Journal of Economic Behavior and Organization 212 (2023) 708–722

Contents lists available at ScienceDirect

Journal of Economic Behavior and Organization

journal homepage: www.elsevier.com/locate/jebo

Got (clean) milk? Organization, incentives, and management in Indian dairy cooperatives[☆]

Manaswini Rao[a,*], Ashish Shenoy[b]

[a] UC San Diego, Department of Economics, 9500 Gilman Dr #0508, La Jolla CA 92093, United States
[b] UC Davis, Department of Agricultural and Resource Economics, United States

ARTICLE INFO

Article history:
Received 10 November 2022
Revised 10 May 2023
Accepted 7 June 2023

JEL classification:
O13
L23
Q13
P13
D23

Keywords:
Quality upgrading
Cooperatives
Dairy
Elite capture
India

ABSTRACT

Smallholder producers in developing countries often collaborate in teams that take advantage of scale economies and allocate surplus among members. We experimentally evaluate team-level incentive contracts for quality upgrading among Indian dairy cooperatives where there is a risk of free-riding because individual quality cannot be traced. Incentives improve aggregate quality, with evidence of increased effort from both producers and cooperative managers. However, several managers decline incentive payments when they cannot control how payment information is disclosed to cooperative members. Survey evidence indicates publicity lowers managerial returns, suggesting transparency-based efforts to constrain elites can undermine the core policy goal.

1. Introduction

Much of the world's population engages in small-scale production. An estimated 400 million farms operate below one hectare (Lowder et al., 2016) and over 90% of firms worldwide employ fewer than ten workers (Hsieh and Olken, 2014). Linking these producers to broader input and output markets can raise profits, thereby increasing productivity and improving livelihoods. To this end, development policy has promoted cooperatives or collective organizations that take ad-

☆ We thank Emily Breza and Arun Chandrasekhar for their contribution to the experiment design and implementation; Abhijit Banerjee, Prashant Bharadwaj, Esther Duflo, Aprajit Mahajan, Craig McIntosh, Karthik Muralidharan, Rich Sexton, Monica Singhal, Tavneet Suri, and multiple anonymous referees for helpful comments and guidance; and Sharada Chandankar, Vasudev Naik, H.Y. Gowramma, Vikas Dimble, Madhumitha Hebbar, HyeJin Kim, Devika Lakhote, Bhavya Srinivasan, and Tithee Mukhopadhyay for excellent research support. This study was funded through generous grants from the George and Obie Shultz Fund, the Agricultural Technology Adoption Initiative, USAID Development Innovation Ventures, and the NSF Graduate Research Fellows Program. This study was preregistered with the AEA RCT Registry under ID Number AEARCTR-0000700. Research was conducted with approval from the Committee on the Use of Humans as Experimental Subjects at the Massachusetts Institute of Technology (COUHES #1405006372) and by the Institutional Review Board at the Institute for Financial Management and Research in Chennai, India. The authors have no financial interests to disclose, and no party had the right to review the paper prior to its circulation.

* Corresponding author.
E-mail addresses: manaswini.rao@gmail.com (M. Rao), shenoy@ucdavis.edu (A. Shenoy).

https://doi.org/10.1016/j.jebo.2023.06.002
0167-2681/

vantage of economies of scale, with an estimated half of the world's population involved in some form of cooperative enterprise (International Labour Organization, 2014). More recently, policymakers have emphasized quality upgrading as a further means to link small-scale producers in developing countries to global value chains with greater economic returns (World Bank, 2020).

This paper evaluates the effectiveness of incentive contracts for quality upgrading in cooperative agriculture through an experiment among rural Indian dairy farmers. Incentive contracts in cooperative systems generate a tension because production is carried out by individual members but returns are realized at the group level, leading to potential free-riding problems and coordination frictions. Nevertheless, cooperatives frequently organize around local community and social structures that have the potential to mitigate this misalignment of incentives (e.g. Ostrom, 1990). On the other hand, hierarchy in social status and power among producers governs the allocation of group surplus. In this study we directly test the capacity for dairy cooperatives to upgrade quality in response to group-level incentive contracts, highlight the role of cooperative managers in the take-up of such contracts, and provide indirect evidence on improvement being driven both by individual farmer as well as manager effort.

We study these questions in the context of village dairy cooperatives in Karnataka, India. The state's dairy sector encompasses more than 2.4 million producers in over 22,000 villages. Production is organized through village-level cooperative societies composed of smallholder farmers who pool their milk for sale and delivery to bulk processing facilities. Dairy is an important source of income for smallholder farmers throughout the developing world, and similar cooperative structures exist at the local level in the agricultural sector worldwide.

We evaluate the potential for upgrading through lowering bacterial contamination with two levels of experimental variation. First, we randomly introduce cooperative-level incentive contracts. Among a random subset of study villages, we offer an incentive payment tied to this aggregate measure of cleanliness. This intervention tests whether cooperative members improve production quality to take advantage of an opportunity for collective payment at the margin.

Second, among those offered incentives, we further randomize transparency around the incentive contract. We randomly vary whether the contract and transfer are disclosed privately to managers alone or announced publicly to managers as well as cooperative members. Prior work has found trust in leadership to be a determining factor in sustaining collective action among smallholder farmers (Casaburi and Macchiavello, 2015; Bernard et al., 2021; Aflagah et al., 2022). This second manipulation gives insight into whether transparency can alleviate trust-based inefficiencies, and investigates how such transparency affects production outcomes and the distribution of surplus.

We present two key findings. First, microbial contamination decreases among cooperatives offered financial incentives. We report indirect evidence that both dairy farmers and managers adjust their practices to improve sanitation. This result indicates that cooperatives can internally enforce a norm of cleanliness even without explicit individual-level quality measurements.

The increase in cleanliness is quantitatively large relative to the magnitude of the incentive. The incentive schedule offers the potential to raise cooperative revenue by up to 2.5 percent over a two-week period.[1] This modest opportunity generates an improvement in milk quality of up to 0.64 standard deviations, which corresponds to an 81% increase in the fraction of raw milk suitable for high-value processing. The size of response indicates a high capacity for organizational innovations to complement technological adoption in quality upgrading.

We find little evidence that cleanliness is enforced internally through direct financial remuneration to farmers from the cooperative. Farmers are paid a flat rate per liter, and this compensation does not change with quality incentives. Instead it appears that managers promote cleanliness through informal pressure along financial or social channels not directly related to this experiment. It remains unclear whether this pressure comes in the form of greater rewards for cleanliness or harsher punishment for lack of effort.

Second, almost a third of cooperative managers opt out of the incentive contract rather than receive payment with public knowledge. Opting out indicates public information lowers managers' surplus relative to their cost of effort. This loss of surplus must be related to the publicity itself, as managers who opt out first request to keep the payment private. On being declined that option, they ultimately consent to continue milk testing without payment. The decision to forego payment is especially perplexing because every cooperative would have received payment had they merely maintained the status quo with no change to production practices—our incentive contract promised a base pay, with increments tied to quality improvements over baseline. Commensurate with the high rate of opt-out, we measure a diminished treatment effect of incentives in the public information arm, though we lack power to statistically distinguish this difference from zero.

Refusing payment is most common among managers with weaker oversight and lower social status. Managerial oversight and status may modulate the effect of public information in two ways. First, publicly announcing payments may create extra work for the manager if the announcement is confusing and managers play a role in coordinating information and expectations within the cooperative. Second, publicity may lower managers' share of surplus from the experiment if managers enjoy information rents from private knowledge about payouts, consistent with economic models of income hiding (e.g. Jakiela and Ozier, 2015). It remains an open question exactly how publicity affects the surplus allocated to the cooperative manager, but in either case public announcement of revenue is less damaging to administrators with higher status.

[1] Of course, most cooperatives do not achieve the maximum payment. Actual payouts amount to a realized increase in cooperative revenue of one percent on average.

Together, these two findings paint a nuanced picture of the collective response to group-level incentives among agricultural cooperatives. Improvements in milk cleanliness at the cooperative level indicate that at the margin, potential misalignment between the private and aggregate returns to cleanliness does not undermine the power of incentives. However, the collective response to incentives may be diminished by barriers to managers' ability to coordinate or to allocate surplus.

This paper relates to several stands of literature. First, we expand the large body of work on quality upgrading in agriculture. Incentives in our study resemble contract farming (see Bellemare and Bloem, 2018, for a review), which we extend to cooperative agriculture. Our research most closely relates to prior studies of quality incentives in dairy using observational variation (Treurniet, 2021) or hypothetical elicitations (Saenger et al., 2013), and to experimental evaluation of quality incentives among Colombian coffee cooperatives (Macchiavello and Miquel-Florensa, 2019).[2]

The recent policy emphasis on quality upgrading as a means of economic development relies crucially on value-added in the supply chain passing through to upstream producers. Existing work demonstrates how transportation costs and market power can limit price passthrough from central markets to rural communities (Atkin and Donaldson, 2015; Mitra et al., 2018; Bergquist and Dinerstein, 2020; Dragusanu et al., 2022). We reveal how elite action at the community level can further curtail the share of value-added that reaches smallholder producers.

Second, our research sheds light on cooperatives in agricultural production. Cooperative agriculture has been promoted as a potential pathway out of rural poverty by connecting producers to markets with higher prices (e.g. Bernard et al., 2008; Markelova et al., 2009; Wanyama, 2014; Hill et al., 2021) and facilitating a more equitable distribution of agricultural revenue (Montero, 2022). The heterogeneous response to public information in our field experiment is consistent with results in prior lab experiments (e.g. Kosfeld and Rustagi, 2015) and observational studies (e.g. Macchiavello and Morjaria, 2021a) demonstrating the importance of manager identity, especially managerial social capital, in cooperative function.

Finally, we provide evidence on the role of local elites in policy implementation. Local elites act as implementing agents for centralized policy in many development settings (Bardhan, 2002; Platteau, 2009). Such arrangements have been shown to leverage local information to improve outcomes in tax collection (Balán et al., 2022), public procurement (Bandiera et al., 2021), and resource targeting (Alatas et al., 2012; Hussam et al., 2020). However, autonomy can also enable elite capture of public surplus (e.g. Reinikka and Svensson, 2004; Niehaus and Sukhtankar, 2013; Alatas et al., 2019). In the context of cooperative agriculture, Banerjee et al. (2001) and Casaburi and Macchiavello (2015) provide evidence that cooperative leaders skew their behavior to generate private returns. Our findings highlight the tension between these two competing pressures.

The rest of the paper proceeds as follows. In Section 2, we describe the setting and production process in more detail. Section 3 outlines our experimental design, and Section 4 presents results. We discuss the findings in Section 5, and Section 6 concludes.

2. Setting

2.1. Supply chain and cleanliness

We study quality upgrading in the context of dairy cooperatives in Karnataka, India. Milk production in the state is organized through village-level cooperatives that aggregate output from smallholder farmers for delivery to processing plants. Cooperatives consist of farmer producers that act as shareholders and managers that oversee production and finances. The typical farmer producer owns 1–2 milking cows and earns 20–30% of their total income from dairy activities. Karnataka state alone processes 2 million gallons of milk per day from over 2.4 million producers in over 22,000 villages across the state. Similar cooperative organizations are present for milk and agricultural production in many Indian states and other developing nations.

Dairy production originates in rural villages with smallholder producers. Each village-level cooperative, typically consisting of 50–100 producers, collects milk from pouring members into common cans at the cooperative premises during a brief daily window. A single can holds milk from 5–10 different producers, and once full, cans are sealed for immediate pickup and delivery to a processing plant. Appendix A walks through the village milk collection process with photographs.

At the processing plant, milk samples from each can are tested for quality to determine suitability for various dairy products. Low-quality milk is packaged directly as liquid milk, while higher-quality milk is creamed into butter or ghee, or cultured for higher value-added products such as cheese, yogurt, and milk sweets.

In this study, we aim to promote quality upgrading along the dimension of microbial load. This is an important margin of adjustment because different retail products require different levels of cleanliness in their raw milk input due to pasteurization methods. Milk used in high-value production must be pasteurized at temperatures of 70–80 °C. At this temperature, the USDA requires[3] that raw milk have no more than 500,000 colony-forming microbial units per milliliter (cfu/ml) to be

[2] Incentive contracts represent a demand-driven approach to quality upgrading. Related demand-side factors affecting agricultural output quality include contract design (Goodhue et al., 2010; Saenger et al., 2013), market competition (Bernard et al., 2017; Macchiavello and Morjaria, 2021b; Bold et al., 2022), and quality verification (Saenger et al., 2014; Hasanain et al., 2022). This work complements research on supply-side determinants of quality such as producer skill and technology adoption (see de Janvry et al., 2017, for a review).

[3] To the best of our knowledge, the Food Safety and Standards Authority of India (FSSAI) sets standards for microbial load in final production but does not regulate raw milk inputs.

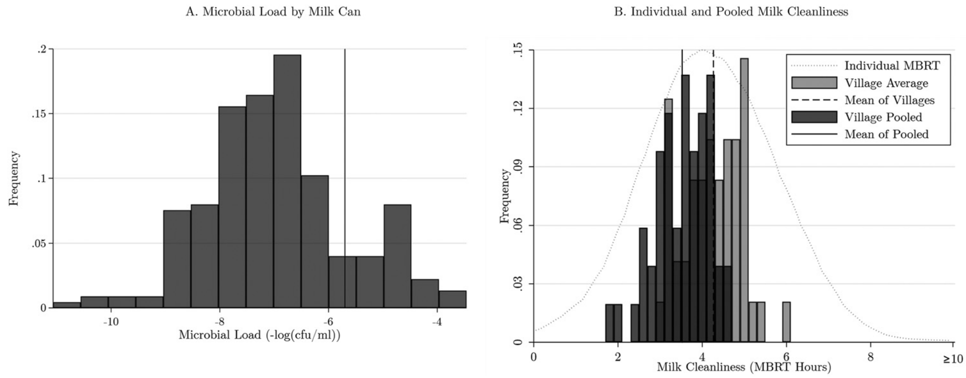

Fig. 1. Baseline distributions of milk quality. Notes: Distributions of milk cleanliness at baseline. Samples are collected during cooperative milk collection. A. Can-level measurements conducted using a standard plate count (SPC), reported in –log units so that higher values indicate cleaner milk. The vertical line represents the 500,000 cfu/ml threshold for use in value-added production. Only 37 of 225 cans tested (16%) satisfy this requirement. B. Measurements conducted using a methylene blue reduction test (MBRT), reported in hours, so that higher values indicate cleaner milk. The dotted density line represents the distribution among samples from individual producers. 14% of producers exceed the 6 h threshold delineating high sanitation. The vertical bars represent distributions of within-cooperative average of samples taken from individual producers immediately before pouring and from collective cans immediately after pouring. The dashed vertical line represents the mean across villages of individual samples and the solid vertical line represents the mean across villages of cooperative cans. Reduction time declines by 0.74 hours from individual to pooled milk, and a *t*-test rejects equality with a t-statistic of 5.6 ($p< 0.01$).. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

used as an input for value-added milk products. Even below this threshold, variation in the bacterial content of raw milk produces noticeable differences in flavor down to 10,000 cfu/ml (Murphy et al., 2016).

There is substantial room for improvement in the cleanliness of milk in our study setting. Panel A of Fig. 1 presents a histogram of the microbial load by delivery can prior to any intervention. Of 225 cans tested at baseline, only 37 met the USDA standard for value-added processing. The remaining milk, with bacterial loads exceeding 500,000 cfm/ml, requires ultra-high temperature (UHT) pasteurization at 135 °C. This process denatures enzymes and proteins, meaning the product is only suitable for sale as liquid milk.[4]

Cleanliness is affected by both farmers' milking practices as well as sanitation of shared equipment and cooperative premises. Farmers can lower the microbial count in their own product with basic hygiene procedures such as regularly cleaning their cows' udders, maintaining a sanitary milking space, and washing hands and equipment prior to milking. Because milk from each farmer is poured into common delivery cans, regular washing of village equipment also contributes to milk cleanliness. These activities complement recent technological investment by the dairy sector to limit contamination further along the supply chain.[5]

We break down the potential for improvement in each of these areas by comparing baseline samples taken from farmers immediately before pouring into village cans to samples taken from village cans immediately after pouring in Panel B of Fig. 1.[6] The dotted density line plots the distribution of cleanliness among individual producers. There is substantial variance, with only 14% of producers delivering milk that achieves the highest sanitation rating. A policy that shrinks this variation by one standard deviation toward the 95th percentile would raise the high-sanitation portion by 16% (2 percentage points). The histogram bars plot the distribution of sanitation at the cooperative level measured before and after pouring into shared cans. There is a large and statistically significant decline in the cleanliness of pooled milk samples from village cans from 4.26 to 3.52; a *t*-test rejects the equality of these two values with a t-statistic above 5 ($p< 0.01$). This contamination introduced by cooperative equipment is equivalent to a 0.5 standard deviation decrease in individual producer quality. Improvements in both individual milking practices as well as sanitation of collective equipment could increase the cleanliness of raw milk delivered for processing.

Measuring microbial load requires lab equipment, training, and time. It is therefore logistically infeasible to regularly measure individual producers' milk at the point of collection beyond a basic sight and smell check for spoilage. In the supply chain, the most decentralized unit that could reasonably be tested is the delivery can, which contains milk from 5–10 producers. In practice, cooperatives do not track which producers pour into which cans so the effective unit of aggregation is the entire cooperative.

[4] UHT can accommodate bacterial contamination up to 5 million cfu/ml for shelf-stable packaging according to the TetraPak Milk Quality handbook, and even greater levels if sold for immediate consumption. Milk that is unsuitable even for UHT is usually detectable by sight or smell, and is therefore rejected before it reaches the processing plant.

[5] In the recent past, the dairy sector has invested significantly in reducing the time to refrigeration through initiatives such as optimizing the transportation routes of collection trucks and installing rural bulk refrigeration facilities.

[6] It was prohibitively expensive to conduct a plate count of microbial load on individual milk samples. Instead, we plot results from a dye reduction test designed to measure microbial contamination, which unfortunately does not directly translate to USDA safety measures. The relationship between these two measures is discussed in Appendix B.

2.2. Production incentives

Production incentives for farmers are misaligned with the value of sanitation in the supply chain in two ways. First, there is little return to cleanliness at the production stage. Cooperatives are paid based on the quantity of raw milk delivered, with little to no variation based on cleanliness. In this study we evaluate incentive contracts designed to address this source of misalignment. We introduce a high-quality testing procedure coupled with incentive payments linked to the measured microbial load at the aggregate cooperative level.

We leverage the existing financial infrastructure for incentive payments. Each cooperative has a bank account to receive payment from the processor. Management is then responsible for disbursing payments to individual farmers.[7] We supplement the regular cooperative revenue from processors with a bonus contract based on the measured microbial load in milk. Beyond making a deposit into the cooperative bank account, we offer no instruction on how this surplus revenue should be allocated.

The second source of misalignment arises because quality-based incentives at the aggregate level generate the potential for free-riding within the cooperative. Payments must be conditioned on the aggregate quality of pooled milk because it is prohibitively expensive to test quality at the individual level, so the return to any individual's effort is shared with the entire cooperative and the effect of shirking is literally diluted. However, shirking may be mitigated because cooperative members and management are part of a local village community and interact outside the cooperative. Even though the cooperative cannot directly measure the microbial load in any individual's milk, they may have local information about members' level of effort that enables them to internally enforce a norm of cleanliness.

2.3. Cooperative governance

Each cooperative is managed by an elected president and secretary who make administrative decisions and serve as local points of contact for our study. Together they manage the cooperative financial account, which is held jointly in their names. In addition, the secretary is in charge of day-to-day operations, most notably managing daily milk collection. The two officers serve staggered ten-year terms, and are overseen by a board of directors typically consisting of 9–10 cooperative members. The board is composed of local member producers and is intended to provide representation for the various communities within the village, though the election process varies idiosyncratically by village. In Appendix Table S2 we provide data on demographic details and social perceptions of producers, board members, secretaries, and presidents.

While the board of directors retains formal authority over cooperative managers, informal authority within the cooperative is mediated by the local social network. Cooperative presidents occupy traditional positions of high social status—they are wealthier and more educated on average than producers and directors, less likely to belong to a scheduled (i.e. low-status) caste or tribe, and more likely to have previously been elected to serve in the local legislative assembly (Gram Panchayat). Secretaries embody a second type of local elite. While their demographic characteristics are more in line with typical producers, the one notable exception is in education. Secretaries have on average twice as much education as the typical producer. The position of cooperative secretary underscores an often overlooked channel through which people of historically lower socioeconomic status can participate in local administration.

Social perceptions of the status and management quality of presidents and secretaries relative to directors correspond to their position as elites. Cooperative members evaluate secretaries and presidents higher than they evaluate members of the board of directors. In fact, secretaries are consistently rated slightly above presidents, even in questions of social standing. These differences in the characteristics of cooperative managers underscore the potential for elite capture in this setting. In particular, the managers in charge of the cooperative bank account—the president and the secretary—are also those that have the greatest education and social standing, and are seen to be the most capable and knowledgeable. Their dominant position in the village social network limits other stakeholders' ability to constrain their actions despite the formal oversight authority of the board of directors.

Rent extraction by elites is also hinted at in cooperatives' finances. Each cooperative runs an operating surplus to pay for facilities and maintenance as well as salaries for staff. At the end of the year, any remaining surplus is mandated by state law to be distributed among members. However, in practice the use of funds is murky. In the three years leading up to our study, all cooperatives participating in our study officially reported net accounting surpluses in every year, indicating that they should have paid bonuses to farmers. Despite this, at baseline only 20% of farmers surveyed could remember ever receiving a bonus from the cooperative, revealing a disconnect between official accounting and the actual use of funds.

3. Experimental design

3.1. Intervention

To promote clean production practices, we couple a high-quality testing procedure with incentive payments for cleaner milk. In every study village, treatment and control, we collect milk samples, measure the microbial load in a lab, and share

[7] During the period of our study, these disbursements from the cooperative to the farmer were predominantly made in cash.

the results with cooperative management. Regular testing already takes place in the cooperative system, but cooperatives normally do not internally measure or receive feedback on cleanliness, so feedback alone may provide new information.[8] We keep monitoring and feedback uniform across study villages in order to experimentally isolate the effect of group incentives on production outcomes.

Experimental randomization takes place across cooperatives in two stages. First, each participating cooperative is randomly assigned to either receive incentive payments for clean milk or not. Second, in treated cooperatives, we further randomly vary whether incentive payments are announced privately or publicly. In the private treatment arm the existence of incentives is disclosed only to the secretary and president, though they may choose to share this information with others at their discretion. In the public treatment arm, we also inform a random subset of cooperative members about the incentive payments. Cooperatives in which we only conduct testing without associated incentives serve as the control group for our experimental manipulation.

The first stage of randomization introduces a supplemental payment based on measured milk quality. In each treated cooperative, we test a sample from each delivery can on a given collection day and then make the incentive payment to the cooperative financial account as a function of the average quality across all cans. This treatment generates returns to cleanliness for the cooperative as a whole, but introduces the possibility for internal free-riding because each individual's effort is diluted across the average quality of the entire village. We test whether the cooperative can raise its total quality in the aggregate even though it cannot directly measure the quality of any individual's output.

Incentive payments range from Rs. 0 for the lowest quality to Rs. 2,000 for the highest quality, equivalent to roughly $40.[9] With average daily revenues of Rs. 5,600, producing the highest quality milk would generate a 36% increase in revenue for the day.[10] The payment schedule is scaled so that the average payment at baseline (i.e. with no quality improvement) would be ∼Rs. 500, or roughly $10, representing a 9% increase in typical daily revenue. Because we test once in a two-week period, these values should be divided by 14 to interpret the expected size of the incentive on any given day.

The second stage of randomization varies the level of information disclosure about the incentive payment across cooperatives in the treated group. In all treated villages, we share the payment schedule and subsequently realized payment with the president and secretary. In a subset of these, designated the public payment arm, we further reveal this information to a random subset of pouring members at the time of milk collection. In the rest, designated the private payment arm, we do not disclose any information publicly. Information revelation plays two roles in this setting. First, it helps alleviate information asymmetry about the collective returns to cleanliness by increasing the set of participants that know about the group incentive. Second, it lowers the potential for managers to extract information rents by constraining their ability to hide the size of these payments.[11] We investigate whether the public provision of information affects aggregate productivity, and whether it changes distributional outcomes by influencing the extent to which realized returns to cleanliness are shared with cooperative members.

3.2. Randomization, data collection, and analysis

We recruited village-level cooperatives affiliated to the milk processing facility in the Dharwad district of Karnataka in India. Participating cooperatives were recruited from the two sub-districts closest to the processor. We contacted all 56 cooperatives in the Hubballi and Dharwad sub-districts, out of which 55 agreed to participate. Four dropped out before the experiment began, leaving a final sample of 51 cooperative societies with a total of 2859 pouring members.

We first conduct baseline surveys with cooperative management and a random sample of pouring members in each village, and collect two rounds of data on pre-intervention milk cleanliness. We then randomly assign village treatment status and collect two more rounds of data on incentivized milk cleanliness. Finally, we conduct endline surveys with another random sample of pouring members after the intervention concluded. Fig. 2 shows the full timeline and treatment assignment across the two rounds of intervention, and we provide further detail in Appendix B.

Treatment assignment is reassigned between the two incentive rounds for some villages. In Round 1, there are 19 villages assigned to control, 19 villages assigned to private payment, and 13 villages assigned to public payment. Between Rounds 1 and 2, we randomly reassign 6 villages from control to public payment and 3 from private to public payment.[12] There are no villages reassigned in the other direction because public announcement of payments is an absorbing state as we cannot credibly revoke the expectation that incentives will be paid.

Table 1 provides descriptive statistics for the treated and control groups. Covariates appear balanced; only the fraction of income earned from dairy differs significantly between the two. A joint test of significance for all survey outcomes fails to

[8] We conjecture the value of this information was minimal because cooperative management were already aware of but did not engage in the basic sanitation practices necessary to improve cleanliness. However, we cannot experimentally quantify the value of testing in isolation because a "pure control" arm with no feedback or no testing at all was not included due to financial constraints.

[9] Full details of the payment schedule are provided in Appendix B.

[10] The high end of the payment scale is equivalent to just under one month's average salary for a secretary, and nearly 80% of a month's self-reported total earnings for the average dairy producer.

[11] The public payment arm is also the only arm that shares feedback about milk cleanliness with cooperative members. We believe incentives, rather than feedback, to be the binding constraint at the producer level, and therefore do not include an unincentivized public information treatment arm.

[12] Motivation for changing treatment assignment mid-intervention is discussed in Appendix D and robustness of results to this design in Appendix C.

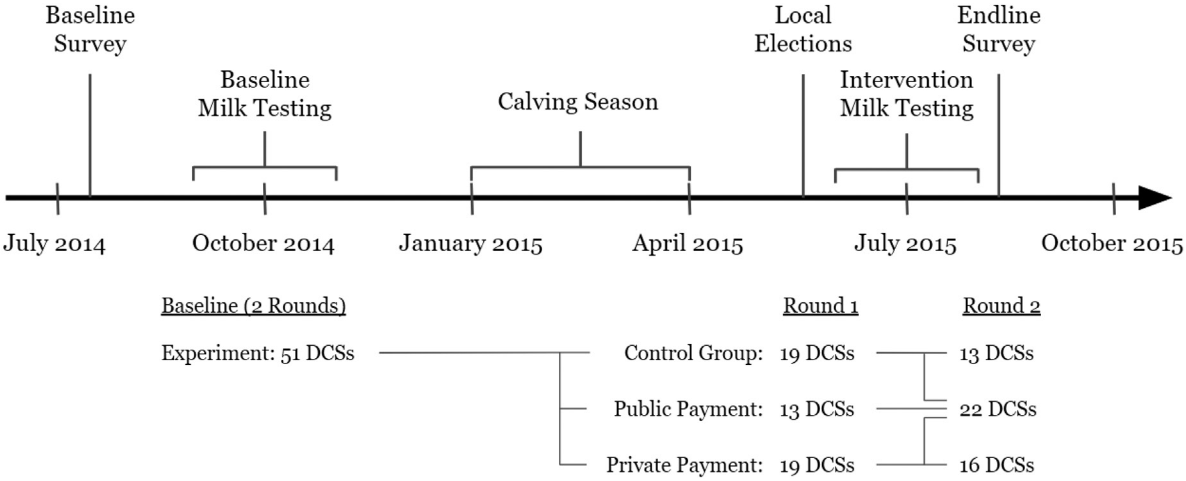

Fig. 2. Experiment timeline and randomization design.

Table 1
Descriptive statistics by treatment status.

	Control	Treated	Difference
HH Size	6.8	6.2	-0.60
	(0.30)	(0.23)	(0.38)
Education	5.4	4.1	-1.3
	(0.34)	(1.0)	(1.1)
Frac. SC/ST	0.31	0.28	-0.03
	(0.05)	(0.03)	(0.06)
Land Owned	7.4	6.0	-1.5
	(0.80)	(0.56)	(0.98)
Cows Owned	1.7	1.7	-0.05
	(0.11)	(0.04)	(0.11)
Monthly Income	13,894	11,114	-2,780*
	(1,218)	(800)	(1,458)
Frac. Dairy Income	0.28	0.33	0.05***
	(0.01)	(0.02)	(0.02)
Frac. Farmers	0.62	0.63	0.00
	(0.04)	(0.03)	(0.05)
Frac. Labor	0.12	0.17	0.05
	(0.02)	(0.02)	(0.03)
Milk Production	6.44	6.17	-0.27
	(0.38)	(0.23)	(0.45)
Milk Cleanliness	0.23	-0.17	-0.40
	(0.49)	(0.39)	(0.28)
Num. Villages	13	38	
Joint F-Statistic			1.5
[p-value]			[0.17]

Notes: Descriptive statistics at baseline for farmers in treated and control cooperatives. The third column reports the differences between the two groups. Joint F-statistic excludes milk cleanliness, which was measured separately from survey responses. Standard errors clustered by village in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

reject equality at the 10% level. Importantly, there are no statistically significant differences between treatment and control in average quantity poured, cleanliness, or number of livestock.

The primary outcome of interest is the microbial load in raw milk produced by the cooperative. We measure the average microbial load in samples taken from every cooperative can[13] during the morning dairy collection using two lab tests of bacterial load—the methylene blue reduction test (MBRT) and the standard plate count (SPC)—which can be considered two noisy measures of underlying milk cleanliness. To maximize power, our main outcome for analysis is a composite measure of milk quality that is the first principal component of these two variables.[14] We construct this index at the village level

[13] In the first baseline visit we collected individual-level samples from a subset of producers prior to pouring, but individual-level sampling proved to be too disruptive to milk collection and was therefore discontinued.

[14] Incentive payments were based only on MBRT for transparency. MBRT is the primary measure used for day-to-day production decisions by processors, and in focus groups we found that most cooperative members and secretaries were familiar with MBRT but not with SPC. It is highly unlikely that study participants could take actions specifically aimed to improve MBRT readings without increasing overall milk cleanliness.

Table 2
Impact of treatment on product quality.

	(1) Cleanliness	(2) Cleanliness	(3) SPC	(4) MBRT
Private Payment	0.64*	0.61**	0.47	0.36
	(0.35)	(0.3)	(0.32)	(0.22)
	[0.1]			
Public Payment	0.32	0.34	0.38	0.17
	(0.32)	(0.28)	(0.32)	(0.18)
	[0.32]			
H_0 Pvt=Pub (p-val)	0.32	0.35	0.78	0.35
Control Mean	0.06	0.06	6.83	3.44
R-Squared	0.08			
Observations	204	204	204	204
Double-Lasso		X	X	X

Notes: The four columns report DD estimates from eqn. (1). Columns 2–4 include covariates selected using the double-lasso method introduced by Belloni et al. (2013). The control variables include flexible trends - each control variable interacted with round dummies - by management and producer wealth, by management and producer education, by management and producer caste (SC/ST), by management and producer income levels—total and from dairy, number of cattle for milk production, engagement as agricultural labor, and by management's past experience in elected office (panchayat). (1)-(2) Cleanliness is an index of milk quality constructed from principal components analysis of SPC and MBRT. (3) SPC is measured in -log(cfu/ml). (4) MBRT is hours to dye reduction. Standard errors clustered by village in parentheses. p-values from randomization inference with clustered bootstrap in square brackets. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

by averaging over can-level measurements. Details on measurement and the principal components analysis are provided in Appendix B.

We supplement quality testing with two rounds of survey data. At baseline, we collect data on demographics, income, dairy production, and subjective perception of cooperative managers from twenty producers per village selected randomly from those contributing milk on the day of the visit. At endline, we repeat this procedure with another twenty randomly sampled producers, with additional questions about cooperative activities and knowledge of incentive payments. We also administer baseline questionnaires to cooperative secretaries, directors, and presidents covering their demographics, dairy involvement, and managerial practices.

To evaluate treatment effects, we implement a difference-in-differences (DD) estimation strategy. The estimating equation is

$$Y_{(i)jt} = \beta^{Pr}T^{Pr}_{jt} + \beta^{Pu}T^{Pu}_{jt} + \gamma_j + \delta_t + \epsilon_{(i)jt} \tag{1}$$

where j indexes cooperatives, t indexes testing rounds, and estimation for producer-level survey outcomes includes the additional index i for producer. The variables T^{Pr} and T^{Pu} are dummies representing assignment to either private or public payment arms in round t, and both dummies are 0 for all cooperatives in the two baseline rounds of observation.[15] γ and δ represent village and time fixed effects, respectively. For the subset of endline survey outcomes that were not asked at baseline, we drop the fixed effect terms and estimate the simple difference between study arms, which should be balanced under the null due to randomization. All standard errors and randomization inference are clustered at the village level.

We submitted a pre-analysis plan for this trial to the AEA RCT Registry before the start of the study, and discuss deviations from the pre-specified design that arose during project implementation in Appendix D.

4. Results

4.1. Cleanliness

Group incentives induce improvements in milk cleanliness. This main result is presented in Table 2, which reports the effect of treatment assignment on milk quality.[16] Col. 1 reports estimates from the DD specification in equation (1) on the index of cleanliness, and Col. 2. repeats the same exercise with control variables selected using the double-lasso method

[15] Some cooperatives change treatment status, but there is only a single endline survey observation per individual. we assign treatment status to be the treatment status in the final round of intervention in such cases.

[16] Table 2 and all subsequent regression tables report both standard errors clustered at the village level in parentheses and p-values generated by randomization inference in square brackets. Randomization inference uses 10,000 iterations of a clustered bootstrap procedure following MacKinnon and Webb (2020). To estimate the significance of the coefficient on assignment to private payment, we randomly re-draw 19 cooperatives from $19 + 13 = 32$

Table 3
Impact of treatment on earnings and quantity.

	(1) Payment Round 1	(2) Payment Round 2	(3) Quantity	(4) Received Bonus	(5) Opted Out Round 2
Private Payment	121.1	98.3	−0.06	0.01	0
	(106.9)	(82.7)	(0.58)	(0.09)	(.)
	[0.33]	[0.26]	[0.94]	[0.84]	[.]
Public Payment	−0.40	16.8	1.0**	0.03	0.32***
	(85.4)	(81.1)	(0.49)	(0.08)	(0.10)
	[1.0]	[0.85]	[0.14]	[0.6]	[0.0]
H_0 Pvt=Pub (p-val)	0.2	0.33	0.049	0.86	0.0032
Control Mean	715.8	676.9	6.43	0.81	0
R-Squared	0.05	0.05	0.01	0.48	0.21
Observations	153	153	2006	2006	51
Diff-in-Diff	X	X	X	X	

Notes: First two columns report DD estimates from Eq. (1). Third and fourth columns report DD estimates from Eq. (1). Fifth column reports simple difference in second intervention round. (1) and (2) report total payment received by cooperative, and control mean reflects counterfactual payment that would have been received by cooperatives in control arm. (3) Quantity is liters per day per producer surveyed; total village quantity is unavailable. (4) Fraction of producers who report ever receiving a bonus payment. (5) Fraction of cooperatives that opt out of payment in second intervention round. Standard errors clustered by village in parentheses. p-values from randomization inference with clustered bootstrap in square brackets. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

proposed by Belloni et al. (2013) to increase precision.[17] Assignment to the private payment arm improves average cleanliness by 0.64 standard deviations, significant at the 10% level without controlling for covariates and at the 5% level with controls. The effect of treatment in the public payment arm is also positive, but smaller in magnitude at 0.32 standard deviations. Given the limited size of the experiment, we can neither statistically distinguish this effect from zero nor can we rule out that it is equal to the effect of private payment.

We next decompose the treatment effect into its constituent components. Cols. 3 and 4 show the independent effect of treatment assignment on SPC and MBRT test measures. The SPC microbial load decreases by 0.42 log(cfu/ml) and the time to MBRT reduction increases by 0.4 hours on average among cooperatives in the private treatment arm. These values represent improvements of 0.37 and 0.7 standard deviations, respectively, which are both consistent with the magnitude of change in the quality index. In Appendix C, we further break the treatment effect down by quantiles. Among treated cooperatives, we find the treatment effect to be consistently strong across the distribution of quality in the private payment arm.

We quantify the economic importance of these effects relative to the benchmark SPC threshold of 500,000 cfu/ml recommended by the USDA for raw milk inputs into value-added processing. Recall from Fig. 1 that only 16 percent of cans tested at baseline satisfied this threshold. A 0.64 standard deviation improvement in the baseline distribution of SPC would correspond to an 81% increase in this number, to nearly 30 percent of cans acceptable for high-value production.[18]

4.2. Payment

The gains in cleanliness were achieved with fairly low-powered incentives. The first two columns of Table 3 show the size of aggregate cleanliness payments to treated cooperatives relative to the counterfactual payment the average control cooperative would have earned in each intervention round. Greater sanitation among treated cooperatives brings in roughly an additional Rs. 100 per collection day per cooperative in the private payment arm. In total, treated cooperatives earn around Rs. 800 per round per cooperative in payments for cleanliness, equivalent to $16 at the time of study. Compared to the average daily revenue of Rs. 5,600, this value amounts to only a 1% increase in revenue over the two-week testing window. The magnitude of the aggregate response to such a small incentive testifies to the low cost of improving cleanliness at the margin.

Producers in the public payment arm appear to behave strategically during the intervention period to secure a portion of this additional revenue. Almost all dairy-related payments in this setting are apportioned as a function of quantity poured: Producers and cooperatives are paid directly by volume, and any year-end bonuses or producer support schemes are awarded

total private payment treatment and control cooperatives in each iteration. For the public payment treatment, we redraw 22 cooperatives from the 22+13 = 35 total public and control cooperatives in each iteration.

[17] Details are discussed in Appendix C.

[18] Regression analysis using a dummy for passing 500,000 cfu/ml on the left hand side estimates a comparable treatment effect magnitude of nine percentage points in the private information arm. However, such a coarsening of the outcome variable in an already small sample leads to large standard errors for this exercise.

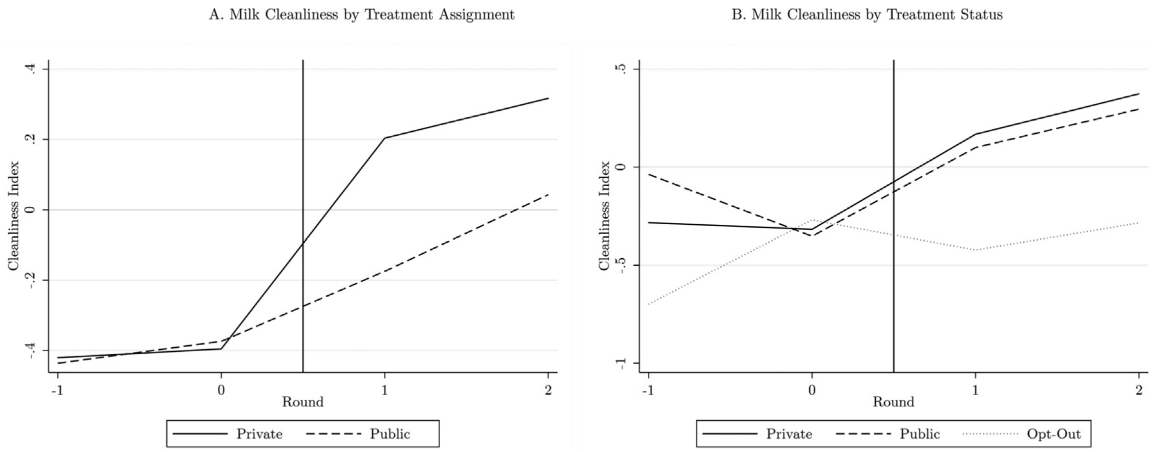

Fig. 3. Event study of cleanliness by treatment assignment. Notes: Outcome is an index of milk quality constructed from principal components analysis of SPC and MBRT. A. Event study version of Eq. (1) by treatment assignment. B. Event study version of Eq. (1) splitting public payment arm based on decision to opt out.

per liter. This fact gives context to the producer-level increase in quantity[19] in the public payment arm, reported in Col. 3 of Table 3. A quantity increase of nearly 16% per producer is observed in the intervention arm where producers knew the cooperative would receive additional revenue, and potentially reflects their attempts to secure a share of that revenue.

Despite producers' efforts, we find no direct evidence that incentive payments were shared with cooperative members in either treatment arm. There is no difference between treatment and control in the share of farmers that recall receiving bonus payments from the cooperative post-intervention, reported in Col. 4 of Table 3.[20] This null result implies that any gains from producer effort were likely achieved through informal social pressure rather than explicit remuneration.

4.3. Declined payment

Some of the gap in measured treatment effect between the public and private payment arms can be attributed to the unexpected fact that a substantial fraction of managers in cooperatives assigned to public treatment declined to be paid. In the second round of intervention, seven out of twenty-two secretaries opted to forego payment entirely rather than accept a publicly announced incentive payment (Table 3, Col.5). In all cases, the managers first requested that payment be made to the cooperative account without public knowledge. Upon being denied, all seven opted out of payment, but consented to continue milk testing and subsequent endline surveying.

We explore the relationship between opting out and cleanliness in Fig. 3. Panel A plots the treatment effect in the two payment arms as the event study counterpart to Table 2, Col. 1. Panel B splits the public payment event study into cooperatives that participate and those that opt out in the second round. The figure reveals two facts: First, cooperatives that opted out start with ex ante lower milk quality than those that remain in the experiment. Therefore, there may be selection into opting out based on the anticipated size of payment or other cooperative characteristics. Second, the trend line for villages that stay in the experiment with public payments closely tracks that of private payments, while the trend line for those foregoing payment remains nearly flat. Quantile treatment effects presented in Appendix C verify this effect heterogeneity, with larger effects observed at higher cleanliness quantiles in the public payment arm.

Regression analysis reveals that opting out explains at least some of the gap between treatment arms. In a two-stage least squares version of Eq. (1) using treatment assignment as an instrument for actual incentive status,[21] the estimated effect of public payment increases from 0.32 (Table 2, Col. 1) to 0.39. Note that this latter value is not directly comparable to the estimated 0.64 effect size in the private treatment arm because it is a local average treatment effect among a selected subset of cooperatives. Those remaining in the public payment arm (i.e. treatment compliers) have higher quality at baseline, and hence may have lower potential for improvement than those that opt out.

Cooperatives that opt out of public payment appear to be negatively selected by managerial capacity. Across all baseline indicators of management quality, cooperatives where the secretary declines payment perform consistently worse than those that remain in the public payment arm. The board of directors meets less frequently, producers can identify fewer board members, and producers are less likely to recall having received bonuses. Moreover, producers rate all managers lower

[19] It is difficult to change quantity through number or quality of livestock over the short horizon of our study, so the most likely margin of adjustment is additional days of milking.

[20] This result is presented with the caveat that the share in control rose from 20% at baseline to 80% at endline due to a statewide support scheme delivered in early 2015, which might drown out any differential impacts between treatment and control arising from our intervention.

[21] The DD estimate can be thought of as an Intention to Treat (ITT), and the 2SLS as a Treatment on Treated (TOT).

Table 4
Impact of treatment on producer beliefs at endline.

	(1) Know about Payments	(2) Coop Gave Lessons	(3) Believe Other Prod. Clean	(4) Believe Secy. Clean	(5) Believe Secy. Good Manager
Private Payment	0.01	0.09	0.45***	−0.26**	0.07
	(0.01)	(0.07)	(0.11)	(0.12)	(0.21)
	[1.0]	[0.53]	[0.0]	[0.01]	[0.58]
Public Payment	0.16***	0.09	0.30**	−0.08	0.24
	(0.04)	(0.07)	(0.12)	(0.13)	(0.21)
	[0.03]	[0.47]	[0.0]	[0.3]	[0.04]
H_0 Pvt=Pub (p-val)	0.0002	0.97	0.24	0.15	0.92
Control Mean	0.008	1.37	4.31	4.53	4.09
R-Squared	0.08	0.004	0.06	0.03	0.05
Observations	982	982	1918	1990	1983
Diff-in-Diff			X	X	X

Notes: First two columns report simple difference at endline; remaining three columns report DD estimates from eqn. (1). (1) Fraction of respondents that know about cleanliness incentive payments. (2) Frequency with which cooperative gives lessons on clean milking practices. (3) Avg. belief among producers about cleanliness of other producers. (4) Avg. belief among producers about cleanliness of secretary. (5) Avg. belief among producers about managerial quality of secretary. Standard errors clustered by village in parentheses. p-values from randomization inference with clustered bootstrap in square brackets. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

in both management quality as well as social status. A full breakdown by measure is given in Appendix Table S4. An F-test confirms the joint significance of producers' negative beliefs about management quality at the 1% level, revealing that managers with lower social standing are more likely to forego public payment.

5. Discussion

5.1. Margins of adjustment

To generate the ideal data to quantify margins of adjustment for cleanliness, we would need to test individual-level milk samples before pouring into common cans. We could then decompose aggregate improvements into the portion attributable to individual producers and the portion attributable to village equipment. While we collected individual milk samples in the first round of baseline data collection, this process turned out to be excessively disruptive to cooperative operations and had to be dropped in order for the experiment to continue. As a result, we cannot provide direct data on the microbial load contributed by individual producers during the experimental rounds of evaluation.

Though we cannot directly measure sources of change within a cooperative, we report suggestive evidence that improvements in cleanliness come from both better sanitation of village equipment and from individual producers pouring cleaner milk. On the part of management, enumerators and producers frequently observed cooperative staff washing collection equipment in incentivized villages during the intervention period; such sights were rare both prior to our involvement and in control villages during the experiment. As reported in Fig. 1, average time to MBRT reduction was 0.74 hours lower in pooled milk than in individual samples at baseline. Therefore, effective sanitation of village equipment alone has the potential to generate the full 0.4 h treatment effect.

There is also indirect evidence of cleaner milking practices among producers in incentivized cooperatives. Table 4 reports results from endline surveying following the intervention period. In Col. 3, we show that producers' beliefs about others' cleanliness improve in both incentive arms. This change is not caused by the salience of testing because the table reports increases relative to control, where quality testing also takes place. It similarly cannot be attributed to the salience of payments because we observe the effect even in the private payment arm where there is little knowledge about incentive payments among member producers (Col. 1). Instead, the difference in perception likely reflects a real change in the observed behavior of other producers.

To the extent that sanitation improves among producers, it seems lack of knowledge was not a constraining factor. There are insignificant and quantitatively small differences between treatment and control in the frequency with which cooperatives taught members about clean practices (Col. 2). Qualitative surveying confirms widespread recognition that washing hands and sanitizing equipment are crucial to mitigating contamination. For the most part, producers in our experiment already understood how to improve and gains were achieved through increased effort.

While perceptions are consistent with cleaner practices among cooperative members, the mechanism driving this behavior change remains unclear. We observe no evidence of increased pay directed to individual producers (Table 3, Col. 4). Moreover, in the private information arm, management does not even notify producers about the existence of incentive contracts for cleanliness (Table 4, Col. 1). These facts imply that producers are not responding to explicit financial rewards for observed effort, but managers instead exert influence through informal channels that are more difficult to quantify. Informal pressure is successful despite the fact that individual quality measurement is prohibitively expensive, suggesting that there is substantially greater local knowledge about private effort than what is observable to the outside market.[22]

The survey results together highlight two channels through which managerial effort affects milk contamination. Most directly, managers spend time sanitizing shared village equipment. They also indirectly lower contamination by expending social capital to promote clean practices or establish norms among producing members. At the margin, we measure high cleanliness returns to the managerial effort induced by a relatively small group incentive.

Counterintuitively, perceptions of secretary cleanliness decrease in the private payment arm despite measured improvements in aggregate quality. This decrease in perception, reported in Col. 4, may stem from the visibility of cleaning activities. Without corresponding knowledge of an increase in returns, from the farmers' perspective it would appear that the cooperative is suddenly promoting clean practices with no additional benefit. It is possible that this leads farmers to conclude that secretaries must have been inefficiently dirty in the past. It is notable that producers only update beliefs about cleanliness specifically and not about managerial capacity in general (Col. 5). In contrast, producers in the public payment arm know that additional effort is a response to new financial incentives and therefore do not update beliefs about the past. Other explanations are possible, but this dynamic hints at a potential channel of path dependence in governance or management that warrants further study. Leaders may maintain bad behavior to avoid revealing information about the low quality of their prior actions.

5.2. Information and managerial returns

Although incentive payments are offered to the cooperative as a whole, managers maintain unilateral authority to block participation. Therefore, a necessary condition for success is that managers' private compensation from our experiment exceeds their private cost of effort. This level of compensation is realized across all cooperatives in the private payment arm, where the secretary and president retain near complete discretion over how incentives payments are allocated. In contrast, the frequency of opting out in the public payment arm indicates that public information constrains managerial returns.

The constraint on managers' share of surplus must be substantial given the structure of incentives. Cooperatives in the public payment arm would have received net positive payment at their baseline level of cleanliness with no change in milking practices. Therefore, simply accepting this revenue and distributing it among members would constitute a Pareto improvement relative to opting out.[23] Coase (1960) argues that in the absence of internal frictions, groups should be able to reach Pareto improving outcomes regardless of how the surplus is initially delivered. The fact that nearly a third of managers decline this option indicates that public information must introduce frictions. Furthermore, these frictions are substantial enough that many cooperatives cannot compensate management for bringing in a new source of free revenue.

Qualitative evidence around the decision to forego payment points to two candidate explanations for the limitations to managerial compensation. Managers who opted out made statements such as, "farmers [will be] angry about why the monetary reward is going to the cooperative when they were the ones who produced the milk" and "farmers will regularly start expecting payments." These statements are consistent with information either imposing coordination costs on secretaries or constraining the share of cooperative surplus claimed by managers.

The first possible explanation for managerial opt-out is that public information raises the cost of coordination within the cooperative. This situation would occur if producers inferred different information from our public announcement than they received from cooperative management.[24] In this case, managers would have to exert additional effort to coordinate beliefs about aggregate returns within the cooperative, meaning the net surplus from the public payment arm would be lower than that of the private payment arm *for the same level of payment*. The cost of coordination is likely higher for those with lower social status, and may be high enough that the cost of effort exceeds the revenue from the experiment. Coordination costs would be lower in the private payment arm where managers fully control messaging about revenue.

A second possibility is that announcements about payment limit managers' capacity to extract surplus by hiding income. Many producers in focus groups expressed frustration that cooperative management has substantial private information and therefore de facto control over the cooperative financial account. This concern is substantiated by discrepancies between positive accounting profits and (lack of) member recall about receiving dividends. Public announcement of experimental payments likely limits the control managers have over revenue from the experiment, and many secretaries worried that

[22] There are alternative approaches to providing incentive payments. For example, one approach would be to pay a uniform fraction of the group-level incentive directly to the producers. However, at the time of our study, the producers were largely unbanked and transacted mainly in cash. Our implementing partner required all payments to be made as bank transfers, which meant that the only logistically feasible option was to transfer the incentives to the cooperative bank account. Finally, our sample size and financial constraints prevented us from experimenting with alternate payment options.

[23] The aggregate welfare impacts net of the cooperative's production response are ambiguous because managers can induce effort on the part of producers using both social rewards and punishment. If managers face a binding limited liability or participation constraint when setting incentives for producers, then producer welfare cannot decrease and improved cleanliness must reflect greater aggregate welfare. However, if neither constraint binds, then producer-level behavior change may be motivated by the threat of punishment. This threat can be sufficiently immiserating to offset any welfare gains that accrue to managers, so it is possible that cleanliness improvements accompany a net decrease in aggregate welfare relative to the prior status quo.

[24] The information content of our announcement could diverge from messaging by cooperative management either because our communication was imprecise, leading farmers to be misinformed about the size of payments, or because the cooperative management shared incomplete/inaccurate information about payments and the return to cleanliness.

announcing payments may lead members to seek out financial information about the cooperative account more generally. Such revelation would be more costly for managers with lower social status who have fewer alternate ways to claim cooperative surplus,[25] and the small bump in revenue from our experiment may not have been worth the risk of losing a larger and more regular stream of private income. This explanation would indicate that information revelation lowers managers' share of surplus in a way that the network cannot commit to compensating after the information is revealed, and suggests control over information may be a way elites maintain their dominant position.

While we cannot quantitatively distinguish between these explanations, both provide a cautionary note regarding the potential for quality upgrading as a development policy. The quality improvements among cooperatives in the private payment arm indicate that aggregate outcomes can be affected by aggregate returns. However, the high frequency of foregoing payment in the public payment arm indicates that this response is sensitive to the way in which surplus is delivered. In particular, we find that incentives are more effective when discretionary control is left in the hands of cooperative management. In effect, the institutional design that allows more elite capture may be the most successful at achieving policy goals.

6. Conclusion

In this paper, we experimentally evaluate the effectiveness of group incentive contracts for quality upgrading in the context of village dairy cooperatives in Karnataka, India. We find that group incentives substantially improve production outcomes. A marginal payment of one percent of cooperative revenue over a two-week period induces an increase in cleanliness of up to 0.64 standard deviations. The improvement corresponds to nearly doubling the fraction of production suitable for high-value processing.

This first result offers hope for development initiatives targeted at small-scale producers that rely on market access for program beneficiaries, such as business asset transfer programs. We show that trade groups formed to enable market access in such settings can effectively respond to group-level price signals, even when individual output cannot be verified within the group.

However, when production teams are organized around existing social structures, we find both the potential for success and distribution of returns are mediated by the role of elites within the team. Quality upgrading was achieved in our study without explicit transfers to dairy producers. Indeed, in the private treatment arm, producers did not even know incentive contracts existed. Thus, quality improvement likely resulted from informal influence exerted by cooperative managers. The nature of this informal influence remains an open question for future research.

Our study specifically highlights control over information as a tool used by elites to maintain power. We present a case in which transparency places a binding check on some elites' capacity to allocate surplus, evidenced by cooperative secretaries' choice to forego payment in the presence of public information. In our setting, this constraint is most limiting to secretaries with the lowest social status, and so severe that many are willing to sacrifice a potential revenue stream entirely.

This second result indicates that for a development initiative to succeed, delivering net positive returns in the aggregate is not sufficient. Development programs must also guarantee positive private return to elites with blocking power. We provide evidence that compensation to elites is sensitive to the information environment within dairy cooperatives in a way that can derail policy goals. As a result, group incentives are more successful when managers retain full discretion over resulting incentive payment.

Our findings highlight a potential tradeoff between aggregate efficiency and distribution of rents in local policy. In our study, the most effective incentive structure is the one that admits the greatest potential for elite capture. More generally, in settings where elites have multiple ways to exert control over social surplus, efforts to promote equity by limiting elite power may have unintended consequences. It follows that policy may optimally allow for some elite capture to minimize distortion and maximize surplus. This tension is common to a broad range of policies targeted at decentralized populations and filtered through local governance.

This research provides a cautionary lesson for technological approaches to limiting corruption. Information disclosure or technological barriers to leakage may discipline elites and reduce their ability to extract surplus (Ferraz and Finan, 2008; Muralidharan et al., 2016; Banerjee et al., 2018; 2020), but transparency can also generate perverse incentives (e.g. Mejia and Parker, 2021; Desarronno et al., 2021). The results of this study reinforce the concern that policies to constrain elite power may backfire if they lead elites to seek out ways to circumvent these efforts entirely. These lessons are especially relevant in the context of recent advances such as electronic banking and mobile money that enable direct cash transfers intended to circumvent the possibility of expropriation along the payment delivery chain. While such innovations hold promise, they will only deliver benefits if implemented in ways that are sensitive to local social structure.

Overall, successful implementation may require buy-in from local elites who feel adequately compensated. Otherwise, alternative avenues of elite capture may leave the intended beneficiaries even worse off. It remains an open question how to balance aggregate efficiency with distributional goals, and the optimal design of group incentives across the social hierarchy in village economies paves the way for future work.

[25] See Appendix E for one possible formalization of the substitutability between extraction through surplus-sharing and extraction through hiding income.

Declaration of Competing Interest

This study was preregistered with the AEA RCT Registry under ID Number AEARCTR-0000700.

Research was conducted with approval from the Committee on the Use of Humans as Experimental Subjects at the Massachusetts Institute of Technology (COUHES #1405006372) and by the Institutional Review Board at the Institute for Financial Management and Research in Chennai, India.

We received financial support from the George and Obie Shultz Fund at MIT, the Agricultural Technology Adoption Initiative, USAID Development Innovation Ventures, and the NSF Graduate Research Fellows Program.

All data and replication code will be made publicly available upon publication.

The authors have no financial interests to disclose, and no party had the right to review the paper prior to its circulation.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jebo.2023.06.002.

References

Aflagah, K., Bernard, T., Viceisza, A., 2022. Cheap talk and coordination in the lab and in the field: collective commercialization in senegal. J Dev Econ 154. doi:10.1016/j.jdeveco.2021.102751.
Alatas, V., Banerjee, A., Hanna, R., Olken, B.A., Purnamasari, R., Wai-Poi, M., 2019. Does elite capture matter? local elites and targeted welfare programs in indonesia. AEA Papers and Proceedings 109, 334–339. doi:10.1257/pandp.20191047.
Alatas, V., Banerjee, A., Hanna, R., Olken, B.A., Tobias, J., 2012. Targeting the poor: evidence from a field experiment in indonesia. American Economic Review 102 (4), 1206–1240. doi:10.1257/aer.102.4.1206.
Atkin, D., Donaldson, D., 2015. Who's Getting Globalized? The Size and Implications of Intra-national Trade Costs. Working Paper 21439. National Bureau of Economic Research doi:10.3386/w21439.
Balán, P., Bergeron, A., Tourek, G., Weigel, J., 2022. Local elites as state capacity: how city chiefs use local information to increase tax compliance in the d.r. congo. American Economic Review forthcoming. doi:10.1093/qje/qjab029.
Bandiera, O., Best, M.C., Khan, A.Q., Prat, A., 2021. The allocation of authority in organizations: afield experiment with bureaucrats. Q J Econ 136 (4), 2195–2242. doi:10.1093/qje/qjab029.
Banerjee, A., Duflo, E., Imbert, C., Mathew, S., Pande, R., 2020. E-governance, accountability, and leakage in public programs: experimental evidence from a financial management reform in india. American Economic Journal: Applied Economics doi:10.1257/app.20180302.
Banerjee, A., Hanna, R., Kyle, J., Olken, B.A., Sumarto, S., 2018. Tangible information and citizen empowerment: identification cards and food subsidy programs in indonesia. Journal of Political Economy 126 (2), 451–491. doi:10.1086/696226.
Banerjee, A., Mookherjee, D., Munshi, K., Ray, D., 2001. Inequality, control rights, and rent seeking: sugar cooperatives in maharashtra. Journal of Political Economy 109 (1), 138–190. doi:10.1086/318600.
Bardhan, P., 2002. Decentralization of governance and development. Journal of Economic Perspectives 16 (4), 185–205. doi:10.1257/089533002320951037.
Bellemare, M., Bloem, J., 2018. Does contract farming improve welfare? a review. World Dev 112 (C), 259–271.
Belloni, A., Chernozhukov, V., Hansen, C., 2013. Inference on treatment effects after selection among high-dimensional controls. Rev Econ Stud 81 (2), 608–650. doi:10.1093/restud/rdt044.
Bergquist, L.F., Dinerstein, M., 2020. Competition and entry in agricultural markets: experimental evidence from kenya. American Economic Review 110 (12), 3705–3747. doi:10.1257/aer.20171397.
Bernard, T., Collion, M.-H., de Janvry, A., Rondot, P., Sadoulet, E., 2008. Do village organizations make a difference in african rural development? A study for senegal and burkina faso. World Dev 36 (11), 2188–2204. doi:10.1016/j.worlddev.2007.10.010.
Bernard, T., Dänzer, P.N., Frölich, M., Landmann, A., Viceisza, A., Wouterse, F., 2021. Building trust in rural producer organizations: results from a randomized controlled trial. Agricultural and Resource Economics Review 1–20. doi:10.1017/age.2021.17.
Bernard, T., de Janvry, A., Mbaye, S., Sadoulet, E., 2017. Expected product market reforms and technology adoption by senegalese onion producers. Am J Agric Econ 99 (4), 1096–1115. doi:10.1093/ajae/aax033.
Bold, T., Ghisolfi, S., Nsonzi, F., Svensson, J., 2022. Market access and quality upgrading: evidence from four field experiments. American Economic Review 112 (8), 2518–2552. doi:10.1257/aer.20210122.
Casaburi, L., Macchiavello, R., 2015. Loyalty, exit, and enforcement: evidence from a kenya dairy cooperative. American Economic Review 105 (5), 286–290. doi:10.1257/aer.p20151076.
Coase, R.H., 1960. The problem of social cost. Journal of Law and Economics 3, 1–44.
de Janvry, A., Sadoulet, E., Suri, T., 2017. Chapter 5: field experiments in developing country agriculture. In: Banerjee, A.V., Duflo, E. (Eds.), Handbook of Economic Field Experiments. In: Handbook of Economic Field Experiments, Vol. 2. North-Holland, pp. 427–466. doi:10.1016/bs.hefe.2016.08.002.
Desarronno, E., Le'on-Ciliotta, G., Witoelar, F., 2021. When transparency fails: Financial incentives for local banking agents in indonesia. Mimeo.
Dragusanu, R., Montero, E., Nunn, N., 2022. The effects of fair trade certification: evidence from coffee producers in costa rica. J Eur Econ Assoc 20 (4), 1743–1790. doi:10.1093/jeea/jvac026.
Ferraz, C., Finan, F., 2008. Exposing corrupt politicians: the effects of brazil's publicly released audits on electoral outcomes. Q J Econ 123 (2), 703–745. doi:10.1162/qjec.2008.123.2.703.
Goodhue, R.E., Mohapatra, S., Rausser, G.C., 2010. Interactions between incentive instruments: contracts and quality in processing tomatoes. Am J Agric Econ 92 (5), 1283–1293. doi:10.1093/ajae/aaq061.
Hasanain, A., Khan, M. Y., Rezaee, A., 2022. No bulls: Experimental evidence on the impact of veterinarian ratings in pakistan. Mimeo.
Hill, R.V., Maruyama, E., Olapade, M., Frölich, M., 2021. Strengthening producer organizations to increase market access of smallholder farmers in uganda. Agricultural and Resource Economics Review 1–29. doi:10.1017/age.2021.19.
Hsieh, C.-T., Olken, B.A., 2014. The missing 'missing middle'. Journal of Economic Perspectives 28 (3), 89–108. doi:10.1257/jep.28.3.89.
Hussam, R., Rigol, N., Roth, B.N., 2020. Targeting High Ability Entrepreneurs Using Community Information: Mechanism Design in the Field. Technical Report 20–082.
International Labour Organization, 2014. Cooperatives and the sustainable development goals. International Labour Organization.
Jakiela, P., Ozier, O., 2015. Does africa need a rotten kin theorem? experimental evidence from village economies. Rev Econ Stud 83 (1), 231–268. doi:10.1093/restud/rdv033.
Kosfeld, M., Rustagi, D., 2015. Leader punishment and cooperation in groups: experimental field evidence from commons management in ethiopia. American Economic Review 105 (2), 747–783.
Lowder, S.K., Skoet, J., Raney, T., 2016. The number, size, and distribution of farms, smallholder farms, and family farms worldwide. World Dev 87, 16–29. doi:10.1016/j.worlddev.2015.10.041.

Macchiavello, R., Miquel-Florensa, J., 2019. Buyer-Driven Upgrading in GVCS: The Sustainable Quality Program in Colombia. SSRN Scholarly Paper ID 3464455. Social Science Research Network, Rochester, NY. https://papers.ssrn.com/abstract=3464455
Macchiavello, R., Morjaria, A., 2021a. Acquisitions, management, and efficiency in rwandas coffee industry. Mimeo.
Macchiavello, R., Morjaria, A., 2021. Competition and relational contracts in the rwanda coffee Chain. Q J Econ 136 (2), 1089–1143. doi:10.1093/qje/qjaa048.
MacKinnon, J.G., Webb, M.D., 2020. Randomization inference for difference-in-differences with few treated clusters. J Econom doi:10.1016/j.jeconom.2020.04.024.
Markelova, H., Meinzen-Dick, R., Hellin, J., Dohrn, S., 2009. Collective action for smallholder market access. Food Policy 34 (1), 1–7. doi:10.1016/j.foodpol.2008.10.001.
Mejia, J., Parker, C., 2021. When transparency fails: bias and financial incentives in ridesharing platforms. Manage Sci 67 (1), 166–184. doi:10.1287/mnsc.2019.3525.
Mitra, S., Mookherjee, D., Torero, M., Visaria, S., 2018. Asymmetric information and middleman margins: an experiment with indian potato farmers. Rev Econ Stat 100 (1), 1–13.
Montero, E., 2022. Cooperative property rights and development: evidence from land reform in el salvador. Journal of Political Economy forthcoming. doi:10.1086/717042.
Muralidharan, K., Niehaus, P., Sukhtankar, S., 2016. Building state capacity: evidence from biometric smartcards in india. American Economic Review 106 (10), 2895–2929. doi:10.1257/aer.20141346.
Murphy, S.C., Martin, N.H., Barbano, D.M., Wiedmann, M., 2016. Influence of raw milk quality on processed dairy products: how do raw milk quality test results relate to product quality and yield? J. Dairy Sci. 99, 10128–10149.
Niehaus, P., Sukhtankar, S., 2013. Corruption dynamics: the golden goose effect. American Economic Journal: Economic Policy 5 (4), 230–269. doi:10.1257/pol.5.4.230.
Ostrom, E., 1990. Governing the Commons: The Evolution of Institutions for Collective Action. Cambridge University Press.
Platteau, J.-P., 2009. Chapter 2: information distortion, elite capture and task complexity in decentralized development. In: Ahmad, E., Brosio, G. (Eds.), Does Decentralization Enhance Service Delivery and Poverty Reduction?. Edward Elgar Publishing, Cheltenham, UK doi:10.4337/9781849801850.00008.
Reinikka, R., Svensson, J., 2004. Local capture: evidence from a central government transfer program in Uganda. Q J Econ 119 (2), 679–705. doi:10.1162/0033553041382120.
Saenger, C., Qaim, M., Torero, M., Viceisza, A., 2013. Contract farming and smallholder incentives to produce high quality: experimental evidence from the vietnamese dairy sector. Agricultural Economics 44 (3), 297–308. doi:10.1111/agec.12012.
Saenger, C., Torero, M., Qaim, M., 2014. Impact of third-party contract enforcement in agricultural marketsa field experiment in vietnam. Am J Agric Econ 96 (4), 1220–1238.
Treurniet, M., 2021. The potency of quality incentives: evidence from the indonesian dairy value chain. Am J Agric Econ 103 (5), 1661–1678. doi:10.1111/ajae.12176.
Wanyama, F., 2014. Cooperatives and the Sustainable Development Goals: A Contribution to the Post-2015 Development Agenda. International Labor Organization.
World Bank, 2020. World Development Report 2020: Trading for Development in the Age of Global Value Chains. The World Bank.