Got (clean) milk? Transparency, governance, and incentives for cleanliness in Indian dairy cooperatives

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Abstract

We implement a randomized evaluation to pay for lower bacteria count among cooperative dairy producers in rural Karnataka. Incentives for cleaner milk are applied at a group level and paid into a shared cooperative bank account. We further vary whether incentive payments are announced publicly to cooperative members or revealed privately to cooperative management alone. Results show that group incentives are sufficient to improve production quality in village cooperatives. However, this result is sensitive to the way in which incentives are administered. When faced with the prospect of public announcement, managers in a third of cooperatives opt out of receiving incentive payments entirely, undermining any possible incentive effect. Dropout is most common among cooperatives with weaker management oversight. We argue that the decision to opt out only in the face of publicly announced incentives seems inconsistent with the Coase Theroem and other simple models of joint utility maximization.

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1 Introduction

Joint production processes are common throughout the developing world. Cooperative agriculture, farmer-producer organizations, self-help groups, and a number of other structures exist to help small-scale producers aggregate their output and sell to a broader market. High individual transaction costs limit the ability of a centralized authority to set individulized incentives to achieve specific production goals. However, these production processes are often characterized by informal social relationships between members that enable them to aggregate local information and take advantage of complementarities related to group incentives.

In this paper we study joint production in one such cooperative structure: the Karnataka Milk Federation (KMF). The KMF collects over 2 million gallons of milk daily from over 2.4 million producers located in over 22,000 villages throughout Karnataka. Milk products are processed and packaged for distribution nationally under the brand name Nandini, with surplus profits nominally returned to farmers.

One challenge faced by the KMF is bacteria in milk production. The presence of bacteria limits profitability by preventing milk from being used in high-value products such as buttermilk, yogurt, and sweets. Instead, milk with high bacteria count is limited to use in ultra-pasteurized shelf-stable liquid packets. The KMF has combated bacterial growth through supply chain innovations such as strict milk delivery schedules and rural refrigeration technology. We investigate the potential to supplement these actions by promoting cleaner milking practices among producers at the cooperative level.

The central body of the KMF is constrained in setting incentives for milk cleanliness by two key factors. First, individual milk quality is effectively untraceable because villagers pour milk together for aggregate delivery to a processing plant. Therefore, incentives for cleanliness can at best be assigned to a cooperative as a whole, and cannot be further decentralized to individual producers. Second, quality testing at the point of collection is logistically infeasible because it requires laboratory equipment and conditions. This technical barrier limits the ability of a central body to assign group incentives even at the village level.

Our research evaluates whether local village institutions are sufficiently strong to overcome the first challenge. We conduct a randomized evaluation that offers group incentives to cooperatives for lowering the bacteria count in milk. We evaluate milk cleanliness through high-cost monitoring, ensuring that incentives are accurately assigned at the group level. Thus we investigate whether, given the appropriate aggregate incentive, villages have enough local information and capacity for enforcement to produce cleaner milk.

Our results indicate that group incentives are sufficient to improve production quality in village cooperatives. However, this result is sensitive to the way in which incentives are applied. Among incentivized villages, we randomly vary whether incentives are announced publicly to all villagers or privately to only cooperative management. When faced with the prospect of public announcement, managers in a third of cooperatives opt out of receiving incentive payments entirely, undermining any possible incentive effect. The decision to opt out only in the face of public announcement of incentives seems inconsistent with the Coase Theroem and other simple models of joint utility maximization.

This paper relates to several stands of literature. First, we provide evidence on the capacity for gains from decentralization of production processes. Decentralization can better enable agents to incorporate local information (Aghion and Tirole, 1997; Acemoglu et al., 2007), but it may also generate unintended consequences that raise agents ability to shirk (Shenoy, 2019). This tradeoff between local information and monitoring capacity is seen in other development contexts as well. Notably, Alatas et al. (2012) and Hussam et al. (2017) demonstrate that local agents have better information on how to allocate funding to its intended targets. However, a large body work shows that decentralized decision-making authority can lead to corruption and elite capture (e.g. Reinikka and Svensson, 2004; Olken, 2007; Niehaus and Sukhtankar, 2013).

Banerjee et al. (2012) note that the costs of corruption depend crucially on the incentives faced by agents in a position to be corrupt; if performance targets align with private returns, then allowing some corruption may actually improve outcomes (e.g. Weaver, 2019). In the context of cooperative agricultural production, Banerjee et al. (2001) and **?** provide evidence on how private incentives can skew the behavior of cooperative leaders. This paper directly extends this work by highlighting how program design decisions may affect private incentives in unanticipated ways.

In Section 2 of this paper we describe the setting and production process in more detail. Section 3 outlines our experimental design, and Section 4 presents results. We place the results into a theoretical framework in Section 5, and Section 6 concludes.

2 Setting

The KMF, founded in 1974, collects, processes, and distributes milk from over 2.4 million producers in over 22,000 villages across Karnataka. Milk production is organized through village-level Dairy Cooperative Societies (DCSs) that aggregate output from smallholder farmers for delivery to processing plants. The typical DCS member in the KMF owns between 1 and 2 producing cows, and earns from 20–30% of their income from dairy activities. The supply chain operates at an impressive scale, turning around milk from a large geographical area for national distribution in a matter of days.

2.1 Supply Chain

Dairy production originates in rural villages with smallholder producers coordinating on a common time to milk cows. At the village-level DCS collection stage, each producer's milk is inspected by sight and smell for spoilage as well as tested for density to prevent adulteration before being poured into the common village can. Village collection typically takes place early in the morning, with all producers delivering milk within a half-hour window. Once full, cans are sealed for immediate pickup and delivery to a processing plant.

At the processing plant, milk from a DCS is rapidly chilled, with a small sample withheld for testing. Following a lab test for bacteria and adulterants, milk is assigned to its appropriate use. Milk with high bacteria count or presence of adulterants must be pasteurized for sale in liquid form, while cleaner milk can be used in value-added goods such as yogurt, buttermilk, or sweets. Milk products packaged by the KMF are distributed nationwide in India under the brand name Nandini.

2.2 Payment

DCSs are paid for milk delivered based on quantity, with very slight adjustments for fat content. During the period of this study, these payments were made fortnightly into the DCS financial account, out of which producers are paid based on their own individual production quantity. There is a slight wedge between the price received by the DCS from the KMF and that paid to producers, which generates a surplus over the course of the year.

A portion of the annual surplus is used to pay for local facilities and maintenance, as well as salaries for local DCS staff. The remainder is intended to be distributed among farmers as a yearly bonus, pro-rated by production quantity. The KMF supplements this local surplus by returning a portion of its annual profits as a dividend to producers. In the several years leading up to our study, the KMF reported net surpluses for all DCSs participating in our study every year, indicating that they should have paid bonuses to farmers. However, only 20% of farmers surveyed could remember ever receiving a bonus from the DCS, revealing a disconnect between official accounting and actual use of funds.

2.3 Governance

Each DCS is managed by a president and secretary, each elected to their position for staggered 10-year terms. 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 cooperative affairs. Most notably, the secretary oversees daily milk collection. The president and secretary are overseen by a board of directors typically consisting of 9–10 members. The board is composed of local member producers, though the selection process varies idiosyncratically by village.

In Table 1 we present demographic characteristics of DCS producers, secretaries, board members, and presidents in our area of study. It is clear from this table that DCS presidents typically enjoy higher social status: They are wealthier and more educated on average than producers and directors, they are less likely to belong to a scheduled caste or tribe, and are more likely to have held a position as a Member of the Local Assembly. While secretaries' demographic characteristics are more in line with typical producers, the one notable exception is in education. DCS secretaries have on average twice as many years of education as the typical producer.

Social perceptions of DCS secretaries relative to directors correspond to this disparity in education. Table 2 presents each group's subjective perceptions of other groups. The rows correspond to beliefs about a group, and the columns correspond the group giving the evaluation. The table includes data on perceptions of social power/standing, management capacity, and and knowledge of dairy practices. Two facts stand out from the table. First, DCS management typically has a higher opinion of other managers than other producers' opinion of managers. Second, all groups evaluate secretaries higher than they evaluate members of the board of directors.

These differences in the characteristics of cooperative manages demonstrate the potential for elite capture in this setting. In particular, the cooperative managers in charge of the DCS 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.

2.4 Milk Cleanliness

Into this setting, we introduce group-level incentives for lowering the bacteria count in milk. The primary value of cleaner milk is realized in value added during processing. While the supply chain and pasteurization technology are sufficiently advanced that outright milk spoilage almost never occurs, cleaner milk generates greater profitability for the KMF the value-added processing. Higher profits are in theory returned to farmers in the form of dividend payments.

One large barrier to improving cleanliness at the DCS level stems from the KMF's inability to assign appropriate incentives. This is infeasible for two reasons: first, it would be prohibitively expensive to test individual-level milk quality. With multiple million producers each pouring small quantities daily, there is no way to directly track and test milk from individual producers. Therefore, the KMF lacks sufficient information to provide incentives at the individual level.

Second, even at the group level, technological constraints hinder the application of incentives. Bacteria testing must take place in central labs as the KMF lacks the personnel and many villages lack appropriate facilities for decentralized field testing. Unfortunately, since bacterial load grows exponentially with time, transportation from the field to the lab introduces a great degree of variance into the testing process. Linking incentives to milk cleanliness at large scales would require overcoming logistical hurdles to prevent the signal recovered by the test from being swamped by variance introduced in the testing procedure.

In this project we alleviate the second barrier by conducting high-intensity milk testing for a small sample of villages. We offer incentives to a subset of these village DCSs linked to the bacterial load in their milk, under the assumption that local producers have greater information about local production practices than external observers. While incentives applied at a group level can be ineffective due to challenges in collective action and free riding, we test whether informal social relationships in villages are sufficiently strong to overcome these challenges and collectively avail a group benefit.

3 Experimental Design

We implement a randomized evaluation of incentives to pay producers for lowering the bacteria count in milk. Villages assigned to receive payment are randomly selected into either having incentive payments announced publicly to all producers or privately to only cooperative management, and all payments are made into the cooperative bank account. We evaluate milk cleanliness through two rounds of baseline testing and two rounds of intervention testing.

3.1 Intervention and Randomization

To promote clean practices in milk production, we introduce financial incentives for cleaner milk among DCSs in the Dharwad district of Karnataka in India. Randomization takes place at the DCS (village) level, and all cleanliness data is collected at the village level as well. After milk testing, all incentive payments are paid into the DCS financial account.

Among those villages offered incentives, we randomly vary whether the incentive is announced publicly to cooperative members or privately to the secretary and president alone. For public announcement, we select a random twenty producers who pour their milk at the time of collection and announce the payment schedule. Similarly, at the time of payment for the public announcement arm, we select another twenty random producers who pour milk and announce the realized payment.

The intervention follows two rounds of incentivized milk collection. In each round, we first announce the payment schedule, either privately or publicly according to treatment assignment, and then give a two-week window during which we may return for testing. We then pick a random day in the two-week window to return during the milk collection period. Immediately after milk collection, we collect a milk sample and a can swab to refrigerate and take to a lab for testing. We then return to deliver payment and announce it as appropriate on the following day.

Payments range from Rs. 0 to Rs. 2,000, equivalent to roughly $40, depending on the quality of milk. At the high end, this level of payment is equivalent to just under one month's average salary from the cooperative for a DCS secretary, and just under 80% of a month's self-reported total earnings for the average dairy producer. The payment schedule is scaled so that the average payment from baseline data would be Rs. 500, or roughly $10. These values represent the total payment to be made to the cooperative to be slpit among the management and producers in whatever way they decide.

Figure 1 shows the treatment assignment across the two rounds of intervention. In Round 1, there are 19 village assigned to control, 19 villages assigned to private payment, and 13 villages assigned to public payment. Between Rounds 1 and 2, 6 villages switch from control to public payment and 3 villages switch from private to public payment.[1] There are no villages that switch in the other direction because public announcement of payments is an absorbing state; we cannot credibly take back the knowledge that incentives will be paid.

Table 3 provides descriptive statistics for the treatment and control groups. The two groups appear balanced on covariates; only the fraction of income earned from dairy differs significantly between the two. Importantly, there are no statistically significant differences between treatment and control in average quantity poured, cleanliness, or number of livestock.

3.2 Data Collection and Analysis

We collect four rounds of data on milk cleanliness and conduct baseline surveys with DCS management and a random sample of producers in each village, as well as an endline surveys of another random sample of producers.

3.2.1 Milk Testing

The main data from the experiment come from lab testing of milk samples and can swabs from each DCS. We collect two rounds of baseline testing data before any incentives are annouced and two rounds of intervention testing data. We employ two tests of bacteria count.

Methylene Blue Reduction Test (MBRT): MBRT involves adding a blue dye to milk and measuring the time until the dye completely disappears. Reduction of the dye is accelerated by removal of dissolved oxygen, typically caused by bacteria in the milk. This test has the advantage of directly testing the bacterial load in solution. However, different microbes produce different compounds through regular metabolism that have varying effects on dye reduction. Thus, test results may vary by type of bacteria rather than just count.

Standard Plate Count (SPC): The SPC is performed by taking a swab of liquid residue from a container and analyzing it under a microscope to count the bacterial density. Unlike the MBRT, SPC is not sensitive to different types of bacteria as all organisms are counted on a slide. However, SPC measures bacterial load in residue from the container rather than directly in solution, and therefore may be overly sensitive to cleaning of the equipment swabbed relative to other cleanliness practices.

For our main analysis, we combine the two measures using principal components analysis. We identify the primary principal component as a joint measure of cleanliness that we use to quantify outcomes. However, incentive payments are tied only to MBRT as that is the measure cooperative members were most familiar with and the one the KMF identified as the most likely to be used if cleanliness-based payments were to be implemented statewide.

[1] Motivation for changing treatment assignment mid-intervention is discussed in Appendix A.

For results on testing-related outcomes, we implement a difference-in-differences estimation strategy. The estimating equation is

$$Y_{jt} = \beta^{Pr} T_{jt}^{Pr} + \beta^{Pu} T_{jt}^{Pu} + \gamma_j + \delta_t + \epsilon_{jt} \tag{1}$$

where j indexes DCSs and t indexes testing rounds. The variables T^{Pr} and T^{Pu} are dummies representing assignment to either private or public incentive treatment arms in round t, and both dummies are 0 for all DCSs in the two baseline rounds of observation. γ and δ represent DCS and time fixed effects, respectively.

3.2.2 Survey Data

We supplement the milk quality tests with two rounds of survey data. At baseline, we survey twenty producers at each DCS randomly selected from the population pouring milk on the day of the visit. Baseline questions include information on demographics, income, dairy production practices, knowledge and beliefs about cleanliness, and subjective evaluations of cooperative governance. We also administer a baseline questionnaire to DCS secretaries, directors, and presidents covering their demographics, dairy involvement, cooperative management, and subjective evaluations of cooperative governance. After the final round of intervention milk testing, we administer an endline questionnaire to another randomly sampled twenty producers per village. The endline questionnaire covers information on demographics, dairy involvement, and beliefs about clean milking practices, cooperative governance, and implementation of the experiment.

Survey data exists at the individual level, but is not available as a panel because the sample of respondents is redrawn between baseline and endline. Therefore, all analysis using outcomes from survey data relies on a simple difference between treatment and control, with the mean village baseline level included as a control when available. The estimating equation is

$$Y_{ij} = \beta^{Pr} T_j^{Pr} + \beta^{Pu} T_j^{Pu} + \gamma \bar{Y}_j + \delta + \epsilon_{ij} \tag{2}$$

where i indexes individual producers in village j. When villagers belong to a DCS that switched treatment assignment, they are assigned $T^{Pu} = 1$ and $T^{Pr} = 0$ if their village was ever assigned to public incentives.

3.3 Timeline

The timeline of project activities revolves around seasonality in dairy production. The two-year production cycle starts with gestation, which lasts around nine months. Milk production peaks around 2 months after calving and remains high for another 6–7 months before tapering off, ending around one year after a calf is born. The cow then goes into a 2–3 month dry rehabilitation period before it is once again ready for insemination. In Karnataka during the time of study, the lean dairy season falls around January–April, with peak production in the months of May–December.

The baseline survey for this study took place in July–August, 2014, followed by two rounds of baseline milk testing in September–October. Program activities then paused through the subsequent dry season to prevent contamination from baseline data collection on endline activities. Following the dry season, state-wide elections took place in June, 2015. Because the KMF is a state-run organization, all project activities were placed on hold in the run-up to elections. Milk testing resumed shortly after elections, with the two rounds of intervention milk testing in July–August, 2015, followed immediately by endline surveying at the end of August, 2015. A full timeline of project activities is given in Figure 2.

intervention timeline also an election; KFM is state-run

4 Results

Results indicate that villages receiving payment for milk testing improve the cleanliness of milk. However, in the public payment arm, seven of twenty-two DCS secretaries opted out of receiving payment rather than allowing the payment to be announced publicly. Secretaries that opted out belong to DCSs that have lower quality at baseline and whose management is perceived to be weaker. We find little evidence of treatment impacts on cooperative outreach to farmers or on perceptions of governance.

4.1 Cleanliness

We find evidence that being exposed to incentives leads to an increase in milk cleanliness, with results presented in Table 4. Column 3 presents the main specification results from (1). In the private incentive arm quality increases by 0.64 standard deviations, but in the public incentive arm it only increases by 0.32 standard deviations. However, the latter value is attenuated by a high degree of non-compliance among those assigned to public incentives, discussed in greater detail below. Using treatment assignment as an instrument for receiving payment in column 4, we find that receiving public incentives increases cleanliness by 0.39 standard deviations, but this increase is neither statistically indistinguishable from 0 nor from being equal to the increase among the private incentive arm. It is not clear whether the difference between treatment arms is due to public announcement providing weaker incentives or the non-compliance selecting a different set of villages on which the average treatment effect is measured.

We document the sources of the improvement in cleanliness in Table 5. Results indicate both a decline in log SPC as well as an increase in MBRT time, though neither result is statistically significant on its own. The final column of the table analyzes treatment impact on the average quantity produced by cooperative members. We find no effect on producer quantity in the private incentive arm, but an increase in quantity poured at the cooperative in the public treatment arm. It should be noted that this result only measures quantity produced conditional on delivering to the cooperative. It is possible that there is adjustment on the extensive margin by the inclusion or exclusion of producers in the private incentive arm, or that the increase is driven by inclusion of larger producers in the public incentive arm, but total quantity or number of pourers is unobserved to us.

4.2 Payment

Consistent with the improved quality, we observe an increase in payments made to incentivized DCSs. The first two columns of Table **??** show the increase in cleanliness payments made to treated DCSs relative to the counterfactual payment a control DCS would have earned in the two intervention rounds. Relative to control, the improvement in cleanliness among treated DCSs generates an additional Rs. 100 per collection day for the cooperative in the private incentive arm. In total, treated DCSs are paid on average Rs. 750, or roughly $15. This value is one third of a typical DCS secretary's monthly earnings from the DCS, and just over a quarter of the monthly dairy earnings for a typical cooperative member.

The final column of Table **??** indicates why treatment effects are smaller among DCSs with public incentives. In the second round of intervention, seven out of twenty-two cooperative secretaries opted to forego payments entirely rather than accept publicly announced payments. In all cases, the secretaries offered to continue accepting payments if they were made privately to the DCS without public announcement, and all seven consented to continued milk testing without payment. There was no pattern in the propensity to opt out based on treatment assignment in Round 1. The decision to forego payment is discussed in much greater detail below.

4.3 Margins of Adjustment

Tables 7 and 8 give some insight into the effect of incentives on village interactions. The first column of Table 7 indicates that knowledge of the presence of payments increased by a statistically significant amount among villages in which the incentive was ever publicly announced. The point estimate is small, however, at only fifteen percentage points. Thus, even in incentivized villages, a large number of producers did not directly know they may be rewarded for production cleanliness.

The second column of Table 7 reports results of whether producers recalled being told about cleanliness by the DCS. The small, insignificant result suggests the DCS took little direct action to change behavior among producers. Instead, most incentivized DCSs appear to have increased cleanliness through actions of the secretary in sanitizing the village collection equipment before daily collection. Although we lack quantitative data on this channel, secretaries could be seen cleaning equipment during the intervention rounds, and subsequently reported doing so in post-experiment debriefs.

While secretaries in villages receiving incentives increased their cleaning practices, the intervention seems to have had the opposite effect on farmers' perception. Among treated villages, producers were more likely to increase their perception of the cleanliness of other producers in intervention villages, but likely to decrease their perception of the cleanliness of the DCS facilities maintained by the secretary. A likely possibility is that the presence of incentives increased the salience of milk cleanliness, which caused increased farmers' sensitivity to the low baseline levels of sanitation at collection centers.

Table 8 investigates whether this perception of cleanliness spills over onto other measures of satisfaction in DCS governance. We find no evidence of changes in farmers' beliefs about secretaries' managerial capacity or the frequency with which they ask for bonuses from the DCS. There is a marginally significant increase in the fraction of farmers that recall receiving a bonus in treated villages relative to control. However, the most salient change in this outcome is in the control, which increases from 20% in the baseline survey to over 80% at endline. This drastic increase came about because of a statewide bonus paid out by the (state-owned) KMF to all farmers in advance of elections in May, 2015.

4.4 Selection into Opting Out

We did not anticipate opting out of payments while consenting to quality measurement would be an outcome in this experiment. In this section we explore some characteristics of the DCSs that drop out. We start by plotting the event-study version of (1) in Figure 3. The figure confirms the regression results that show a greater increase in milk cleanliness among villages receiving public payment relative to private payment.

Figure 4 breaks down the public payment event study into those that participate and those that opt out in the second round. The figure reveals two important facts: first, those DCSs that drop out have ex ante lower milk quality than those that remain in the experiment. In other words, there appears to be selection on the size of prospective payments. 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 opting out remains nearly flat. This is slightly misleading; if the cleanest villages remain in the incentive arm, then we would expect to see even cleaner milk among the public payment arm were it to have the same size treatment effect as private payments. However, the trend line is increasing relative to control, indicating that public payments generate an incentive effect among those villages that consent to receive them.

In Table 9 we report other predictors of opting out from public payments. The table gives the difference in baseline mean between those DCSs assigned to Round 2 public payment that remained in the experiment and those that dropped out. The consistent trend across all variables is that DCSs whose secretaries opted out have weaker indicators of management quality. The most statistically significant variables are frequency of meetings among the board of directors, public perceptions of director power, and public perceptions of director management ability. These factors indicate DCS secretaries are more likely to opt out of payment when they experience weaker oversight. However, public perception of secretary power is also lower among DCSs that opt out, indicating that this is not a case of a strong secretary overruling a weak board of directors. Instead, it seems to be the case that weak management in general leaves a DCS averse to publicly announced payments.

5 Discussion

The decision to opt out of the experiment in the case of public incentives appears to be a violation of the Coase Theorem. Treatment guarantees a strictly non-negative shock to cooperative earnings. Even if there were extremely high transactions costs or cost of cleaning, the cooperative would stand to benefit by remaining in the study and not changing their behavior. Moreover, the quality improvements seen in the private incentive arm demonstrate this is not the case. When incentives are made known only to the DCS secretary and president, there is an observable improvement in production quality. In this section we present a simple model to explain why this behavior is puzzling and suggest some possible resolutions.

Consider a cooperative represented by a secretary S and a single representative producer P. Each party i puts in effort X_i with private, linear cost, to generate an output given by the production function $Y = (X_S, X_P)$. Let the production function be increasing and concave in both arguments. They then split the returns and consume portions Y_S and Y_P, respectively, with increasing concave utility $U_i(Y_i)$. We introduce transaction costs as an iceberg penalty τ to producers' consumption, so that the cost of assigning a level of consumption Y_P is $Y_P(1+\tau)$.

We can solve the planner's problem in this economy by assigning Pareto weight α to the secretary. The planner's problem is then

$$\max_{X_S,X_P,Y_S,Y_P} \alpha[U_S(Y_S) - X_S] + (1-\alpha)[U_P(Y_P) - X_P] \tag{3}$$
$$\text{s.t. } Y_S + (1+\tau)Y_P = f(X_S, X_P)$$

The first-order-conditions from optimization show that effort and consumption at the optimum are separable conditional on the multiplier on the budget constraint. That is, the problem can be broken into the village separately optimizing the allocation of effort and of consumption conditional on a total output level Y, and then choosing the optimal output level.

Let the introduction of incentives for cleanliness be represented by an increase in the partial derivatives $f_1(\cdot)$ and $f_2(\cdot)$ at all levels of effort, with the increase being strict over some range. Then it must be the case that

Proposition 1. *If an intervention increases $f_1(\cdot)$ and $f_2(\cdot)$ in the maximization given by (3), with* *the increase being strict over some range, then the maximized utility for agents i, $U_i(Y_i^*)$, can be no lower than before the increase.*

Proof. (Sketch) In the case where $f_1(\cdot)$ and $f_2(\cdot)$ do not change at the optimum, the proposition is trivial. When at least one of them changes, then the FOC for that agent's effort is no longer satisfied. To return to equilibrium, that agent must increase effort, which raises overall output and lowers the multiplier λ. Each agent's utility is increasing in aggregate output, so neither agent can be worse off. □

Intuitively, after the change the secretary can always choose the same level of effort as before. The producer will never decrease effort as a result of the change, and therefore aggregate output will be no lower than before. Since each agent's utility is increasing in aggregate output, the secretary will be no worse off than before.

It is possible that income from our experiment is viewed differently from regular dairy income and is therefore allocated differently. Even in this case, no agent would be worse off with the incentive than without the bonus incentive.

Proposition 2. *(Sketch) Let the optimum in (3) be fixed, and in response to an increase in the partial derivatives $f_1(\cdot)$ and $f_2(\cdot)$ the planner allocates only the additional surplus $Y' - Y$ according to different Pareto weights given by α' with different transaction cost given by τ'. Even in this case, the maximized utility for agents i, $U_i(Y_i^*)$, can be no lower than before the increase.*

Proof. (Sketch) Basically the same as before. □

As a corollary to the propositions, the secretary should never privately choose to reject a proposal that increases aggregate village income. We propose three extensions that may rationalize the decision to opt out of participation in the case of public payments but not in the case of private payments.

First, there may be differences in timing between the exertion of effort and allocation of returns, with limited commitment between rounds. In this case, producers may request a high level of effort from the secretary with the promise of remuneration later, but renege at the time of payment. We require two additional assumptions for this explanation to be credible. First, there must be some enforcement of the secretary's effort in the first period; otherwise the secretary would anticipate the renegotiation and exert low effort. Second, producers must be able to renege in the second period. The first assumption may be possible in this context through the application social sanctions, but seems unlikely due to the high degree of social power attributed to DCS secretaries and presidents. The second assumption seems even less likely given that the secretary is in charge of the DCS financial account. If anything, we expect that the secretary could hold up producers ex post and not vice versa.

Second, introduction of public incentives may increase transactions costs or introduce related frictions. If this increase spills over onto the allocation of total surplus, and is not confined to only the allocation of surplus from our experiment, then this cost may constrain the budget by more than the revenue brought in by the experiment itself. Secretaries that opted out of public payment suggested a version of this effect. Many said that publicly announcing the payment would lead to confusion, causing farmers to believe the were owed more than their share of the total village incentive. The increase in quantity poured among farmers in the public incentive villages lends some credibility to the theory that might have mistakenly thought they would receive payment as a per-liter bonus like other forms of cooperative payment; alternately, this might be the cooperatives' standard method of disbursing all surpluses. In the secretaries' view, the cost of resolving this confusion about payment owed would exceed the maximum possible incentive payment; with private payments they felt they could better control the message to mitigate misunderstandings.

Third, publicly announced incentives may affect the Pareto weights on more than just the surplus generated by the experiment. In our experimental context, DCS secretaries and presidents have complete formal control over the cooperative budget with little oversight. In focus groups, many producers reported receiving no information about cooperative finances. Moreover, although year-end bonuses were disbursed to coops by the KMF every year, the majority of producers reported never having received a bonus. These facts suggest that DCS management may de facto receive a substantial portion of the surplus generated by the coop. If public announcement of experiment incentives, paid into the cooperative budget, reveals other financial information, then it may trigger renegotiation to allocate more surplus from the overall budget to producers. Given the vast size of the cooperative annual budget relative to our one-off incentive payments, some secretaries may have felt that even a small chance of this occurring would not be worth the risk.

Although it is impossible to separately test these stories in our limited data, the implications for decentralized production are clear. In the context of Indian dairy cooperatives, local villagers clearly have enough local information and are capable of coordinating well enough to overcome production inefficiencies that cannot be addressed at higher levels. However, whether this coordination occurs or not depends on more than just the size of local incentives, but also on the way in which the incentives are administered. If introduced in the wrong way, group incentives may generate transactions costs or be sabotaged by elites in a way that undermines their intended effect.

6 Conclusion

In this study we find evidence that incentives can improve cleanliness in milk production in rural Karnataka, India. However, we also find that the incentive delivery mechanism matters. Poor governance may limit the passthrough of incentives to local producers, hindering their effectiveness. In the context of dairy cooperatives, we find that incentives are more effective when they are targeted and delivered privately to local elites rather than announced publicly within the cooperative. This result highlights an interesting tradeoff between efficiency and distribution. Production is most efficient when the incentive structure targets benefits to those already in power. This study also demonstrates a potential benefit of recent demonetizatoin efforts that have promoted electronic banking in rural areas, allowing payments to bypass elites entirely and be delivered directly to farmers.

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A Accordance with Pre-Analaysis Plan

A.1 Experimental Design

Three treatment arms collapsed to two; randomization remains in place.

A.2 Analysis of Outcomes

Unable to collect some outcomes due to funding constraints.

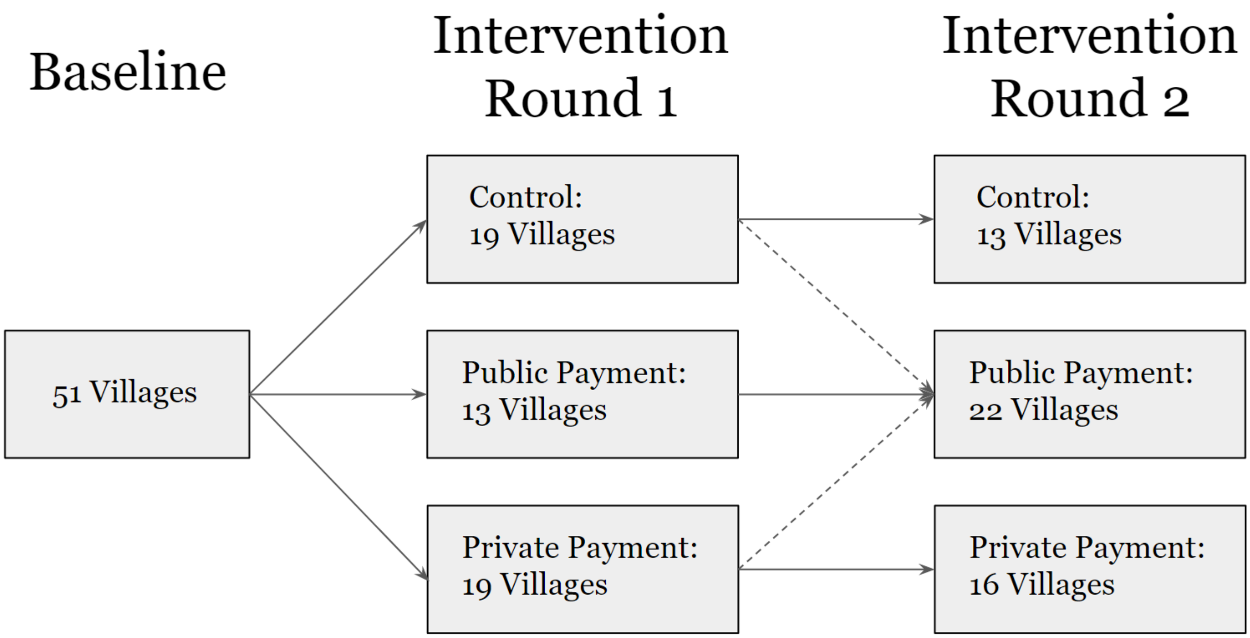

Figure 1: Randomization across two rounds of intervention.

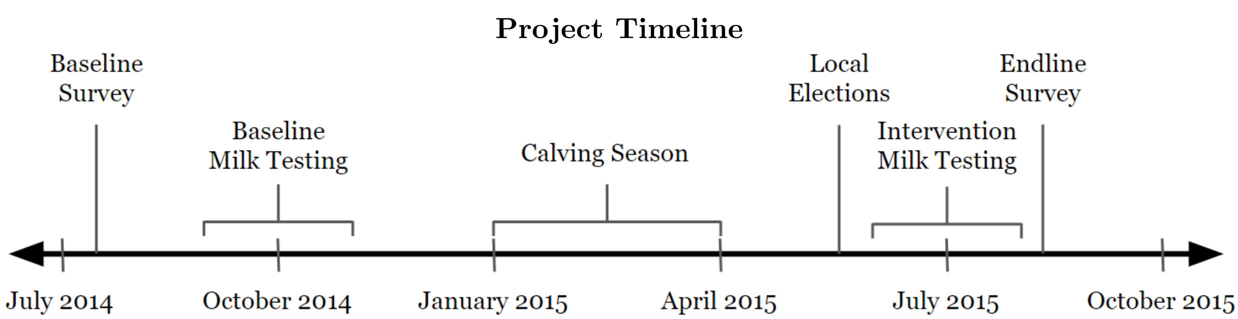

Figure 2: Timeline of project activities.

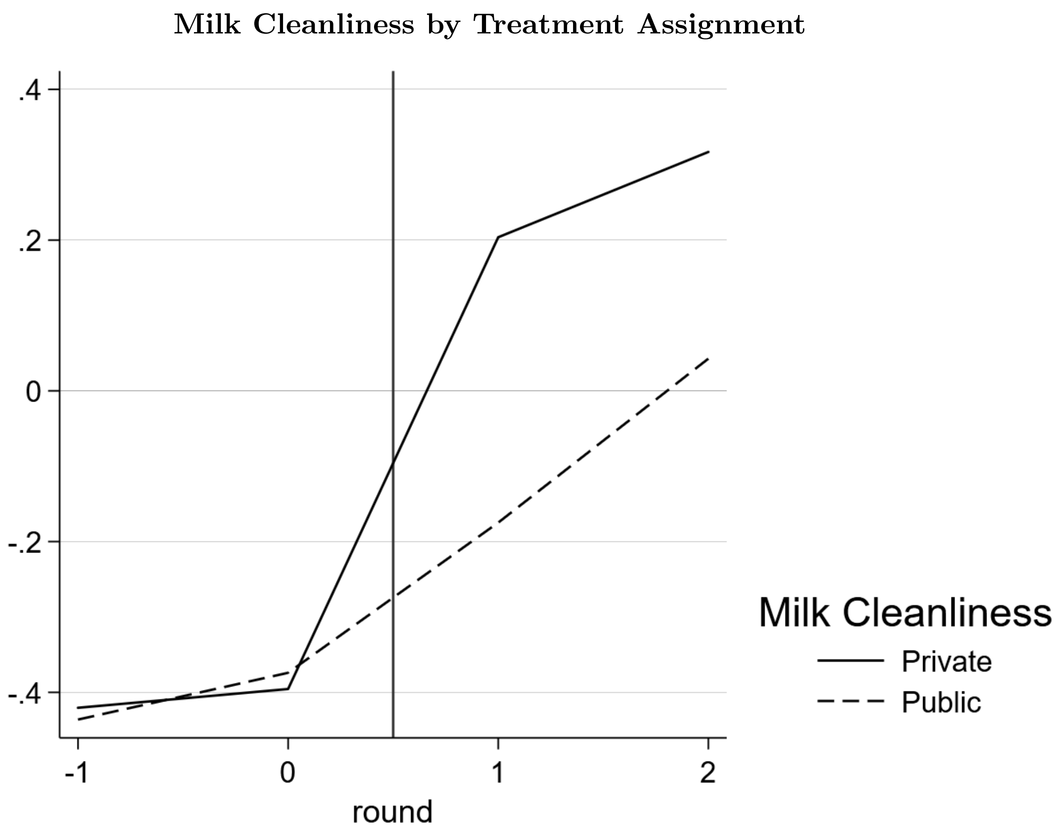

Figure 3: Event study of cleanliness by treatment assignment. Coefficients plotted relative to control mean.

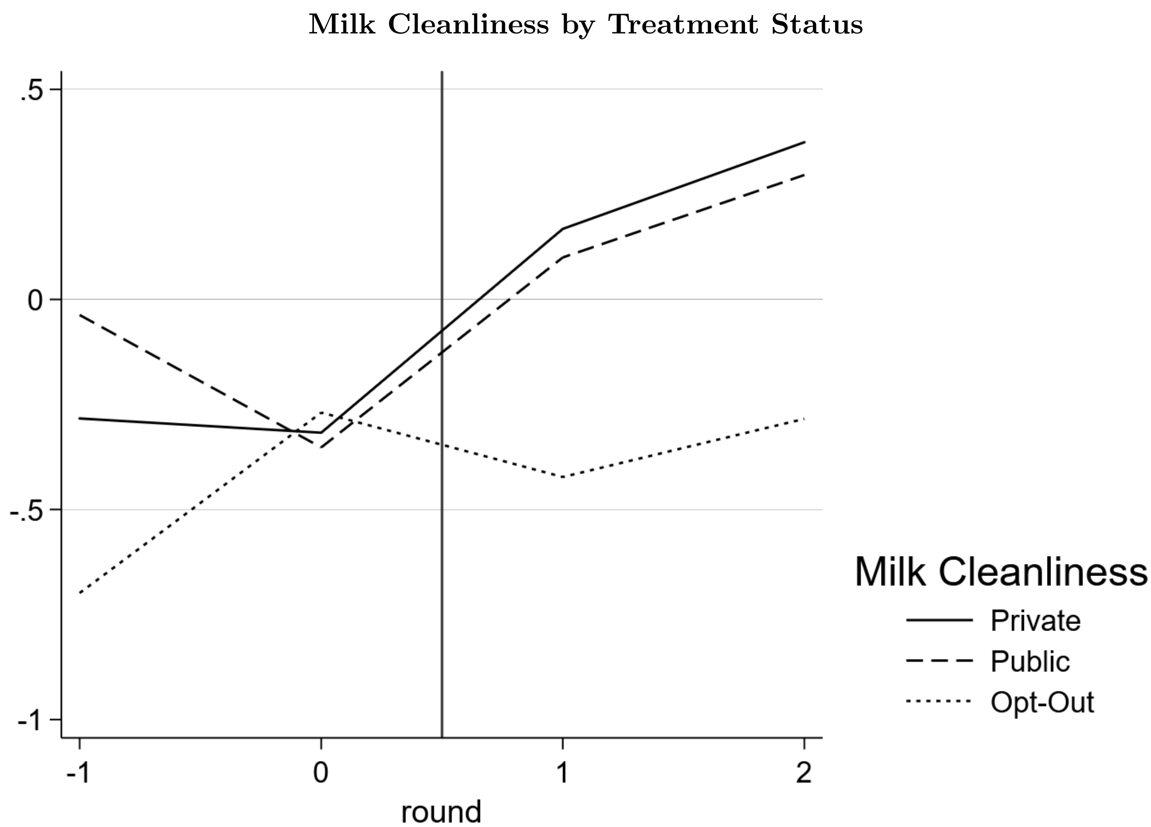

Figure 4: Event study of cleanliness by treatment status. Coefficients plotted relative to control mean.

DCS Governance Characteristics

	Producers	Secretary	Directors	President
Education	4.5	10.9	5.1	8.2
	(0.66)	(0.3)	(0.3)	(0.5)
Frac. SC/ST	0.29	0.23	0.31	0.08
	(0.02)	(0.06)	(0.02)	(0.04)
Land Owned	6.4	5.9	5.5	14.9
	(0.45)	(1.2)	(2.5)	(1.9)
Monthly Income	11765	14426	13163	19244
	(648)	(2282)	(862)	(2170)
Panchayat			0.06	0.21
			(0.01)	(0.06)
Observations	1106	53	436	75

Table 1: Characteristics of milk producers, DCS secretaries, DCS directors, and DCS presidents. Standard errors clustered by village in parentheses.

Perceptions of DCS Governance

Opinion of:	Producers	Secretary	Directors	President
Secy. Powerful	3.7		4.1	4.1
	(0.06)		(0.07)	(0.13)
Dirs. Powerful	3.1	3.4	3.4	3.4
	(0.04)	(0.11)	(0.05)	(0.07)
Secy. Good Manager	3.7		4.4	4.4
	(0.06)		(0.06)	(0.09)
Dirs. Good Manager	3.0	3.5	3.4	3.4
	(0.05)	(0.09)	(0.05)	(0.06)
Secy. Knowledgable	3.8			
about Dairy	(0.06)			
Dirs. Knowledgable	3.0			
about Dairy	(0.05)			
Observations	1106	53	436	75

Table 2: Beliefs about governor characteristics according milk producers, to DCS secretaries, DCS directors, and DCS presidents. Questions never asked about own self. Standard errors clustered by village in parentheses.

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	13894	11114	-2780 *
	(1218)	(800)	(1458)
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	15	36	

Table 3: Descriptive statistics for treated and control farmers. The third column reports differences between the two groups. Standard errors clustered by village in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Impact of Treatment Arms on Milk Cleanliness

	Simple Difference	Baseline Controls	Diff-in-Diff	2SLS
Private Incentive	0.256	0.363	0.641*	0.640*
	(0.291)	(0.252)	(0.355)	(0.345)
Public Incentive	-0.0356	0.0899	0.320	0.393
	(0.271)	(0.257)	(0.318)	(0.381)
Control Mean	0.0648	0.0648	0.0648	0.0648
R-Squared	0.0179	0.145	0.0822	0.0808
Observations	102	102	204	204

Table 4: Outcome is the first principal component score from a principal-components analysis of MBRT and SPC. Standard errors clustered by village in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Impact of Treatment Arms on Milk Production

	Cleanliness	SPC	MBRT	Quantity
Private Incentive	0.641*	-0.415	0.402	0.145
	(0.355)	(0.360)	(0.242)	(0.562)
Public Incentive	0.320	-0.275	0.163	1.076**
	(0.318)	(0.331)	(0.182)	(0.516)
Control Mean	0.0648	6.826	3.441	6.433
R-Squared	0.0822	0.0316	0.0659	0.0308
Observations	204	204	204	982

Table 5: Cleanliness is the first principal component score from a principal-components analysis of MBRT and SPC. Quantity is in liters per day for producers surveyed; total DCS quantity is unavailable. First three columns are difference-in-difference; fourth column controls for baseline average. Standard errors clustered by village in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Impact of Treatment Arms on Payment Received

	Payment Round 1	Payment Round 2	Opted Out Round 2
Private Incentive	121.1	98.32	0
	(106.9)	(82.73)	(0.00)
Public Incentive	-0.405	16.78	0.318***
	(85.41)	(81.12)	(0.102)
Avg. Pay	734.4	771.1	0
R-Squared	0.0485	0.0517	0.210
Observations	153	153	51

Table 6: The first two columns are diff-in-diff relative to the counterfactual payment the control arm would have received. The third column is a simple difference in Round 2 only. Standard errors clustered by village in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Impact of Treatment Arms on Knowledge and Beliefs

	Know about Payments	DCS Gave Information	Believe Secy. Clean	Believe Others Clean
Private Incentive	0.0104	-0.0527	-0.199**	0.220***
	(0.0112)	(0.0624)	(0.0971)	(0.0784)
Public Incentive	0.156***	-0.0572	-0.113	0.182**
	(0.0349)	(0.0657)	(0.0884)	(0.0748)
Baseline Control			X	X
Control Mean	0.00833	1.592	4.529	4.308
R-Squared	0.0761	0.00161	0.0177	0.0262
Observations	982	982	975	972

Table 7: First two columns report simple difference; second two columns control for baseline average. Standard errors clustered by village in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Impact of Treatment Arms on Knowledge and Beliefs

	Secretary Management	Received Bonus	Freq. Asked for Bonus	Asked Last Month
Private Incentive	0.0115	0.0972*	-0.0686	0.0236
	(0.158)	(0.0511)	(0.128)	(0.0433)
Public Incentive	0.0756	0.0976*	-0.203	0.0574
	(0.154)	(0.0502)	(0.134)	(0.0383)
Baseline Controls	X	X		
Control Mean	4.092	0.808	1.025	0.104
R-Squared	0.0400	0.0227	0.00486	0.00466
Observations	976	982	982	982

Table 8: First two columns control for baseline average; second two columns report simple difference. Standard errors clustered by village in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

DCS Characteristics by Study Participation

	Treated	Opted Out	Difference
Received Bonus	0.25	0.19	-0.06
	(0.07)	(0.06)	(0.04)
Frac. Directors Known	0.27	0.24	-0.03
	(0.03)	(0.0)	(0.0)
Directors Meetings	1.66	1.27	-0.39 ***
	(0.05)	(0.16)	(0.10)
Secy. Powerful	3.7	3.5	-0.20 **
(farmer opinion)	(0.09)	(0.22)	(0.10)
Secy. Powerful	4.1	3.9	-0.25 *
(directors' opinion)	(0.10)	(0.18)	(0.14)
Secy. Management	3.6	3.5	-0.1
(farmer opinion)	(0.13)	(0.11)	(0.11)
Secy. Management	4.3	4.4	0.04
(directors' opinion)	(0.09)	(0.13)	(0.10)
Dirs. Powerful	3.2	2.7	-0.42 ***
(farmer opinion)	(0.05)	(0.15)	(0.06)
Dirs. Powerful	3.4	3.3	-0.07
(directors' opinion)	(0.09)	(0.11)	(0.08)
Dirs. Management	3.1	2.7	-0.32 ***
(farmer opinion)	(0.07)	(0.15)	(0.07)
Dirs. Management	3.4	3.3	-0.07
(directors' opinion)	(0.08)	(0.11)	(0.07)
Num. Villages	15	7	

Table 9: Descriptive statistics at baseline for DCSs assigned to receive public payment in Round 2 by decision to continue participation. The third column reports differences between the two groups. Standard errors clustered by village in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$