Strengthening Producer Organizations to unleash productive potential of smallholder farmers in Uganda

-Work in progress, all results are preliminary and subject to change-

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1 Introduction

Producer organizations (PO) can improve market access, lower transaction costs and increase the profits of smallholder farmers by offsetting diseconomies of scale (Heyer et al., 1999; Hill et al., 2008; The World Bank, 2008). Yet, evidence for Sub-Saharan Africa shows that POs often fail to attain these objectives. Instead of acting collectively, PO members frequently opt for engaging in markets (and with local traders) individually (Fafchamps and Hill, 2005; Hill, 2010). Two types of organizational weaknesses are held responsible for this reluctance on the part of the PO members: Firstly, PO sales procedures commonly cause substantial delays in payments which adversely affects cash constrained farmers and, secondly, PO members lack access to reliable information about the final sales prices that PO leaders negotiate when selling the members' harvest[1].

In this paper we present the results from field experiments that, firstly, test rigorously the merit of these two explanations and, secondly, shed light on how improved PO performance affects agricultural production, labor utilization and other input use decisions by PO members. The latter, we suggest, sheds valuable empirical light on the rural growth potential that may be unleashed by repairing common inefficiencies in farmers' institutions.

More precisely we assess whether addressing the above mentioned weaknesses of POs effectively changes the allocation of labor and non-labor inputs. Given the effects on labor and non-labor inputs that we observe, we continue to investigate whether agricultural output was also affected. Besides analyses on the full sample, we look at effect heterogenities along the lines of trust in PO leaders, type of cash crop that is produced (coffee or maize) and the size of the farm. Unfortunately, the small number of female members in the producer organizations under study does not allow us to conduct statistically robust analysis along gender lines[2].

Our underlying hypothesis regarding the effect on labor allocation has been stated as early as in the seminal work on agricultural household models by Singh et al. (1986). Farm households that do not have access to markets produce only for own consumption, relying exclusively on own labor. Improving access to markets, in the present case through relaxation of institutional bottlenecks of the PO, might (1) mobilize underutilized household labor, (2) increase the demand for hired labor and (3) increase the demand for other inputs. Increases in (2) and (3) essentially offer potential for multiplier effects on rural markets.

We conduct a cluster-randomized controlled trial to test this hypothesis in the context of producer organizations in Uganda. We randomly allocated three different alterations all designed to render participation in the joint sales activities more interesting. In a set of 167 POs we assess how these changes affect the labor and non-labor input decisions of the PO members.

The first alteration tackles the problem of delayed payments concur-

[1]The lack of transparency in the sales procedures can be further complicated by a lack of trust in the PO leaders.

[2]Dolan (2004) reports results from a study in Uganda emphasizing financial constraints as a main barrier to usage of non-labor inputs and hired labor for female farmers.

rent to bulking activities. It effectively aims at reducing the period of time that PO members have to wait before receiving the revenue from their sales through the PO. By reducing the postponement in payment, inefficiencies for cash constrained farmers are reduced. This could affect the demand for hired labor as it increases the capacity to pay laborers immediately after harvesting. Adams (1991) stresses the importance of smallholder agriculture for the demand of agricultural wage labor for the case of Zimbabwe. In many rural African labor markets daily payment or payment on piece/task basis is prevalent for rural laborers. Sender (2005) studies the labor market in Mozambique and reports that 70 percent of agricultural wage laborers receive either daily pay or are paid by the piece delivered or task accomplished. Under such conditions smallholder farmers with cash constraints are unattractive employers. Naturally, laborers prefer to offer their labor to smallholder farmers who do not engage in bulking and who can pay remuneration without postponement. Simultaneously from the point of view of the demand side, active PO members might refrain from hiring additional labor due to their inherent incapacity to pay wages promptly after harvesting.

The second intervention tackles the problem of opaque sales procedures. Ordinary PO members are not present at the time of the final sale of their bulked produce. Hence, their engagement in joint marketing requires trust in the PO in general and in its leaders more specifically. If trust is low the inclination to participate is low, too. By providing information on the final sale this intervention tries to offset the lack of trust. In a first step, a voucher system is put in place to document the quantity a farmer delivers to the PO. To reinforce voucher distribution, financial incentives are given to those who issue the vouchers. In a second step, external consultants are present when the PO leaders make the final sale. These consultants then inform the smallholder farmers via sms text messages about the exact details of the sale.

A key underlying assumption in our analysis is the following: The implementation of the interventions started during the planting season. Therefore, we hypothesize that farmers' aspirations change way ahead of the moment when the sales actually take place. Changed aspirations about the sales procedures might lead to changes in the farm managers' labor and non-labor input decision. This relies on the further assumption that participation in the bulking activities is perceived as a means to increase bargaining power and consequently sales prices.

With varying significance, we observe that farmers in our sample adapt their household labor allocation and their demand for hired labor in the advent of the interventions under study. This suggests that mere anticipation of changes that promise better market participation through the PO suffices to induce these effects. Furthermore, the distinct treatment arms prove to have differential effects on the type of labor that is being mobilized. Information on sales procedures rather increases the amount of household labor, while relieving cash constraints at harvest increases the labor that is hired for crop production. The latter is the case first and foremost if the farmer has trust in his PO leaders.

We equally provide evidence that non-labor inputs are positively affected. It shows that the CoD treatment increases the use of inorganic fertilizer while IoS affects the use of pesticides and fungicides as well as the use of traditional and hybrid seeds. Giving consideration to the generally low adoption rates of these technologies among smallholder farmers, our work provides evidence on the role producer organizations can play for the propagation of innovative farming practices.

2 Context and the Interventions

2.1 Labor Market Context

Uganda's population has increased from around 9 million in 1969 to an estimated 34 million today[3]. Close to 90 percent of the population reside in rural areas. This extent of population growth poses a problem for food security and employment. According to official estimations 66 percent of the labor force make a living in agriculture (Uganda Bureau of Statistics 2010)[4]. In rural areas the share of people aged 15-60 who work in agriculture is above 70 percent. Uganda's agriculture predominantly features smallholder farming whereby poverty levels amongst the farm managers' households are high. Monthly wages in agriculture amounted to only 120,000 UGX in 2008 (=70

[3]See `http://countrystat.org/home.aspx?c=UGA&tr=12`.
[4]See `http://www.ubos.org`.

USD[5]At 2008 exchange rates.(Uganda Bureau of Statistics and Development").

In order to offset unfavorable market structures smallholder farmers seek refuge in the formation of POs to improve their market situation. Heyer et al. (1999) point out that groups perform efficiency functions. Farmers engage in collective action as the group activity promises to improve their situation vis-a-vis adverse market conditions, e.g. high transaction costs and information asymmetries. High direct costs for transportation offer one example for inefficiencies that smallholder farmers face. Due to high costs individual farmers sell unprocessed Robusta berries (local name: kiboko) to itinerant traders at the farm-gate. While sales prices increase after milling the berries only few farmers can afford to travel to markets themselves where they mill the coffee before selling. Hill and Vigneri (2011) report that among a sample of 300 Ugandan coffee farmers only 3 percent of all observed transactions were for milled coffee.

Through collective action POs can reduce the cost for transportation or provide milling services.

2.2 Producer Organization Context

Develtere et al. (2008) explore the history of producer organizations in Uganda that started as early as 1913. Later, after independence in 1962 the Ugandan government heavily regulated the market for agricultural outputs and organized marketing boards. Such boards set constant buying prices independently of the time of the season, liberating the farmer of the question when to sell (Ponte, 2002; Shepherd, 2011). The question to whom to sell was equally redundant at that time, since producer organizations were the only marketing option for smallholder farmers (Hill et al., 2008; Ponte, 2002).

With liberalization starting in the 1990s the POs saw change internally as well as in their environment. Under the structural adjustment programs liberalization in the coffee market was promoted and with the introduction of competitive market structures POs nearly disappeared (Ponte, 2002). Develtere et al. (2008, p. 156) document that many POs collapsed due

[5])

to "...massive corruption, mismanagement, theft, failure to hold elections, failure to surrender members' deposits, failure to hold elections on time, favoritism and dismissal of staff, refusal of officials to vacate office after being duly voted out..."[6].

Against this historical background, the role of trust in producer organization gains in importance. Current research on POs in Uganda reveals that the efforts that PO leaders exert in managing the organization's collective activities such as bulking, remain below levels of effort that can be attained if leaders are being monitored (Grossman and Hanlon, 2013). This result emphasizes the importance of measures that aim at reducing the possibility for elite capture.

2.3 Interventions

This study looks at two interventions designed to tackle barriers that keep PO members from participating in the bulk sales: partial payment at the harvest delivery and increase in transparency of the sales process. We are not aware of previous evidence in the literature documenting the effectiveness of these measures on labor allocation.

Cash on Delivery

This intervention provides randomly selected POs with the financial means to pay members a share of their revenue right when the farmer delivers the harvest to the PO. The intervention started by distributing vouchers to PO members. Holding such a voucher qualified the farmer to receive 30 percent of the total sales price right at delivery. The remaining 70 percent of the sales price were to be paid out after the PO made the final sale.

Information on Sales

POs randomly allocated to receive this intervention set up a voucher system that documents the quantities a farmer delivered to the PO. At delivery the farmer and the PO representative would fill out a slip stating the quantity that the farmer delivered. The PO representatives were given

[6]Kwapong and Korugyendo (2010) further explore the factors underlying the survival of some and the failure of other POs in Uganda.

financial incentives to distribute these vouchers. All PO members were informed about this documentation system and about the incentive system for the representatives.

In addition to installing and reinforcing the voucher system, an external consultant was hired that provided the ordinary members with information on the final sale. The ordinary members elected two of them who would receive a text message from the consultant right after the final sale. The text message stated the final quantity and the price per kilogram. The two elected members received training on how to interpret and explain the text message. The regular members received training on how to interpret the information from that text message.

Both interventions

In POs selected for both interventions the members received training on using the cash-on-delivery vouchers, on how to fill out the delivery slips and on interpreting the information-on-sales text messages.

3 Experimental Design and Data Collection

For the experiment we applied a cluster randomized trial where the unit of randomization is the PO and the unit of analysis is the individual PO member. From our sample of 167 POs 42 were selected to receive the cash-on-delivery intervention only, 42 were selected to receive the information-on-sales intervention only, 40 POs were allocated to receive both the cash-on-delivery and the information-on-sales interventions, and 43 were allocated to the control group.

The organizational structure of these member driven organizations is such that on the district level POs form depot committees (DC). The DCs process and sell the crop[7]. Affiliation with a DC could potentially lead to imbalances in observable and unobservable or correlate with the effects of

[7]The next highest level above the DC is the service organization. Each DC in our sample is member in one of the following five service organizations: the National Union of Coffee Agribusinesses and Farm Enterprises (NUCAFE), NKG Coffee Alliance Trust, Volunteer Effort for Development Concerns (VEDCO), the Uganda Co-operative Alliance (UCA) and Kulika.

the intervention. To avoid such imbalances we stratified our sample of POs by DC. Stratification on the basis of variables that are likely to influence main outcomes also increases the power of the statistical analyzes (Bruhn and McKenzie, 2009). We stratified the POs into ten strata according to DC affiliation. The number of POs per DC/strata varies substantially with the smallest DC contributing seven and the largest one 30 POs. Furthermore, the smallest DC is exceptional as it does not have a PO allocated to receive the cash-on-delivery treatment only.

We conducted a baseline and an endline household survey where we interviewed at least two members of each PO, amounting to around 80 respondents per treatment arm. The interventions were implemented between November 2010 and September 2011. The endline data was collected in October 2011. For the analysis, we dropped three households from this sample because they had extreme values on a few characteristics[8].

3.1 Baseline Information by Treatment Status

Table A1 presents summary statistics at baseline for the sample by treatment status. The last columns list the p-value from t-tests to indicate whether any observed difference in means between the treatment group and the control group is statistically significant. Despite the randomization into treatment we find a handful of variables that are significantly different at the ten per cent level. At baseline, the PO members in our sample have around 7 household members, the head is on average roughly 50 years old, the spouse is about ten years younger, and in only 11 to 15 percent of the cases, depending on the group, is the head of the household a woman. The average schooling of the household head lies between six and seven years and is lowest in the control group.

The only variable that we find to be significantly different at the five percent level is the number of parcels, with 2.5 parcels in the control group and 2.1 in the group that received both interventions. Roughly 50 percent of the treatment sample plants coffee, whereby only 43 percent of the control group do so. Yet, this difference is not significant. Around 34 to 39 percent

[8]One of the dropped households has 25,000 coffee trees as compared to the average in the sample of 660. The other two were remotely distant from the next producer market with over 45km as compared to the average distance of five km.

of the farmers have more than 500 coffee plants. The share of members living more than five km away from the next agricultural market is around 30 per cent in the CoD group and in the group that received both interventions, but only for the former is the difference with the control group statistically significant at the 10 percent level. In the control group we find that 45 percent of the respondents live less than five km from the next market.

Availability of electricity in the household is low at 6 to 11 percent, compared to ownership of a mobile telephone which is at roughly 60 per cent. Ownership of savings accounts is at around 23 to 36 percent and about 33 to 41 per cent of the respondents hold informal savings. Trust is relatively high given that 75 percent trust their neighbor and over 50 per cent trust people in general. We create a proxy variable to measure trust in PO leaders which reveal quite high confidence in the leaders.

4 Results

4.1 Mean Comparison of Outcome Variables by Treatment Status

We present in table 2 summary statistics of the four response variables that measure labor input: household member labor days in farming activities, household member labor days in crop production, hired labor days used on farm, and hired labor days for crop production[9].

At baseline all outcomes are balanced, i.e. we do not find any significant difference neither in the first nor the second season between the means of the control group and the groups that received an intervention. For the first season, we find some evidence for impact as differences at the 10 per cent level exist at endline between the mean of the control group and that of the group that received information on sales. This is the case for household member labor days in farming activities, and for household member labor days in crop production. For the second season, we find evidence of a significant difference at the endline for the variable household member labor days in crop production for the group that received both interventions.

[9]In the Appendix, Table A2 presents the same results for the first agricultural season, and table A3 those for the second season. In addition the tables in the Appendix provide results from t-tests at baseline and at the endline and present the p-values.

4.2 Estimation Strategy

Given that a few significant differences exist between the treated and the control group at baseline we complement the simple means comparison by OLS estimations. This allows controlling for the unbalanced items. We evaluate the impact of being a member in a PO that received one of the three interventions on our outcome variables: household member labor days in farming activities, household member labor days in crop production, hired labor days used on farm, and hired labor days for crop production, by estimating the following equation for member i:

$$Y_i = \alpha + \beta CoD_i + \gamma IoS_i + \delta Both_i + \eta X_i + \sum_{s=1}^{10} \lambda_s DC_{i,s} + \epsilon_i \quad (1)$$

where CoD_i is an indicator variable taking on value one if member i is from a PO that was assigned to distribute vouchers for cash-on-delivery, IoS_i is an indicator variable taking on value one if member i is from a PO where information-on-sales were distributed, and $Both_i$ is an indicator variable that denotes affiliation of member i to a PO that received both interventions at the same time. With coefficient β we then estimate the Intention to treat (ITT) effect of the Cash on Delivery treatment, with γ the ITT effect of the information on sales treatment and with δ the ITT effect of implementing both interventions at the same time.

The vector X_i contains the control variables household size, years of schooling of the household head, age of the household head, a dummy for female household heads, a dummy for land size is larger than seven acres, the number of parcels, a dummy for farmer plants coffee, a dummy for number of coffee trees is above 500, a dummy for savings at a bank, a dummy for household has a mobile phone, a dummy for household has electricity, a dummy for distance to next producer market is larger than 5 km and a dummy capturing whether the neighbors can be trusted. All control variables where measured at baseline. The matrix X_i is only included when specifically mentioned. The DC indicators on which we stratified the random allocation of the treatments enter our estimation via $DC_{i,s}$, where s is the DC.

We restrict our estimations to the ITT effect which gives us the average impact of offering the intervention on the PO level, regardless of whether the individual ultimately participates in joint marketing or not. The ITT effect is the relevant effect here since it is offering the treatment that affects the labor input decisions which are taken way before the final decision to join in the bulking of the PO. In other words, the sequencing is such that the labor decisions are affected by offering the intervention even if the member does not take them up in the end.

In addition, we estimate regressions where we control also for the initial value of the outcome variable. As proposed by (McKenzie, 2012) this specification increases statistical power when auto-correlation of the outcome variable is low:

$$Y_{i1} = \alpha + \beta CoD_i + \gamma IoS_i + \delta Both_i + \chi Y_{i0} + \sum_{s=1}^{10} \lambda_s DC_{i,s} + \epsilon_i \qquad (2)$$

where Y_{it} captures the outcome at $t = 0, 1$, i.e. baseline or endline. The period of observation covers two agricultural seasons. We are explicitly interested in the distinct effects of the interventions on labor input for these two seasons. Therefore, we estimate all regressions separately for the first season - where labor decisions are taken without any experience about how the intervention changes access to markets - and the second season where members have some experience with the intervention.

In all regressions we correct the standard errors through clustering on the PO level. This takes into account that the regressors of interest, i.e. the treatment indicators, do not vary on the individual but only on the PO level. We cannot rule out that intraclass correlation is potentially high within the PO and when using robust standard errors its presence might lead us to overestimate the precision of our coefficients (Angrist and Pischke, 2008).

4.3 Average Impacts on Household Labor Inputs

Table 3 presents first season coefficient estimates of Equation 1 in Column (1) and of Equation 2 in Column (2). Columns (3) and (4) present estimates for the second agricultural season. In Panel A, the outcome variable is the number of household member labor days in farming activities. In the first season, the control group reports having spent around 195 days in farming (see Table 2). Here, only assignment to IoS shows a significant effect, suggesting that the members of IoS POs increased the number of labor days their household spends farming by 65.1 days. Not only is this statistically significant (at the 10 percent level), also from an economic point of view the effect size plays a significant role as on average every household member spends over one week more (65.1/7 = 9.3) in farming activities. When controlling for individual characteristics the effect remains significant at the 10 per cent level. In the second season the estimated impacts of IoS are slightly lower in size, ranging from 55.6 to 59.9, and are no longer significant. The reduction in significance and effect size could be explained by an unsatisfied experience made with the intervention during the first season. But this cannot be tested here. CoD has low and insignificant effects ranging from 10.3 to 18.9 days in the first season. In the second season these effects are much larger but remain insignificant.

Panel B of Table 3 then looks at household labor days spent in crop production. Columns 1 and 2 show again that it is only assignment to the IoS group that has significant and sizable effects on the days the household spends on this activity in the first season. Again, these effects do not carry over to the second agricultural season. For household member days in crop production (Panel C) we observe large and significant effects from assignment to the third treatment arm *both*, i.e. the combination of both interventions. In this treatment group members increase input to this activity by at least 55.5 days. These effects are significant at the 5 per cent level when estimated with individual level covariates and when we control for covariates including the initial value of the dependent variable. One could argue that the high increase in effect size from the first to the second season comes as a result from positive experience made during the first season. As for the earlier case, this cannot be tested here.

4.4 Average Impacts on Hired Labor Inputs

A key question is not only whether households adapt their own labor input in anticipation of better market access but whether their demand for external labor is also affected. Panel C of Table 3 examines the impact on labor hired for farming activities. We see that there is no significant impact. In part, this might be explained by the fact that all four groups increase hired labor substantially from baseline to endline. The mean for the control group is at around 50 days in both seasons which is about 20 days higher than at baseline (see Table 2). For the other groups we even observe increases by 30 days. Given that we observe already significant increases in household labor for this activity, at least for the IoS group, one could also assume that this crowds out the need for hired labor. In Panel D of Table 3 we see that all estimates for the impact on hired labor for crop production are insignificant. Here the mean of the control group in the first season is 42 days and 58 days in the second season (Table 2).

4.5 Average Impacts on Non-Labor Inputs

In Uganda traditional agricultural practices and local seed varieties (open pollinated varieties) widely remain the dominant choice of technology of smallholder farmers. Adoption rates of modern hybrid seeds are low in Uganda (Doss et al., 2003) and so is the use of potentially productivity-increasing but costly inorganic fertilizers (Byerlee and Eicher, 1997). Benson et al. (2012) report that less than 5 percent of Uganda's farmers regularly use inorganic fertilizers. They list several reasons for low usage rates: Financial Constraints, inappropriateness of fertilizers for the farmers' soils, lack of awareness about productivity enhancing features, expanding farming area rather than increasing yields per hectare, etc. In the presence of financial constraints, farm managers facing a market situation characterized by low bargaining power might lack incentives to make investments in costly inputs. In the following, we test whether creating a situation where improvements in market access are anticipated is sufficient to encourage farmers to invest more in productivity enhancing technologies.

As for labor inputs, the impact on non-labor inputs are affected positively and differentially so across treatment arms. Panel A of Table 5 presents the impact estimates on non-labor input factors. It shows that the impact on the use of organic manure fertilizer is close to zero for all treatment arms. For inorganic fertilizer on the other hand we obtain significant estimates in the range of a 10 percent increase among the CoD farmers. The estimates on use of pesticides and fungicides are of similar size and significance for the IoS and the Both treatment arms. The use of traditional seeds also increased by 10 percent in the IoS sample but the coefficients are only significant at the 10 percent level. The use of hybrid seeds was affected significantly only among IoS farmers. These results indicates that monies are available to the farm household that can be accessed and put to production use.

4.6 Heterogeneity of Impacts

4.6.1 Impact on Labor Inputs in Sample of Farmers that trust their PO Leaders

Naturally, trust plays an important role in the member's decision to adjust labor input as a result of the anticipated changes from the interventions. This is especially relevant, in the first season there is no experience on how these changes will be implemented. We therefore examine whether the effects of assignment to treatment differ for those members that have trust in their PO leaders at baseline. A question asking directly whether the respondent trusts the PO leaders risks to suffer from courtesy bias, i.e. the respondent gives socially desirable answers. Instead, we elicit the respondent's opinion of the PO leaders indirectly. At baseline, the respondents rated the effort that, in their opinion, the PO leaders would exert for a communal project [10]. This rating reflects to what degree the respondent esteems the PO leaders. We assume that this correlates with the level of trust in the PO leaders. We normalized this rating by the effort that the respondent expects from ordinary members and created a dummy variable taking on value one whenever the respondent thinks the PO leaders would put more effort into the project than the ordinary members. The following regressions include only the sub-sample of PO members who trust their PO leaders according to this variable.

Table 6 presents the effect on household member days in farming activities in Panel A. As compared to the full sample the subgroup analysis shows similar results for the first season: Only assignment to IoS causes significant increases in the number of days the households spends on farming. The size of the effects is at around 77.9 days which is roughly 15 days higher than for the full sample. Interestingly, for the 'trustful' sample we also detect significant effects of similar size for IoS assignment in the second season.

[10] At endline we ask directly whether the PO leaders can be trusted despite the risk of courtesy bias.

For the members assigned to receive both interventions simultaneously we obtain a negative but insignificant effect in the first season. CoD members only increase their household farming days insignificantly in the first season and the second season.

In Panel B of Table 6 we find a similar pattern for crop production. The impact of IoS assignment that we observe to be significant in the full sample is stronger here in terms of magnitude and significance and it also remains significant in the second season. While the IoS treatment is propelled by trust in the PO leaders, assignment to CoD and *Both* does not lead to heterogeneous effects. On the contrary, assignment to *Both* has significant effects on the full sample but not on the sub-sample. The estimates of the impact on hiring labor for farming are all larger in absolute size. Striking is the large negative impact that assignment to *Both* causes in the first season. It is counter intuitive that the implementation of both interventions leads members to reduce their demand for hired labor, and even more so for those who have trust in their leaders. The significance of the effect is not robust across seasons or specifications, yet the sign remains negative.

The regression results presented in Panel D of Table 6 capture the ITT effect on labor hired for crop production for the sub-sample of members that trust their PO leaders. We see that in the first season assignment to CoD increases hired labor input by 24.0 days and more, depending on the specification. These effects are substantially higher as compared to the estimates on the entire sample and they are also significant at the 10 per cent level. This indicates that the outlook to be eligible to receive cash on delivery has had stronger effects on those members that state at baseline to trust their PO leaders. For the second season, we document even higher effects of 25.0 days but these impact estimates are insignificant.

4.6.2 Impact on Non -Labor Inputs in Sample of Farmers that trust their PO Leaders

As presented in Panel B of Table 5, the effects on non-labor inputs among the trustful sample vary slightly from the effects in the full sample (Panel A). For the CoD treatment we obtain significant impact estimates for organic and inorganic fertilizer products. Especially robust across specifications is the 10 percent increase in farmers who use inorganic fertilizer. The impact on manure fertilizer, is significant only if we include for individual level controls.

In the IoS and Both treatment arms usage of pesticides and fungicides increases significantly. The coefficients are slightly larger than in the full sample ranging from 11 to 14 percent.

4.7 Average Impacts on Agricultural Output

4.8 Average Impacts on Trust in Leaders

The previous section provided evidence that heterogeneity in the impact exist according to whether the member has trust in PO leaders or not. It is trust at baseline that creates this heterogeneity. Since we measure trust at endline as well, we can test whether any of the interventions affected it.

Table A4 documents the descriptive results for three different trust measures observed at endline. The respondents rated whether most people can be trusted, whether in their absence neighbors could be trusted to look after their house and whether PO leaders could be trusted to make decisions that are good for the PO members. All three original variables are scaled from 1 (strongly disagree) to 7 (strongly agree). Based on this we created indicator variables taking on value one whenever an answer greater than 4 (neither agree nor disagree) was given. We present the mean level of trust for the respective group in the middle panel and p-values from t-tests comparing the means for the different treatment groups to the control group in the last panel. None of the trust indicators shows a significant difference. This indicates that trust as we measure it was not affected by any of the interventions.

Trust in PO leaders is very high at over 80 percent in all groups. While this variable was proxied at baseline (see Section 3.1), at endline we asked directly whether the PO leaders can be trusted. This could in part explain the higher average outcome at endline as compared to the baseline (compare Table A1). If one assumes that trust is constant over time, the difference between the proxy measure at baseline and the direct measure at endline could be interpreted as a naive estimate of the courtesy bias, i.e. the interview situation causes the respondent to rate the trust in PO leaders higher than she would rate it under other circumstances.

We assess how the interventions affected trust equally by estimating equation 1 with and without individual level covariates. The question on trust in PO leaders differs between baseline and endline so that we are not able to control for initial values of the dependent variable as it is done in Equation 2. The coefficient estimates are presented in Table 8. We find no evidence for a positive effect on trust from any of the interventions. Our impact estimates are fairly small in size, amounting at most to 3 per cent, and are all statistically insignificant. This confirms the descriptive picture that trust in the PO leaders, as we measure it, was not affected by the intervention.

5 Discussion

In the econometric analysis we found positive effects on labor and non-labor inputs for the Information-on-Sales (IoS) intervention. The effects were significant only for family labor, use of pesticides and purchase of seeds, whereas no statistically significant effects were found for hired labor. Regarding the second intervention, Cash-on-Delivery (CoD) the point estimates were mostly positive, but hardly ever statistically significant, either because the true effects are very small or because effects are highly heterogeneous (which increased noise in our regression equations). Certainly the statistical power of our analysis does not permit us to claim that the effects of IoS are larger than those for CoD, yet overall it seems that there are some indications that IoS had been the more successful intervention.

This would be consistent with a scenario where asymmetric information about prices and transactions is indeed a main concern. Small-holder farmers anticipate that they may be cheated when delivering their produce at the depot in that they will be told a price lower than the realized one. In addition, small-holder farmers also anticipate that traders (who often collude to develop local monopsonies) pay less than market prices. Farmers thus anticipate that they will attain only below market prices (and in addition may face asymmetric information about the extent and distribution of these price gaps). Therefore, their expected value from agricultural production of coffee/maize decreases.

The IoS-intervention, which provides truthful information on sales prices as well as amounts, promises to reduce this asymmetric information. The promise of transparency may increase prices even if farmers do not deliver their (entire) produce to the depot, because traders will also anticipate that their information advantage will decrease or vanish, such that they have to increase prices. In addition, the benefits of collusion among traders to form local monopsonies also decreases with the decrease in the traders' information advantage. In other words, with better flow of information, farmers might learn which traders regularly cheat more than others, and may shun cheating traders, opening up competition among traders.

In addition to the reduction in asymmetric information, farmers may also increase their labor inputs as they anticipate (or hope) that the interventions will lead to a better general functioning of the PO, which could also lead to higher prices if the PO can sell a larger aggregate output.

Given the positive effects of IoS on labor and non-labor inputs, one may wonder why there are no effects on hired labor. One reason may still be cash constraints in that hired labor has to be paid well in advance of the harvest and sales. In addition, the final sales price of coffee/maize is unknown in advance when hired labor has to be paid. To some extent, one could also expect effects for hired labor to be smaller than for family labor since the overall higher labor demand in a village because of the IoS intervention will decrease surplus supply (i.e. supply of labor days that are not invested on the own farm) and push up prices for higher labor.

In our setting, we are unable to analyze such effects since we also observed a general increase in hired labor during the study period: Even in the control villages, the amount of hired labor increased from 34 days at baseline to 52 days at endline. At least in part these increases in the entire study sample can be explained by hikes in the prices for coffee. World market prices increased dramatically during the study period (see Figure 1). It is not unlikely that these sparks were passed through to local markets, sparking the farmers' interest in increased crop production and therefore also the demand for (hired) labor. Given the large overall increase of hired labor, the potential for IoS to increase hired labor even further may be limited: First, there is less labor supply available and second we would expect decreasing productivity for further increases in hired labor (coordination and monitoring costs, etc). Given this special situation, it may be that our finding of no significant effects on hired labor is only driven by the large general increase of hired labor that occurred during the study period.

We thus restrict our main conclusion that better price transparency for rural producer organizations can indeed help to increase agricultural production, labor and non-labor inputs in rural Africa, whereas the case is less clear for interventions intended to reduce liquidity constraints.

As we observe the changes in labor inputs are not constant across seasons. Several explanations can be brought forward to explain why we observe significant effects only for the first season. First, this can be due to farmers' dissatisfaction with the implementation of the interventions. If, for example, the change in transparency is not as substantial as the farmer anticipated a fallback to the initial status of input allocation and sales channels might be the consequence. Alternatively, if the expected increase in prices obtained through the bulking remains behind the expectations farmers might equally return to standard practice. Table A11 shows the impact of the different interventions on the price per kilo obtained at the last three or largest three sales in the preceding 12 months. We find weakly significant positive effects only for the IoS group in the second largest/ second to last sale. This indicates that the treatment may not significantly affected the price in the largest sale that the farmers has made.

6 Conclusion

Our results provide further evidence for the relevance of collective action for rural development. We have shown that interventions aiming at improved market access through producer organizations in rural Africa can substantially affect the labor input decisions of the PO members. The results support the assumption that lack of market access creates disincentives for labor input in smallholder agriculture, which potentially hampers development. We observe that tackling the imperfections that block market access can unleash labor resources. Sadoulet et al. (1998) describe three different labor regimes in agricultural economies: wage laborers, farmers relying solely on household labor and farmers that hire in additional labor. Our results suggest that the interventions we study can make a difference for two of the three regimes.

Providing the farmers with information about the outcomes of the PO sale proved effective in unleashing untapped household labor. We document that these effects are stronger when the level of trust in PO leaders is higher. This demonstrates the importance of strong institutional features. We observe further evidence for effect heterogeneity as the effects wear off in the full sample after the first season. The impact persists over time only in the sub-sample of members who trust their PO leaders.

Our analysis regarding the impact on non-labor inputs reveals significant impacts among the different treatment groups. The CoD treatment affects significantly the usage of inorganic fertilizer. The IoS treatment increased significantly the use of pesticide and fungicide use as well as the demand for seeds.

Ultimately, these results are good news for producer organizations and for the role they can play. The challenge ahead is the implementation of changes in their structure similar or identical to those assessed in this paper. These changes need not only be communicated through a person that has the farmers' trust but also is there the need to deliver.

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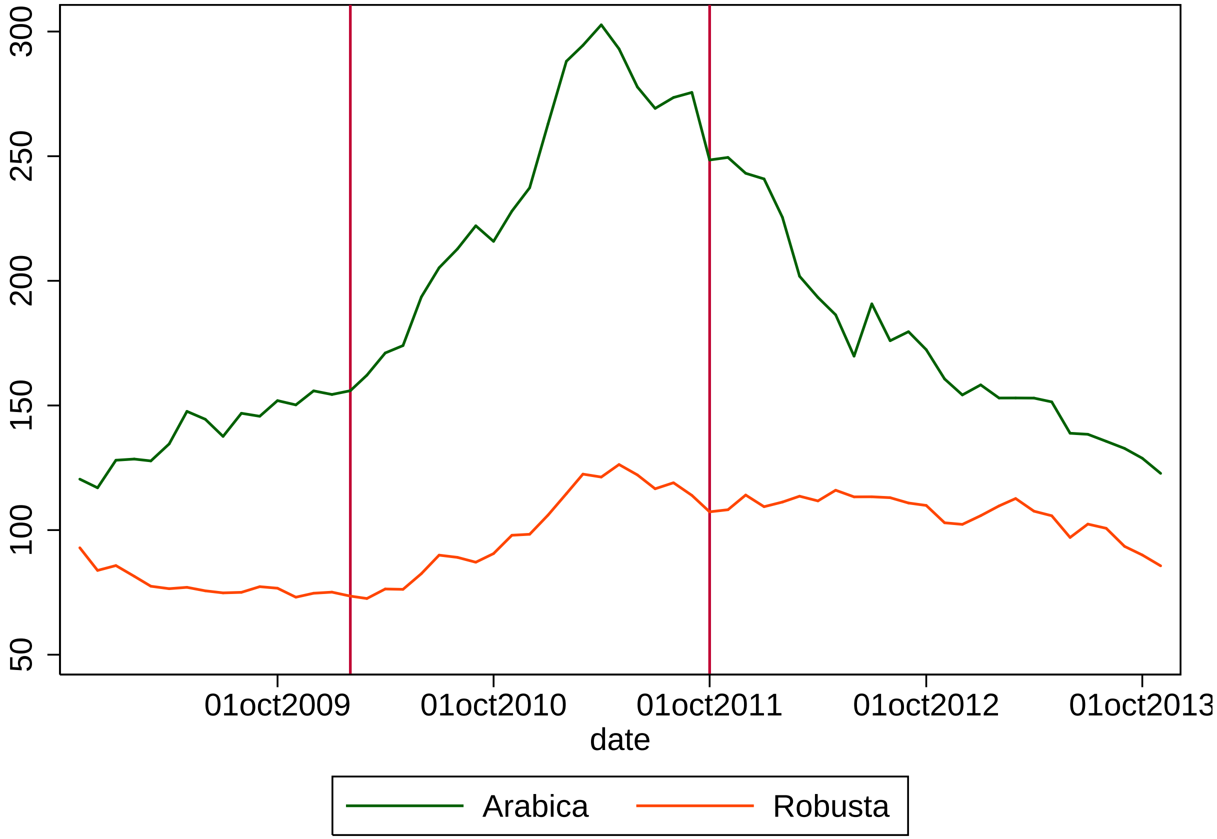

Figure 1: New York cash price for Arabica and Robusta coffee, US cents per Pound (Source: `http://www.indexmundi.com`). Vertical lines indicate time of survey.

Table 1: Summary Statistics of Household Characteristics at Baseline

	Mean of Variable			
Variable Names	Cash-on-Delivery	Information-on-Sales	Both Interventions	Control
Household size	7.1	7.2	7.1	7.3
Age head	49.6	49.0	53.3	48.8
Age spouse	39.9	39.5	42.4	38.1
Gender head	.15	.10	.13	.12
Yrs schooling head	6.6	7.2	6.3	6.1
Yrs schooling spouse	4.7	5.7	5.4	4.8
Distance to market>5km	.33	.48	.32	.45
Savings account bank	.16	.26	.10	.19
Savings account MFI	.23	.35	.31	.29
Informal saving	.41	.31	.25	.33
Owns mobile phone	.57	.69	.61	.63
HH has electricity	.08	.11	.07	.11
Head born in village	.67	.69	.69	.68
Land size	10.5	9.2	6.6	8.2
Land size >7 acres	.45	.48	.35	.48
Number of parcels	2.4	2.2	2.0	2.5
Plants coffee	.47	.55	.45	.42
>500 trees	.33	.40	.39	.36
Received PO loan	.28	.31	.34	.37
Neighbours can be trusted	.75	.73	.73	.78
Most people can be trusted	.53	.56	.55	.59
Trust PO leaders (proxy)	.71	.79	.75	.67

Note: *Trust PO leaders (proxy)* is a proxy variable created from a question that asked the respondents to rate the amount of time PO leaders would spend on a hypothetical community project. If the respondent esteemed that the PO leaders would spend more time on the project than ordinary PO members the proxy takes on value one. Variables `Age spouse` and `Yrs schooling spouse` all single households heads have missing values. Only two observations have missing value despite being married. Table A1 in the Appendix provides additional information on the sample.

Table 2: Summary Statistics of Labor Input

	First Season				Second Season			
	Cash-on-delivery	Information-on-sales	Both	Control	Cash-on-delivery	Information-on-sales	Both	Control
Panel A: Farming activities								
HH member labor days								
Baseline	179.8	231.3	205.5	205.2	191.3	239.2	207.0	216.0
Endline	201.9	259.6	204.1	185.8	233.3	248.1	255.8	185.1
Hired labor days								
Baseline	41.6	46.7	34.1	34.1	40.8	50.0	36.5	32.6
Endline	72.2	65.2	42.2	52.0	73.6	56.7	66.6	54.5
Change in hired labor proportion	.06	.05	.03	.02	.03	.03	.04	.10
Panel B: Crop production								
HH member labor days								
Baseline	57.2	66.5	62.0	64.9	70.9	67.2	68.7	67.8
Endline	81.4	107.7	86.6	75.2	108.0	121.4	138.6	86.5
Hired labor days								
Baseline	21.2	16.7	13.0	15.3	22.2	19.9	13.1	14.6
Endline	41.3	30.8	27.7	24.2	51.3	33.9	52.1	36.7
Change in hired labor proportion	.09	.08	.06	-.07	.08	.05	.05	-.05

Note: Tables A2 and A3 in the Appendix provide additional information.

Table 3: OLS - Impact on Labor Inputs

	First Season		Second Season	
	(1)	(2)	(3)	(4)
A: Impact on Household Member Labor Days in Farming Activities				
CoD	10.3	19.6	44.4	56.9
	(33.7)	(33.0)	(39.3)	(38.4)
IoS	65.1*	62.6*	59.9	55.2
	(36.5)	(36.8)	(39.9)	(39.2)
both	19.3	30.4	77.2	86.6*
	(27.8)	(28.7)	(47.1)	(44.5)
B: Impact on Household Member Labor Days in Crop Production				
CoD	5.4	8.6	21.1	26.4
	(15.6)	(15.7)	(21.0)	(21.5)
IoS	29.2*	27.2	37.0	31.6
	(17.2)	(16.6)	(26.2)	(25.5)
both	10.8	17.3	55.5**	62.3**
	(13.1)	(13.4)	(27.5)	(27.8)
C: Impact on Hired labor Days in Farming Activities				
CoD	20.1	18.5	18.8	15.1
	(23.2)	(24.2)	(26.4)	(27.0)
IoS	15.5	13.3	4.0	0.3
	(18.1)	(17.5)	(20.0)	(19.5)
both	-7.3	4.7	16.3	28.0
	(17.2)	(18.0)	(29.9)	(32.5)
D: Impact on Hired Labor Days in Crop Production				
CoD	17.0	15.0	14.4	11.4
	(12.2)	(12.2)	(18.8)	(19.4)
IoS	5.9	5.7	-1.8	-1.8
	(10.7)	(10.3)	(16.2)	(15.9)
both	3.8	11.9	18.6	28.8
	(11.9)	(12.9)	(28.1)	(30.6)
Observations	305	305	305	305
Ind. level cov.	no	yes	no	yes
Initial value of Y as cov.	no	yes	no	yes

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses clustered at PO-level.

All estimations include fixed effects for the Depot Committee.

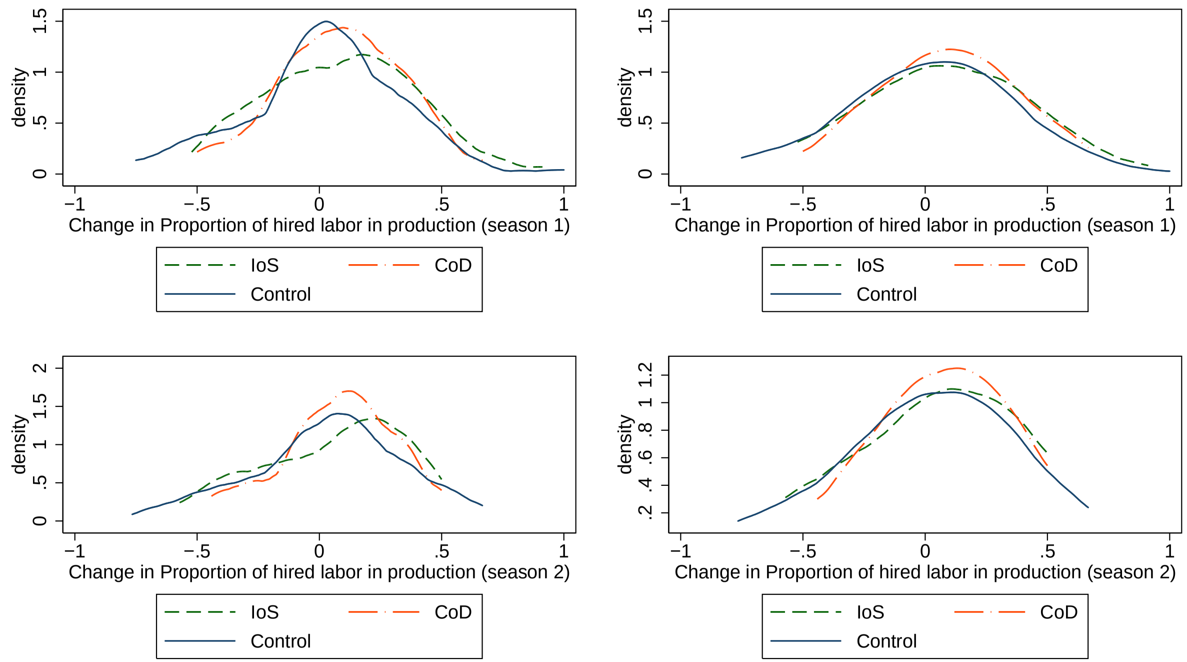

Figure 2: Changes in Proportion of Hired Labor Days in Total Days in Crop Production

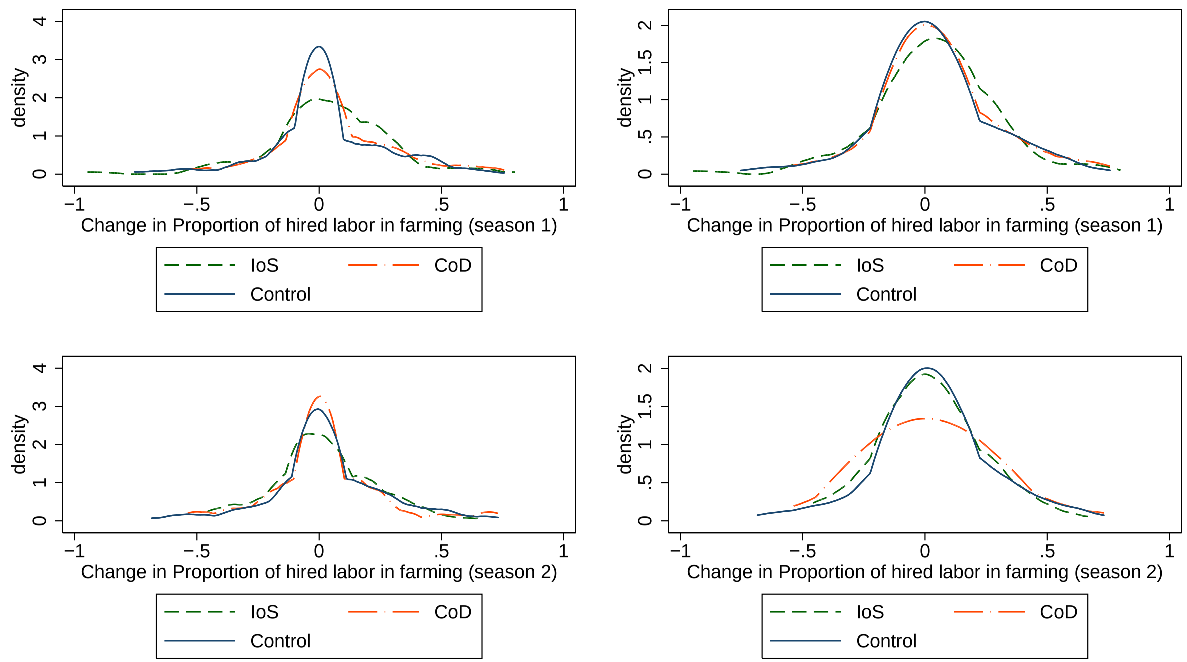

Figure 3: Changes in Proportion of Hired Labor Days in Total Days in Farming

Table 4: Summary Statistics of Non-Labor Inputs

	Cash-on-delivery	Information-on-sales	Both	Control
Manure Fertilizer	.	.	.	.
Baseline	.08	.09	.04	.08
Endline	.07	.05	.07	.03
Inorganic Fertilizer	.	.	.	.
Baseline	.09	.15	.11	.12
Endline	.17	.18	.15	.07
Pesticide/Fungicide	.	.	.	.
Baseline	.12	.16	.08	.11
Endline	.19	.31	.27	.11
Traditional Seeds	.	.	.	.
Baseline	.23	.2	.18	.14
Endline	.13	.2	.18	.11
Hybrid Seeds	.	.	.	.
Baseline	.12	.28	.15	.11
Endline	.23	.31	.24	.11
Soil Preparation	.	.	.	.
Baseline	.24	.29	.20	.23
Endline	.09	.14	.18	.14

Note: Cells show the share of households that have spent any money on the respective input in the last two seasons. The item *Soil Preparation* captures any costs for soil preparation (e.g. oxen, tractor, etc.). Table A7 in the Appendix provides additional information on sample size and mean comparisons.

Table 5: OLS - Impact on Non Labor Inputs

	Manure Fertilizer		Inorganic Fertilizer		Pesticide/Fungicide		Traditional Seeds		Hybrid Seeds		Soil Preparation	
A: Full Sample of Farmers												
CoD	0.03	0.04	0.09**	0.10**	0.05	0.05	0.04	0.03	0.09	0.09	-0.05	-0.06
	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
IoS	0.01	0.01	0.05	0.06	0.11**	0.10*	0.10*	0.10*	0.14**	0.13*	-0.01	-0.01
	(0.04)	(0.03)	(0.04)	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)	(0.07)	(0.06)	(0.06)
both	0.03	0.05	0.05	0.07	0.10**	0.12**	0.08	0.08	0.09	0.10*	0.03	0.04
	(0.03)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Observations	299	299	299	299	299	299	299	299	299	299	299	299
B: Sample of Farmers with Trust in PO Leaders												
CoD	0.06	0.07**	0.10**	0.10**	0.07	0.06	0.04	0.04	0.09	0.09	-0.06	-0.07
	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
IoS	0.01	0.01	0.05	0.07	0.14**	0.12*	0.11*	0.12*	0.10	0.10	-0.03	-0.04
	(0.03)	(0.03)	(0.05)	(0.05)	(0.06)	(0.07)	(0.06)	(0.06)	(0.08)	(0.08)	(0.07)	(0.07)
both	0.04	0.07**	0.07	0.09	0.11**	0.14***	0.05	0.05	0.09	0.10	0.05	0.05
	(0.03)	(0.04)	(0.06)	(0.06)	(0.05)	(0.05)	(0.06)	(0.06)	(0.07)	(0.07)	(0.07)	(0.07)
Observations	252	252	252	252	252	252	252	252	252	252	252	252
Ind. level cov.	no	yes	no	yes	no	yes	no	yes	no	yes	no	yes
Initial value of Y as cov.	no	yes	no	yes	no	yes	no	yes	no	yes	no	yes

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses clustered at PO-level.

All dependent variables are dummy variables indicating whether the farmer spent money on the specific technique in the last two seasons.

All estimations include fixed effects for the Depot Committee. *Soil Preparation* captures any costs for soil preparation (e.g. oxen, tractor, etc.).

Table 6: OLS - Impact on Labor Inputs among Farmers with Trust in PO Leaders

	First Season		Second Season	
	(1)	(2)	(3)	(4)
A: Impact on Household Member Labor Days in Farming Activities				
CoD	9.8	18.3	37.2	47.2
	(37.1)	(34.6)	(42.7)	(40.4)
IoS	82.3**	78.6*	75.8*	69.7*
	(40.0)	(40.3)	(42.4)	(40.6)
both	-4.4	7.7	52.3	67.4
	(32.5)	(32.0)	(51.6)	(50.0)
B: Impact on Household Member Labor Days in Crop Production				
CoD	-0.2	1.9	17.5	24.0
	(15.4)	(14.5)	(21.9)	(22.6)
IoS	40.6**	38.0**	52.6*	45.4*
	(18.6)	(17.4)	(27.9)	(26.5)
both	2.0	8.2	44.6	49.4
	(15.2)	(15.3)	(30.2)	(31.0)
C: Impact on Hired labor Days in Farming Activities				
CoD	22.9	17.2	25.4	18.8
	(21.6)	(20.5)	(25.8)	(24.5)
IoS	28.7	27.1	21.3	15.0
	(19.0)	(17.5)	(18.1)	(16.7)
both	-20.3	-4.6	-10.1	3.7
	(13.4)	(13.1)	(14.8)	(15.8)
D: Impact on Hired Labor Days in Crop Production				
CoD	19.5*	15.5	13.5	9.5
	(11.5)	(10.1)	(15.3)	(14.9)
IoS	14.6	12.1	12.3	8.7
	(11.2)	(9.7)	(14.2)	(12.5)
both	-10.1	-2.8	-7.1	1.9
	(6.9)	(6.6)	(12.4)	(13.0)
Observations	252	252	252	252
Ind. level cov.	no	yes	no	yes
Initial value of Y as cov.	no	yes	no	yes

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses clustered at PO-level.

All estimations include fixed effects for the Depot Committee.

Table 7: Summary Statistics Trust Outcomes at Endline

Variable Names	Mean of Variable				p-value of t-test with control group		
	CoD	IoS	Both	Control	CoD	IoS	Both
Trust most people	.63	.46	.54	.58	.53	.17	.63
Trust neighbours	.77	.69	.73	.82	.47	.06	.2
Trust PO leaders	.85	.86	.83	.82	.61	.49	.89

Note: Variables are indicators taking value one if respondent agrees to statement.

Table 8: OLS - Impact on Trust in PO Leaders

	(1)	(2)
Cash-on-Delivery	0.03	0.03
	(0.1)	(0.1)
Information-on-Sales	0.02	0.02
	(0.1)	(0.1)
Both	0.00	0.01
	(0.1)	(0.1)
Observations	299	299
R^2	0.037	0.070
Adjusted R^2	-0.003	-0.023
Individual level covariates	no	yes
Initial value of Y as covariate	no	no

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Standard errors in parentheses clustered at PO-level.
Specification (3) with initial level of Y as covariate cannot be estimated as Trust in PO leader was not measured at baseline. All estimations include fixed effects for the Depot Committee.

Appendix

Table A1: Summary Statistics of Household Characteristics at Baseline

	Number of Observations				Mean of Variable				p-value of t-test with control group		
Variable Names	CoD	IoS	Both	Control	CoD	IoS	Both	Control	CoD	IoS	Both
Household size	75	80	71	73	7.09	7.20	7.11	7.32	.64	.79	.67
Age head	75	80	71	73	49.56	49.04	53.28	48.77	.73	.90	.06
Age spouse	63	70	59	59	39.87	39.5	42.42	38.10	.34	.47	.04
Gender head	75	80	71	73	.15	.10	.13	.12	.68	.65	.95
Yrs schooling head	75	80	71	73	6.58	7.19	6.34	6.12	.45	.08	.71
Yrs schooling spouse	63	70	59	59	4.67	5.67	5.42	4.81	.83	.14	.36
Distance to market>5km	75	80	71	73	.33	.48	.32	.45	.14	.78	.12
Savings account bank	75	80	71	73	.16	.26	.10	.19	.61	.30	.11
Savings account MFI	75	80	70	73	.23	.35	.31	.29	.4	.41	.73
Informal saving	75	80	71	73	.41	.31	.25	.33	.29	.83	.32
Owns mobile phone	75	80	71	73	.57	.69	.61	.63	.48	.46	.76
HH has electricity	75	80	71	73	.08	.11	.07	.11	.54	.95	.42
Head born in village	75	80	71	73	.67	.69	.69	.68	.81	.97	.95
Land size	75	80	71	73	10.53	9.17	6.58	8.18	.42	.59	.16
Land size >7 acres	75	80	71	73	.45	.48	.35	.48	.75	.96	.12
Number of parcels	75	80	71	73	2.44	2.21	2.01	2.51	.75	.15	.01
Plants coffee	75	80	71	73	.47	.55	.45	.42	.61	.12	.75
>500 trees	75	80	71	73	.33	.40	.39	.36	.77	.58	.64
Received PO loan	72	77	71	70	.28	.31	.34	.37	.24	.45	.68
Neighbours can be trusted	75	80	71	73	.75	.73	.73	.78	.63	.43	.50
Most people can be trusted	75	80	71	73	.53	.56	.55	.59	.50	.74	.63
Trust PO leaders (proxy)	75	80	71	73	.71	.79	.75	.67	.64	.11	.32

Note: *Trust PO leaders (proxy)* is a proxy variable created from a question that asked the respondents to rate the amount of time PO leaders would spend on a hypothetical community project. If the respondent esteemed that the PO leaders would spend more time on the project than ordinary PO members the proxy takes on value one. `Age spouse` and `Yrs schooling spouse` are missing-by-construction for single headed households. Only two observations have missing value despite being married.

Table A2: Summary Statistics of Labor Input: First Season

	Number of Observations				Mean of Variable				p-value of t-test with control group		
Variable Names	CoD	IoS	Both	Control	CoD	IoS	Both	Control	CoD	IoS	Both
Farming activities											
HH member labor days											
Baseline	78	80	74	73	179.8	231.3	205.5	205.2	.40	.46	.99
Endline	78	80	74	73	201.9	259.6	204.1	185.8	.57	.02	.52
Hired labor days											
Baseline	78	80	74	73	41.6	46.7	34.1	34.1	.57	.38	.99
Endline	78	80	74	73	72.2	65.2	42.2	52	.33	.39	.53
Change in hired labor proportion	77	80	74	71	.06	.05	.03	.02	.41	.6	.93
Crop production											
HH member labor days											
Baseline	78	80	73	73	57.2	66.5	62.0	64.9	.41	.86	.79
Endline	78	80	74	73	81.4	107.7	86.6	75.2	.69	.05	.43
Hired labor days											
Baseline	78	80	74	73	21.2	16.7	13.0	15.3	.49	.81	.74
Endline	78	80	74	73	41.3	30.8	27.7	24.2	.13	.46	.77
Change in hired labor proportion	33	33	23	29	.09	.08	.06	-.07	.04	.06	.18

Table A3: Summary Statistics of Labor Input: Second Season

	Number of Observations				Mean of Variable				p-value of t-test with control group		
Variable Names	CoD	IoS	Both	Control	CoD	IoS	Both	Control	CoD	IoS	Both
Farming activities											
HH member labor days											
Baseline	78	80	74	73	191.3	239.2	207.0	216.0	.46	.55	.80
Endline	78	80	74	73	233.3	248.1	255.8	185.1	.15	.06	.09
Hired labor days											
Baseline	78	80	74	73	40.8	50.0	36.5	32.6	.50	.23	.78
Endline	78	80	74	73	73.6	56.7	66.6	54.5	.41	.88	.67
Hired labor proportion	76	80	74	71	.03	.03	.04	.05	.54	.51	.79
Crop production											
HH member labor days											
Baseline	78	80	74	73	70.9	67.2	68.7	67.8	.78	.95	.94
Endline	78	80	74	73	108.0	121.4	138.6	86.5	.27	.13	.04
Hired labor days											
Baseline	78	80	74	73	22.2	19.9	13.1	14.6	.32	.41	.78
Endline	78	80	74	73	51.3	33.9	52.1	36.7	.42	.82	.57
Hired labor proportion	30	29	26	25	.08	.05	.05	-.05	.15	.30	.30

Table A4: Summary Statistics of Trust Measures at Endline

	Number of Observations				Mean of Variable				p-value of t-test with control group		
Variable Names	CoD	IoS	Both	Control	CoD	IoS	Both	Control	CoD	IoS	Both
Trust most people	75	80	71	73	.63	.46	.54	.58	.53	.17	.63
Trust neighbours	75	80	71	73	.77	.69	.73	.82	.47	.06	.2
Trust PO leaders	75	80	71	73	.85	.86	.83	.82	.61	.49	.89

Note: Variables are indicators taking value one if respondent agrees to statement.

Table A5: OLS - Impact on Household Member Labor Days by Farm Size (big farms larger than 5 hectare)

	Days in Farming Activities				Days in Crop Production			
	First Season		Second Season		First Season		Second Season	
CoD	42.0	46.5	51.0	62.1	8.0	8.8	23.9	18.8
	(45.0)	(47.1)	(47.4)	(50.6)	(22.3)	(24.0)	(23.7)	(26.3)
IoS	53.4	67.7	54.7	60.5	19.8	28.9	33.3	38.9*
	(39.2)	(41.6)	(42.4)	(41.9)	(19.6)	(20.0)	(25.1)	(22.5)
both	44.8	50.7	104.6	99.6	21.5	22.7	78.8*	86.1*
	(40.7)	(41.8)	(76.6)	(69.4)	(21.3)	(22.1)	(44.2)	(44.8)
Observations	165	165	165	165	165	165	165	165
CoD	-16.7	-21.2	41.2	32.7	5.8	2.2	15.9	6.7
	(45.0)	(44.1)	(57.1)	(58.7)	(19.5)	(20.2)	(33.9)	(36.7)
IoS	61.9	47.0	43.5	14.2	32.1	16.7	27.8	4.6
	(65.9)	(64.1)	(73.6)	(75.2)	(29.8)	(25.7)	(51.0)	(47.9)
both	16.4	4.9	66.5	56.5	11.9	5.6	39.2	25.5
	(42.6)	(42.1)	(55.8)	(57.9)	(19.8)	(20.9)	(36.4)	(40.6)
Observations	140	140	140	140	140	140	140	140
Ind. level cov.	no	yes	no	yes	no	yes	no	yes
Initial value of Y as cov.	no	yes	no	yes	no	yes	no	yes

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses clustered at PO-level. All estimations include fixed effects for the Depot Committee.

Table A6: OLS - Impact on Household Member Labor Days by Farm Size (big farms larger than 3 hectare)

	Days in Farming Activities				Days in Crop Production			
	First Season		Second Season		First Season		Second Season	
CoD	17.7	19.0	41.2	50.3	3.9	4.7	18.8	18.6
	(38.2)	(37.3)	(41.7)	(41.8)	(17.1)	(17.5)	(19.9)	(21.0)
IoS	62.3	71.4*	57.8	67.5	25.9	31.1*	40.5	43.7
	(38.3)	(39.2)	(43.2)	(43.8)	(17.8)	(17.1)	(28.8)	(28.5)
both	32.3	42.1	96.7*	105.8*	15.4	20.6	74.9**	82.3**
	(32.8)	(35.2)	(56.8)	(55.7)	(17.3)	(18.2)	(33.9)	(35.5)
Observations	229	229	229	229	229	229	229	229
CoD	-23.7	10.8	25.1	68.0	-5.5	18.4	-4.4	12.8
	(55.6)	(67.6)	(68.3)	(85.0)	(29.5)	(33.9)	(41.7)	(44.5)
IoS	41.8	71.4	0.9	40.8	13.3	19.4	-31.8	-20.5
	(72.9)	(74.0)	(73.8)	(89.6)	(37.0)	(38.2)	(43.6)	(55.9)
both	-35.3	-24.4	-15.1	34.4	-15.7	3.2	-34.4	-2.4
	(50.9)	(53.7)	(56.2)	(68.2)	(24.5)	(28.1)	(35.1)	(39.0)
Observations	76	76	76	76	76	76	76	76
Ind. level cov.	no	yes	no	yes	no	yes	no	yes
Initial value of Y as cov.	no	yes	no	yes	no	yes	no	yes

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses clustered at PO-level.
All estimations include fixed effects for the Depot Committee.

Table A7: Summary Statistics of Non-Labor Inputs

	Number of Observations				Mean of Variable				p-value of t-test with control group		
Variable Names	CoD	IoS	Both	Control	CoD	IoS	Both	Control	CoD	IoS	Both
Manure Fertilizer											
Baseline	75	80	71	73	.08	.09	.04	.08	.96	.91	.33
Endline	75	80	71	73	.07	.05	.07	.03	.26	.48	.23
Inorganic Fertilizer											
Baseline	75	80	71	73	.09	.15	.11	.12	.56	.63	.84
Endline	75	80	71	73	.17	.18	.15	.07	.05	.05	.10
Pesticide/Fungicide											
Baseline	75	80	71	73	.12	.16	.08	.11	.84	.35	.61
Endline	75	80	71	73	.19	.31	.27	.11	.19	.00	.01
Traditional Seeds											
Baseline	75	80	71	73	.23	.20	.18	.14	.16	.30	.45
Endline	75	80	71	73	.13	.20	.18	.11	.66	.13	.21
Hybrid Seeds											
Baseline	75	80	71	73	.12	.28	.15	.11	.84	.01	.43
Endline	75	80	71	73	.23	.31	.24	.11	.06	.00	.04
Soil Preparation											
Baseline	75	80	71	73	.24	.29	.20	.23	.92	.45	.61
Endline	75	80	71	73	.09	.14	.18	.14	.41	.99	.45

Note: *Soil Preparation* captures any costs for soil preparation (e.g. oxen, tractor, etc.).

Table A8: OLS - Impact on Quantity of Harvest Produced/by plot size (Maize and Coffee)

	(First Season)		(Second Season)		(Both Seasons)	
A: Full Sample of Farmers						
CoD	35.8	30.6	30.0	23.1	65.8	53.7
	(26.7)	(27.8)	(26.6)	(27.1)	(52.9)	(54.6)
IoS	-14.3	-14.1	-6.2	-7.4	-20.5	-21.5
	(27.6)	(33.5)	(28.6)	(34.0)	(55.6)	(67.1)
both	68.6*	72.9**	70.4*	74.6**	139.0*	147.6**
	(38.3)	(35.5)	(38.4)	(35.3)	(76.7)	(70.7)
Observations	305	305	305	305	305	305
Adjusted R^2	0.138	0.154	0.123	0.153	0.131	0.155
B: Sub-Sample of Coffee Farmers						
CoD	48.0**	42.8**	47.9**	42.2*	95.9**	85.0**
	(19.7)	(21.4)	(20.7)	(21.5)	(39.8)	(42.3)
IoS	4.3	16.4	8.7	22.0	12.9	38.4
	(16.4)	(19.2)	(17.9)	(19.8)	(33.9)	(38.5)
both	64.1**	70.0**	63.0**	70.0**	127.2**	140.0**
	(28.7)	(30.3)	(29.1)	(30.3)	(57.6)	(60.4)
Observations	181	181	181	181	181	181
Adjusted R^2	0.249	0.258	0.229	0.251	0.240	0.256
C: Sub-Sample of Maize Farmers						
CoD	17.9	44.5	4.2	34.6	22.1	79.1
	(59.9)	(64.2)	(58.8)	(62.3)	(118.1)	(125.7)
IoS	-39.8	-9.2	-26.8	8.6	-66.7	-0.7
	(62.9)	(66.9)	(65.1)	(67.9)	(126.7)	(133.1)
both	76.0	140.5	82.8	162.1*	158.9	302.5*
	(89.7)	(86.5)	(89.7)	(83.8)	(179.4)	(169.7)
Observations	124	124	124	124	124	124
Adjusted R^2	0.042	0.030	0.030	0.063	0.036	0.046
Ind. level cov.	no	yes	no	yes	no	yes

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses clustered at PO-level. All estimations include fixed effects for the Depot Committee. All quantities are in kilograms.

Table A9: OLS - Impact on Household Labor Days

	(Household Labor in Farming)				(Household Labor in Crop Production)			
	(First Season)		(Second Season)		(First Season)		(Second Season)	
A: Full Sample of Farmers								
CoD	10.3	19.6	44.4	56.9	5.4	8.6	21.1	26.4
	(33.7)	(33.0)	(39.3)	(38.4)	(15.6)	(15.7)	(21.0)	(21.5)
IoS	65.1*	62.6*	59.9	55.2	29.2*	27.2	37.0	31.6
	(36.5)	(36.8)	(39.9)	(39.2)	(17.2)	(16.6)	(26.2)	(25.5)
both	19.3	30.4	77.2	86.6*	10.8	17.3	55.5**	62.3**
	(27.8)	(28.7)	(47.1)	(44.5)	(13.1)	(13.4)	(27.5)	(27.8)
Observations	305	305	305	305	305	305	305	305
Adjusted R^2	0.029	0.106	0.084	0.163	0.075	0.162	0.111	0.155
B: Sub-Sample of Coffee Farmers								
CoD	-4.6	11.5	34.0	51.9	18.2	23.8	37.5	56.8*
	(36.4)	(39.3)	(46.4)	(51.0)	(20.7)	(22.7)	(30.8)	(34.0)
IoS	50.1	54.8	54.8	59.2	35.7	33.4	38.7	38.8
	(51.8)	(51.6)	(60.0)	(61.4)	(25.4)	(24.5)	(42.3)	(43.8)
both	15.8	37.0	108.5	127.4*	17.4	30.0	74.6*	84.4**
	(34.6)	(36.7)	(70.3)	(67.3)	(17.5)	(19.7)	(41.6)	(41.8)
Observations	181	181	181	181	181	181	181	181
Adjusted R^2	0.050	0.138	0.101	0.184	0.066	0.183	0.084	0.175
C: Sub-Sample of Maize Farmers								
CoD	32.6	85.5	57.4	119.4*	-13.7	-8.2	-3.9	2.1
	(64.9)	(61.6)	(69.7)	(65.6)	(23.3)	(24.5)	(24.7)	(25.9)
IoS	86.8	117.0**	63.8	112.8**	18.8	12.5	31.4	51.6**
	(52.1)	(58.2)	(48.3)	(53.5)	(21.2)	(25.1)	(20.7)	(23.3)
both	24.9	86.7	25.3	110.4*	0.2	9.5	24.2	54.3*
	(48.4)	(55.9)	(44.9)	(59.7)	(19.0)	(22.5)	(25.0)	(31.2)
Observations	124	124	124	124	124	124	124	124
Adjusted R^2	-0.011	0.205	-0.021	0.196	0.016	0.274	-0.012	0.144
Ind. level cov.	no	yes	no	yes	no	yes	no	yes
Initial value of Y as cov.	no	yes	no	yes	no	yes	no	yes

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses clustered at PO-level. All estimations include fixed effects for the Depot Committee.

Table A10: OLS - Impact on Hired Labor Days

	(Hired Labor in Farming)				(Hired Labor in Crop Production)			
	(First Season)		(Second Season)		(First Season)		(Second Season)	
A: Full Sample of Farmers								
CoD	20.1	18.5	18.8	15.1	17.0	15.0	14.4	11.4
	(23.2)	(24.2)	(26.4)	(27.0)	(12.2)	(12.2)	(18.8)	(19.4)
IoS	15.5	13.3	4.0	0.3	5.9	5.7	-1.8	-1.8
	(18.1)	(17.5)	(20.0)	(19.5)	(10.7)	(10.3)	(16.2)	(15.9)
both	-7.3	4.7	16.3	28.0	3.8	11.9	18.6	28.8
	(17.2)	(18.0)	(29.9)	(32.5)	(11.9)	(12.9)	(28.1)	(30.6)
B: Sub-Sample of Coffee Farmers								
CoD	12.5	17.3	12.1	11.7	20.1	23.1	20.1	20.5
	(32.9)	(34.1)	(35.7)	(37.1)	(18.7)	(17.7)	(30.3)	(32.2)
IoS	19.2	22.6	4.6	3.3	13.9	12.7	-0.1	2.2
	(28.0)	(26.0)	(32.5)	(34.1)	(16.8)	(15.1)	(27.2)	(28.8)
both	-23.9	-2.0	19.5	36.9	-7.6	1.2	24.1	37.1
	(23.6)	(26.1)	(47.1)	(54.3)	(15.0)	(17.0)	(44.8)	(52.1)
C: Sub-Sample of Maize Farmers								
CoD	32.1	44.7	28.2	41.9	12.9	8.6	5.8	9.3
	(31.5)	(36.8)	(38.9)	(46.2)	(12.7)	(17.2)	(13.8)	(17.0)
IoS	12.7	39.6*	3.2	30.0	-3.8	0.1	-5.0	5.0
	(18.3)	(23.0)	(15.3)	(24.6)	(8.5)	(11.3)	(7.3)	(10.5)
both	19.9	62.9**	10.9	60.7	22.8	32.7**	9.7	28.4**
	(22.4)	(27.9)	(17.7)	(37.8)	(17.5)	(12.9)	(13.6)	(13.9)
Ind. level cov.	no	yes	no	yes	no	yes	no	yes
Initial value of Y as cov.	no	yes	no	yes	no	yes	no	yes

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses clustered at PO-level. All estimations include fixed effects for the Depot Committee.

Table A11: OLS - Impact on the price (per kilo) at which the last three or largest three sales in the preceding 12 months were made

	(Largest/Last Sale)		(Second Largest/Last Sale)		(Third Largest/Last Sale)	
A: Full Sample of Farmers						
CoD	303.1	282.1	205.0	163.4	-428.8	-232.3
	(220.5)	(220.1)	(317.9)	(299.7)	(540.6)	(511.8)
IoS	108.2	65.7	478.3*	559.5*	142.1	200.5
	(209.7)	(218.0)	(279.4)	(301.5)	(440.1)	(500.8)
both	-20.1	23.9	71.2	182.0	228.4	251.9
	(223.4)	(234.0)	(291.2)	(338.6)	(448.2)	(501.4)
Observations	273	267	180	178	74	74
Adjusted R^2	0.769	0.769	0.672	0.651	0.572	0.527
B: Sub-Sample of Coffee Farmers						
CoD	421.8	302.2	260.1	226.5	-514.0	-302.1
	(323.5)	(330.8)	(402.6)	(376.0)	(605.1)	(634.0)
IoS	134.3	52.1	652.8*	836.8**	157.5	313.0
	(326.7)	(350.1)	(361.8)	(402.5)	(505.3)	(684.0)
both	-57.6	-102.8	132.2	283.4	207.3	232.2
	(319.4)	(353.8)	(358.6)	(405.8)	(482.3)	(575.9)
Observations	179	177	143	142	65	65
Adjusted R^2	0.478	0.488	0.488	0.458	0.489	0.446
C: Sub-Sample of Maize Farmers						
CoD	61.7	71.6	72.8	148.3	387.1***	330.8
	(46.2)	(61.5)	(83.7)	(120.3)	(42.2)	.
IoS	50.3	54.0	-79.3	36.5	-75.7	178.1
	(47.8)	(59.4)	(68.6)	(98.2)	(45.7)	.
both	72.6	117.6**	-85.5	60.3	.	.
	(45.8)	(58.1)	(100.4)	(127.2)	.	.
Observations	94	90	37	36	9	9
Adjusted R^2	0.121	0.139	0.016	0.039	0.460	.
Ind. level cov.	no	yes	no	yes	no	yes

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses clustered at PO-level. All estimations include fixed effects for the Depot Committee. All quantities are in kilograms.