carrot in the form of a conditional bonus payment.

The comparison of choices in the bonus and baseline treatments reveals if the bonus encourages subjects to choose higher input levels. We acknowledge that this comparison involves a change in more than one moment of the payoff distribution (Table 1), which makes it more difficult to identify the exact cause of observed behavioral change. An alternative would have been to raise the base price, but explorative discussions with company representatives revealed that this would not be a realistic option. On the other hand, a conditional bonus payment might be considered in reality. It should be noted that the level of the bonus chosen in the experiment is probably higher than what a company would be willing to pay. Given the limited number of subjects and treatments, we decided to calibrate the bonus

at an upper boundary. If subjects are not driven into higher input use by a bonus payment of this size, smaller premiums would probably be even less effective.

Additional details on design

We close this subsection on experimental design by mentioning three additional points. First, a between-subject design was implemented, implying that each subject was exposed to one treatment only. Hence, the choice task was identical in each of the five rounds, ensuring that no treatment ordering effects confound the analysis (Harrison et al., 2005). Second, in designing the experiment we took into account that presentation of highly abstract and complex decision tasks may confuse subjects with limited numerical skills (Dave et al., 2010). The strong framing in terms of dairy farming and the comparably low complexity of the choice between gambles with identical probabilities leads to a simple task interface, which in our view is appropriate for the subject pool of Vietnamese dairy farmers.

Third, while Tanaka et al. (2010) conducted experiments in Vietnam that included a series of lotteries involving both losses and gains, we decided to use lotteries with gains (or zero payoff) only. This is comparable to Lybbert's (2006) experiment with Indian farmers and other studies where subjects are endowed at the beginning of the experiment and may lose only little money of this endowment in a given round. While this may not provide exactly the same incentive structure as in real-world situations, where farmers may incur losses after risky decisions, we note that it is the relative treatment effect that we are mainly interested in. This should be unaffected, because we compare treatments that all do not allow losses beyond the initial endowment.

3.2 Sample selection and sample characteristics

For the experiment we collaborated with Vietnam's largest dairy company. This company provided a complete list of 402 dairy farmers currently contracted in Long An and Tien Giang, two provinces south of Ho-Chi-Minh-City (HCMC). These provinces are representative dairy producing regions in Vietnam. More than two-thirds of Vietnam's total dairy population is held in the greater HCMC area (USDA, 2011). Milk production takes place on small farms. The average herd size in the sample is 7.8 heads, including cows, heifers, bulls, and calves. The animals are mostly cross-breeds of high-yielding Holstein-Friesian and local races. They are held in cowsheds year-round where they are fed with a ration of own-produced forage and purchased components such as concentrate and mineral fodder. The milk yield per cow (4,000 to 4,500 kg per annum) is considerably lower than in developed countries, mostly due to poor herd management practices and suboptimal feeding.

Milk produced on the farms is not directly delivered to dairy plants in HCMC but is channeled through milk collection centers (MCCs) located in the vicinity of the farms. Roughly 100 farmers are grouped into an MCC, usually operated on commission by a private entrepreneur. Three of the four MCCs in the target region are geographically clustered, while the fourth is located around 50 km north-west of this cluster. We found significant differences in terms of some farm characteristics (e.g., herd size, milk quantity and quality) between producers delivering to different MCCS, which may be due to unobservable factors. For the three geographically clustered MCCs, farmers can choose freely where to deliver their milk. Anecdotal evidence suggests that this decision does not only depend on distance, but also on soft factors such as trust towards the manager of a particular MCC.

Employing factorial design, we generated treatment groups with the same average characteristics before implementing the experiment. We decided to pool farmers from both provinces. Out of the population of 402 farmers, we randomly sampled 205, who were then

randomly assigned to one of the three treatments (baseline, counterfactual, and bonus). All sample farmers were visited in their homes for a comprehensive household survey using a structured questionnaire prior to the experiment (see below).

3.3 Implementation and procedures

We chose a large public gathering hall in the city of Long An as the venue for the experimental sessions. Long An is the capital city of Long An province and is located 50 km south of HCMC. All randomly selected farmers received a written invitation one week before the experiment was conducted. Farmers located close to the venue used their motorcycles to participate in the experiment, while a bus shuttle was installed for participants located further away. The public gathering hall was sufficiently large to allow wide spacing between participants. To ensure privacy during the decision-making process, the tables were equipped with voting boxes high enough to separate the subjects from each other.

The experiment comprised six sessions, which were conducted over the course of three days (one morning and one afternoon session on each day). Each farmer only participated in one of these sessions. Out of the six sessions, two followed the baseline, two the counterfactual, and two the bonus treatment protocol. In total, 185 of the 205 invited farmers showed up at the venue; hence, attrition was only 9 percent, suggesting high representativeness of the participants. Each session consisted of registration, instructions with trial rounds, five consecutive rounds of decision making, a short post-experiment survey, and payment. The average number of subjects per session was 31.

At the beginning of the first round, farmers received an envelope containing the initial endowment in cash. This money was used to make the input purchase decision by inserting the cost for the chosen input quantity into an envelope, which was then collected by the

experimenters. Subsequently, the state of nature was determined by drawing colored chips from an urn that contained three blue and one red chips, representing *good* and *bad* conditions, respectively. The probability of the good event was kept constant at $p = 0.75$ and was known to all subjects. There were two different controlled sequences (one for each session) of events, which were repeated in each of the three treatments. In the first sequence, events were drawn in the following order: *good-bad-good-good-bad*. In the second sequence, the order was slightly different: *good-good-bad-good-bad*. While the sequence of draws was random to the subjects, it was not random to the experimenters in that it was determined prior to the experiment.

Controlling the sequence of events had two major advantages. First, with only five rounds per session, and the probability of a good event of $p = 0.75$, purely stochastic on-site-draws of nature could have led to a situation where only very few or no bad events happened in a specific session. Through pre-drawn sequences, we could ensure a certain number of bad events and thus variation in the five-round spell. Second, we increased comparability between treatments. Given that the series of events was the same in each of the treatments, the treatment effect can be identified by comparing the outcomes without controlling for differences in realizations of states of nature. This would have been necessary if the realization of events was truly random and subjects maintained a heuristic understanding of probabilities (e.g., Hill and Viceisza, 2011).

After the state of nature was determined, the individual payoffs were computed based on the revenue resulting from farmers' choice and the cost of production. The resulting cash payoffs were placed in an envelope and redistributed to the individual farmer. Each round's payoff and the sum of payoffs from previous rounds could then be reinvested by purchasing input bags at the beginning of the following round. On average, farmers earned 129,800 VDN

through participating in the experiment (varying from 90,000 to 150,000 VDN), which is equivalent to two daily wages for unskilled labor.

4. Empirical strategy and comparative analysis

Given the random assignment of the treatment status, the average treatment effects are explored by (a) comparing mean input levels between the treatments and (b) regressing chosen input quantities on treatment dummies and other covariates, including socio-economic characteristics collected in the household survey.

4.1 Survey data

The household survey was conducted in April/May 2009, two months before the experiment was run. Demographic and socio-economic data were collected, including age, gender, education, and income-generating activities of household members, as well as asset ownership. Moreover, information on individual characteristics like altruism, trust, time preferences, and risk preferences was elicited. To capture altruism and trust levels, we included questions on whether interviewees gave money or would lend money to other farmers. Further, respondents had to rate the statement "the dairy company is trustworthy" on a Likert scale.[2] Time preferences were captured as interest rates at which farmers were willing to postpone receiving a certain amount of money for three months. To elicit risk preferences,

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[2] Interviewees had to rate this statement on a four-point 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 "very much agree", and 0 otherwise.

we included a Binswanger (1980) lottery in which interviewees had to choose between risky gambles.[3]

4.2 Randomization

The random assignment of experimental subjects led to treatment groups which were generally balanced with respect to most demographic and socio-economic variables (Table 2). However, subjects in the bonus treatment tend to have less experience in dairy farming than their peers in the baseline and counterfactual groups. We also observe that subjects in the baseline group were more trustful but less wealthy than subjects in the counterfactual treatment. Despite these slight differences (which are random and non-systematic), the random assignment led to comparable treatment groups.

4.3 Comparative analysis

Mean values of the choice variable in the experiment (number of purchased input bags) are shown in Table 3 for the three treatments. The average choice over five rounds was 1.652 bags in the baseline treatment, while it was lower in the counterfactual treatment and higher in the bonus treatment. All differences are statistically significant. These comparisons imply two important but preliminary results in terms of the contract designs we are testing: First, the difference in input choice between the baseline and counterfactual scenario shows that the baseline pricing scheme, which mimics the financial incentives currently provided by the processing company, is effective in driving farmers into higher input use. Second, the average

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[3] Interviewees had to choose between five gambles with increasing SD of the payoff distributions (the probability of winning the higher prize was the same in each gamble). Accordingly, the variable takes the value 1 if farmers were risk averse and higher values if farmers were less risk averse (with 5 as upper bound).

input quantity increases significantly when the penalty for low quality underlying the baseline specification is complemented with a bonus for consistent high quality.

Further, the results provide insights into the risk preferences of our subjects. The payoffs in the baseline treatment were calibrated such that risk-neutral subjects would be indifferent between choosing 1 or 2 bags of input (see Table 1 and subsection 3.1). Consequently, the mean choice should asymptotically converge to 1.5, given enough observations. However, the observed mean choice in the baseline treatment is significantly larger than 1.5 (at 1 percent error rate), suggesting that farmers were not indifferent but preferred to choose 2 bags (which represents the gamble with lower SD of the payoff distribution).

The results of the counterfactual treatment underpin these findings. In this treatment, risk-neutral subjects would be expected to prefer buying 1 bag to realize the highest EV of the payoff distribution. However, we observe a significantly larger average choice (1.41). Subjects chose more bags, giving up some EV for a lower SD, pointing to a considerable level of risk aversion.

These descriptive results are preliminary due to potentially confounding factors such as the statistically significant differences in characteristics between treatment groups or round and session effects. Therefore, we employ a regression framework, control for such confounding factors. Moreover, with suitable regression model specifications we can investigate potential mechanisms driving the observed input decisions.

4.4 Regression analysis

For the regression analysis we use input choice as dependent variable. By design this is restricted to integers between 0 and 2. To account for the left and right censoring of the dependent variable, we employ a Tobit model with the following specification:

$$y = \propto +\beta T + \gamma X + \delta XT + \vartheta Z + \sigma ZT + \varepsilon, \tag{3}$$

where the dependent variable y is the number of purchased input bags in a given round, T is a vector of treatment dummies, and X is a vector of control variables. X includes experiment-specific variables such as round and session dummies, as well as household and individual characteristics for which we found differences in mean values between treatment groups.

In subsequent specifications, we introduce a vector Z, which comprises additional socio-demographic variables. Z can also help to explain some of the mechanisms that may drive farmers' input purchase decisions. We expect risk preferences and wealth to play a role. Selected variables of X and Z are also interacted with T. The interaction terms allow us to analyze heterogeneous treatment effects. ε is the error term.

To exploit the panel structure of the experimental data, with several rounds of decisions, we use a random effects longitudinal Tobit model. This takes into account that each subject was only exposed to one treatment, that is, the treatment effects can only be identified across groups, not across time (experimental rounds).

5. Regression results

The regression results are depicted in Tables 4 and 5.[4] Model (I) in Table 4 is a simple specification, which only includes the treatment dummies for the counterfactual and bonus treatments (the baseline treatment is the reference). The treatment effect is negative (positive) and significant for the counterfactual (bonus) dummy. This confirms the results from the comparative analysis, namely that the harsh price penalty for low quality milk in the baseline

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[4] The number of observations varies slightly between different model specifications. The reason is that in some cases, the person participating in the experiment was a household member other than the respondent in the preceding survey (e.g., the survey respondent was sick at the time of the experiment). For regressions with individual-specific covariates from the survey, these cases had to be dropped.

increases input use, and that this effect can be further strengthened through an additional bonus.

In model (II) we add a set of binary variables to control for session (morning or afternoon) and round-specific factors. The subjects' ability to understand the rules of the game may also play a role. In the short post-experimental survey, farmers had to answer a simple question of understanding. Based on this, we constructed the dummy 'misunderstanding of instructions', which takes the value 1 if this question was not correctly answered. Further, we control for the previously discussed differences in farm and household characteristics between treatment groups.

The results for model (II) in Table 4 show that the treatment effects remain robust; for the bonus treatment, the effect even increases in magnitude. The session effect is not significant, although the interaction terms suggest that the impact of the bonus was lower in afternoon sessions. The coefficients of the dummies for later rounds are positive and significant, implying that farmers' willingness to invest in inputs increased over time. This may be due to learning effects. End-of-game effects may also play a role, although farmers did not know the exact number of rounds to play before the game actually ended. The results further show that subjects who had difficulties to understand the rules of the game purchased significantly fewer bags of input, which is plausible. The positive coefficient of concentrate use shows that farmers who purchase more fodder in reality also purchased a larger number of bags in the experiment. This is a welcome finding, as it confirms that the experimental framing was realistic. Finally, subjects with more experience in dairy farming tend to purchase more input bags.

In model (III) we include additional variables that capture household demographic factors and possible MCC effects. Again, the treatment effects remain robust. Being female and being older seem to have negative impacts on input purchases. The risk literature suggests

that women and older individuals often tend to be more risk averse (Eckel and Grossman 2008). Hence, our results may surprise, given that the input in the experiment is risk reducing. One possible explanation is that the mineral fodder is regarded as a new and risky technology by some. Two of the MCC dummies have significant effects, which we attribute to unobserved factors (see subsection 3.2).

Role of risk preferences

The comparative analysis showed that some subjects preferred gambles with lower SD, even giving up higher EV of an alternative gamble. This suggests that farmers are risk averse. To explore further whether risk aversion explains the observed behavior, we specify model (IV) where we include the risk proxy variable from the household survey, which we also interact with the treatment dummies. The estimation results are shown in Table 5. Risk preferences do not seem to affect input choice significantly. One possible explanation may be that the data variation for risk preferences is relatively low. In the survey, 60 percent of the respondents opted for the least risky gamble, only 5 percent opted for the riskiest alternative.

Role of wealth

Wealth levels may also explain farmers' input choices. We differentiate between wealth levels external and internal to the experiment. Wealth external to the experiment is captured through the total value of assets owned. This is based on the household survey and was already included in previous model specifications. Wealth internal to the experiment is captured by lagged profit, that is, profit realized in the previous round.[5] Both measures of wealth are not correlated and hence are included simultaneously in models (V) and (VI).

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[5] For the first round in each experimental session, we use the stochastic initial endowment.

Table 5 shows that assets have a small negative effect on input choice, while lagged profit has a positive and significant effect. Subjects who realized higher profits in the previous round tend to invest more. This is probably related to liquidity considerations: subjects purchase more inputs when they are less financially constrained. Interacting lagged profit with the treatment dummies shows that the positive effect of internal wealth disappears in the bonus treatment.

6. Conclusion

Modern and more integrated supply chains for high-value agricultural products are gaining in importance in many developing countries. These supply chains often involve contractual arrangements between agribusiness companies and farmers. Whether smallholder farmers can successfully participate and benefit from contract schemes depends on many factors. One important question is how well they meet specific quality requirements. If smallholders have a tendency to produce lower quality, companies will search for alternatives, such as sourcing raw material from larger farms or engaging in primary production themselves. This could entail further marginalization of smallholders. Farmers' behavior and performance depend on abilities and incentive structures, which can be influenced through contracts. Yet, relatively little is known about suitable contractual designs in a smallholder context.

We conducted a framed field experiment with Vietnamese dairy farmers to better understand the relationships between contractual pricing schemes, input use, and output quality. The experimental data were complemented with socio-economic data from a household survey. The production contract, which is currently used in Vietnam, builds on strong price penalties for lower quality milk. Our results confirm that this is an effective

instrument to incentivize higher input use among farmers. Providing a bonus payment for consistent high quality further increases input use. But obviously, a bonus payment would entail additional costs for the buying company. The amount of bonus in the experiment was effective but relatively large. A somewhat lower bonus or some form of targeting may also work potentially. In the end, it remains an empirical question whether the marginal benefits for the company can over-compensate the marginal costs of the bonus payment under real-world conditions. This depends on the supply response of farmers and the value that the company attributes to increases in quality, which is hard to analyze in framed field experiments.

A contract design that relies only on price penalties as an incentive to produce high quality is typical for a monopsonistic situation. In Vietnam, dairy farmers hardly have options to sell their milk outside the contract. They also incur relationship-specific investments, so that their bargaining power is limited. This may be a favorable situation for the buying company in the short run. But there could be a downside from a more dynamic perspective. If farmers are threatened into high input use by harsh price penalties, their cost of production may increase. In agricultural markets where margins for sellers are low, heavily investing in variable risk-reducing inputs may potentially strain the capacity to invest in longer term upgrading of the enterprise. This is especially true among smallholder farmers, who are often liquidity constrained. Thus, through harsh negative incentives, the contracting company might strangulate future growth of its supplier base. This may result in stagnating productivity among contract farms, obstructing potentials for reduced transaction costs in the future.

There is clearly scope for further research, investigating the relations between price and other contractual incentives, risk aversion, investment in variable inputs, and long-term growth and viability of farming enterprises. Some of these issues may be addressed through randomized controlled trials.

Acknowledgements

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Table 1: Payoff (profits) distributions by treatment (in '000 VND – quality grades in parentheses)

Baseline treatment		Choice (number of bags of input)		
		0	1	2
State of nature	good (p=0.75)	15.00 (D)	35.00 (B)	30.00 (A)
	bad (1-p)	0 (E)	5.00 (D)	20.00 (C)
Payoff distribution moments	**EV**	11.25	27.50	27.50
	SD	6.50	12.99	4.33

Counterfactual treatment		Choice (number of bags of input)		
		0	1	2
State of nature	good (p=0.75)	25.00 (D)	35.00 (B)	30.00 (A)
	bad (1-p)	0 (E)	15.00 (D)	20.00 (C)
Payoff distribution moments	**EV**	18.75	30.00	27.5
	SD	10.83	6.70	4.33

Bonus treatment		Choice (number of bags of input)		
		0	1	2
State of nature	good (p=0.75)	15.00 (D)	35.00 (B)	30.00 (+ 10.00 bonus after 2 rounds) (A)
	bad (1-p)	0 (E)	5.00 (D)	20.00 (C)
Payoff distribution moments	**EV**	11.25	27.50	31.25
	SD	6.50	12.99	7.77

Notes: The unit prices per kg of milk are: A: 7,000; B: 6,500; C: 6,000; D: 3,500 in the baseline and bonus treatment and 4,500 in the counterfactual treatment; E: 2,000. The payoff per round is the profit from milk production, $\Pi = 10$ kg milk * unit price – fixed costs – variable costs. For example, if in the baseline treatment 1 bag of input is chosen and nature takes the state *good*, the quality would be B; the resulting profit is $\Pi = 10$ kg milk * 6,500 – 20,000 – 10,000 = 35,000.
Source: Own calculation

Table 2: Sample means of selected characteristics by treatment

Variables	Treatment			Mean differences		
	Basline (T1)	Counterf. (T2)	Bonus (T3)	(T1)-(T2)	(T1)-(T3)	(T2)-(T3)
Demographic variables						
Age (years)	42.83	42.68	45.30	0.150	-2.464	-2.614
	[9.466]	[12.04]	[11.88]	[1.977]	[1.937]	[2.149]
Gender (1=female)	0.197	0.200	0.109	-0.003	0.087	0.091
	[0.401]	[0.403]	[0.315]	[0.073]	[0.064]	[0.065]
Education (years of schooling)	8.800	8.650	8.094	0.150	0.706	0.556
	[4.372]	[2.748]	[3.079]	[0.667]	[0.676]	[0.525]
Economic and dairy production variables						
Total assets (100 USD)	26.264	28.110	27.553	-1.847*	-1.290	0.557
	[0.861]	[1.025]	[1.107]	[1.339]	[1.415]	[1.514]
Total HH income ('000 VND)	84.399	86.455	78.330	-2.056	6.069	8.124
	[53.965]	[55.144]	[70.493]	[9.961]	[11.360]	[11.450]
Dairy income ('000 VND)	58.050	62.738	46.812	-4.689	11.238	15.926
	[51.256]	[52.439]	[47.779]	[9.588]	[89.679]	[9.187]
Experience in dairy farming (years)	4.850	4.483	3.766	0.367	1.084*	0.718
	[3.434]	[2.514]	[2.395]	[0.549]	[0.529]	[0.441]
Dairy herd size (heads)	8.417	7.950	7.016	0.467	1.401	0.934
	[5.328]	[4.300]	[4.463]	[0.884]	[0.881]	[0.788]
Concentrate use (kg/cow*day)	7.950	7.475[a]	7.802	0.475**	0.148	-0.327
	[1.826]	[1.437]	[2.153]	[0.302]	[0.363]	[0.334]
Affiliation to milk collection center						
Delivering milk to MCC 1	0.311	0.233	0.266	0.078	0.046	-0.032
	[0.467]	[0.427]	[0.445]	[0.081]	[0.082]	[0.078]
Delivering milk to MCC 2	0.279	0.233	0.344	0.045	-0.065	-0.110
	[0.452]	[0.427]	[0.479]	[0.080]	[0.083]	[0.082]
Delivering milk to MCC 3	0.197	0.267	0.219	-0.070	-0.022	0.048
	[0.401]	[0.446]	[0.417]	[0.077]	[0.073]	[0.078]
Delivering milk to MCC 4	0.197	0.267	0.172	-0.070	0.025	0.095
	[0.401]	[0.446]	[0.380]	[0.077]	[0.070]	[0.074]
Preferences						
Risk preference (1-5 with 1 being most risk-averse)	1.850	1.650	1.906	0.200	-0.056	-0.256
	[1.191]	[1.087]	[1.231]	[0.208]	[0.218]	[0.209]
Patient (discount rate <3.5%; 1=y)	0.383	0.267	0.359	0.117	0.024	-0.093
	[0.490]	[0.446]	[0.484]	[0.086]	[0.088]	[0.084]
Dairy company is trustworthy (1=y)	0.590	0.533	0.563	0.057	-0.029	-0.028
	[0.496]	[0.503]	[0.500]	[0.091]	[0.090]	[0.089]
Trust proxy (money lent to farmers; 1=y)	1.400	1.300	1.170	0.100	0.228***	0.128***
	[0.068]	[0.060]	[0.048]	[0.091]	[0.082]	[0.076]
Altruism (money given to farmer; 1=y)	1.150	1.217	1.109	-0.0667	0.0406	0.107
	[0.360]	[0.415]	[0.315]	[0.071]	[0.061]	[0.066]
Observations	61	60	64			

Notes: Standard deviations in brackets.
*** p<0.01, ** p<0.05, * p<0.10.
[a] One outlier was omitted for this variable (n=59).
Source: Own calculation

Table 3: Mean input choice by treatment

	Treatment			Mean differences		
	Baseline (T1)	Counterfactual (T2)	Bonus (T3)	(T1)-(T2)	(T1)-(T3)	(T2)-(T3)
Number of bags	1.652	1.410	1.769	0.242***	-0.116**	-0.359***
	[0.565]	[0.591]	[0.471]	[0.047]	[0.042]	[0.043]
Observ. (NK)	305	300	320			
Rounds (K)	5	5	5			
Number of individuals (N)	61	60	64			

Notes: Standard deviations in brackets.
*** p<0.01, ** p<0.05, * p<0.10.
Source: Own calculation

Table 4: Estimation results (random effects longitudinal Tobit model)

	(I)		(II)		(III)	
	Coefficient	SE	Coefficient	SE	Coefficient	SE
Treatment variables						
Counterfactual treatment T2 (1=y)	-0.719***	[0.140]	-0.652***	[0.190]	-0.609***	[0.217]
Bonus treatment T3 (1=y)	0.438***	[0.149]	1.432***	[0.243]	1.502***	[0.285]
Variables X						
Session dummy (1=afternoon)			0.325	[0.205]	0.204	[0.227]
Session * T2			0.193	[0.278]	0.324	[0.317]
Session * T3			-1.108***	[0.312]	-0.615*	[0.372]
Round 2 dummy (1=y)			-0.005	[0.173]	-0.030	[0.193]
Round 3			0.216	[0.176]	0.202	[0.197]
Round 4			0.392**	[0.179]	0.418**	[0.202]
Round 5			0.454**	[0.180]	0.519**	[0.203]
Misunderstanding of instructions (1=n)			-0.809***	[0.165]	-0.818***	[0.198]
Dairy farming experience (yrs)			0.046**	[0.023]	0.065**	[0.026]
Concentrate use (kg/cow*day)			0.115***	[0.036]	0.140***	[0.038]
Trust proxy (money lent to farmer; 1=y)			0.081	[0.134]	-0.297*	[0.170]
Total assets (in 100 USD)			-0.006	[0.004]	-0.011**	[0.004]
Variables Z						
Age (yrs)					-0.025***	[0.006]
Gender (1=female)					-0.336*	[0.199]
Education (yrs)					-0.007	[0.021]
MCC 1 dummy (1=y)					-0.377**	[0.179]
MCC 3 dummy (1=y)					0.458**	[0.213]
MCC 4 dummy (1=y)					0.131	[0.220]
Sigma u	0.131	[0.082]	0.000	[0.056]	0.000	[0.063]
Sigma e	1.451***	[0.074]	1.377***	[0.071]	1.359***	[0.080]
Constant	2.614***	[0.131]	1.240***	[0.336]	2.386***	[0.470]
Observations	925		910[a]		735[a]	
Number of rounds	5		5		5	

Notes: The dependent variable in all models is the number of input bags chosen in a given round (0-2).
*** $p<0.01$, ** $p<0.05$, * $p<0.10$.
[a] Observations for which experimental subject and respondent in the household survey are not identical were excluded.
Source: Own calculation

Table 5: Additional estimation results (random effects longitudinal Tobit model)

	(IV)		(V)		(VI)	
	Coefficient	SE	Coefficient	SE	Coefficient	SE
Treatment variables						
Counterfactual treatment T2 (1=y)	-0.689**	0.326	-0.511	0.518	-0.594	0.585
Bonus treatment T3 (1=y)	1.731***	0.404	2.566***	0.660	2.890***	0.744
Variables X						
Session dummy (1=afternoon)	0.201	0.227	0.229	0.225	0.228	0.225
Session * T2	0.316	0.317	0.308	0.314	0.299	0.314
Session * T3	-0.580	0.372	-0.628*	0.368	-0.591	0.369
Round 2 dummy (1=y)	-0.029	0.193	-0.169	0.199	-0.169	0.199
Round 3	0.202	0.196	0.244	0.196	0.244	0.196
Round 4	0.420**	0.202	0.475**	0.202	0.477**	0.202
Round 5	0.519**	0.203	0.380*	0.209	0.381*	0.208
Misunderstanding of instructions (1=n)	-0.812***	0.199	-0.790***	0.196	-0.784***	0.197
Dairy farming experience (yrs	0.064**	0.026	0.061**	0.026	0.061**	0.026
Concentrate use (kg/cow*day)	0.142***	0.038	0.135***	0.038	0.137***	0.038
Trust proxy (money lent to farmer; 1=y)	-0.294*	0.170	-0.305*	0.168	-0.300*	0.169
Total assets (in 100 USD)	-0.010**	0.005	-0.011**	0.004	-0.010**	0.005
Variables Z						
Age (yrs)	-0.026***	0.006	-0.024***	0.006	-0.024***	0.006
Gender (1=female)	-0.351*	0.199	-0.321	0.198	-0.338*	0.198
Education (yrs)	-0.007	0.021	-0.006	0.021	-0.006	0.021
MCC 1 dummy (1=y)	-0.392**	0.180	-0.382**	0.177	-0.398**	0.178
MCC 3 dummy (1=y)	0.437**	0.213	0.450**	0.211	0.428**	0.211
MCC 4 dummy (1=y)	0.120	0.223	0.135	0.219	0.119	0.222
Mechanism I: Risk preferences						
Risk proxy (1=risk averse; 5=risk loving)	0.010	0.103			0.027	0.103
Risk proxy * T2	0.056	0.147			0.050	0.146
Risk proxy * T3	-0.144	0.168			-0.169	0.167
Mechanism II: Wealth						
Lagged profit (in '000 VND)			0.032**	0.013	0.032**	0.013
Lagged profit * T2			-0.005	0.018	-0.005	0.018
Lagged profit * T3			-0.042*	0.023	-0.044*	0.023
Sigma u	0.000	0.063	0.000	0.063	0.000	0.062
Sigma e	1.357***	0.079	1.342***	0.079	1.340***	0.078
Constant	2.360***	0.498	1.543***	0.564	1.486**	0.594
Observations	735[a]		735[a]		735[a]	
Number of rounds	5		5		5	

Notes: The dependent variable in all models is the number of input bags chosen in a given round (0-2).

*** $p<0.01$, ** $p<0.05$, * $p<0.10$.

[a] Observations for which experimental subject and respondent in the household survey are not identical were excluded.
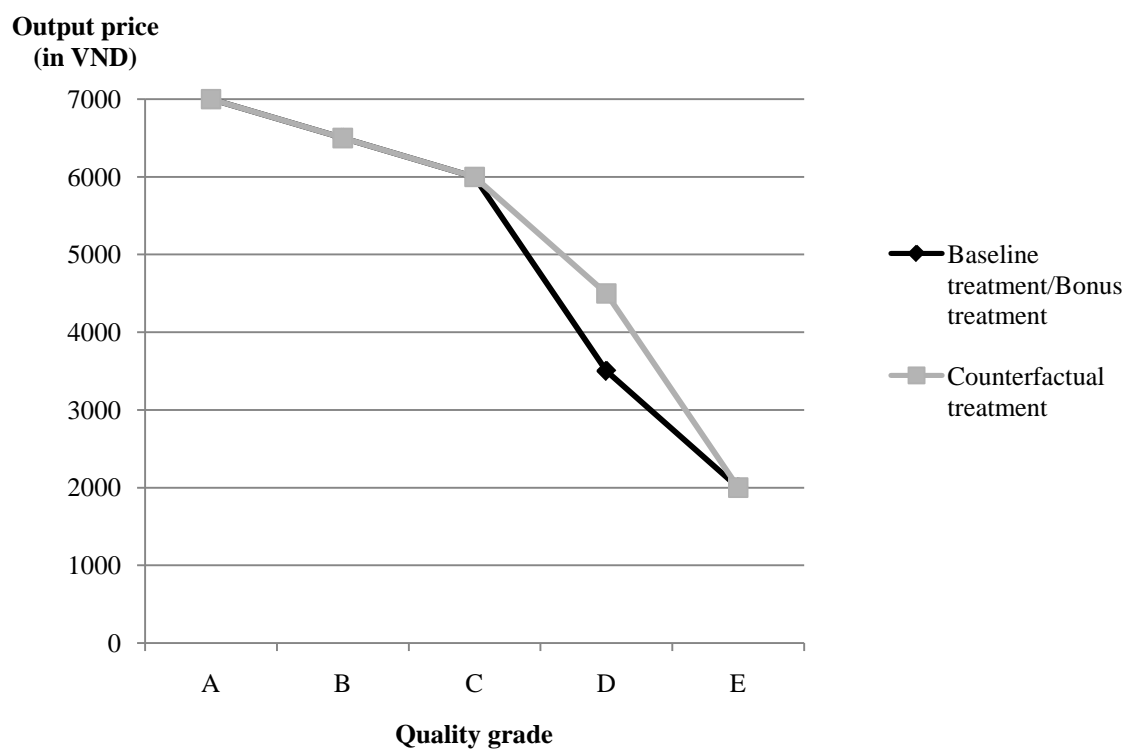
Source: Own calculation

Figure 1: Pricing scheme by treatment