Impacts of Smallholder Contract Farming in Uganda

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Evidence from the "IMPROVE" Coffee Scheme

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Abstract:
This master thesis analyzes the effects of smallholder participation in contract farming on profits and adoption of new farming techniques in a tropical African context. We use a unique panel data set collected by the Danish Institute for International Studies to identify the effects of continued scheme participation for a group of smallholder coffee farmers relative to a control group of non-scheme farmers. Our results show that participation in the contract farming scheme between 2005 and 2008 has no statistically significant effect on profits from coffee production. However, project farmers respond to scheme trainings by applying more "good agricultural practices" in coffee production.

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List of Abbreviations

CWD:	Coffee Wilt Disease
DIIS:	Danish Institute for International Studies
DiD:	Difference in Difference
EU:	European Union
FD:	First Difference
FE:	Fixed Effects
GAPs:	Good agricultural practices
Ibero:	Ibero (U) Ltd.
OLS:	Ordinary Least Squares
RCTs:	Randomized controlled trials
UCDA:	Ugandan Coffee Development Authority

1. Introduction

This master thesis analyzes the impacts of participation in the IMPROVE contract farming scheme in Eastern Uganda. Scheme participants are smallholder coffee farmers who agree to sell their coffee produce directly to a large international exporting company instead of marketing it through conventional channels. Farmers can participate in scheme trainings in coffee management practices and receive a price premium in return for compliance with certain quality standards. We are interested in identifying the effects on farmers' profits from coffee production and changes in coffee production practices that may arise from participation in the IMPROVE contract farming scheme.

Our research questions are directly linked to the rising concern that smallholder farmers in developing countries may benefit too little from the opportunities associated with the internationalization of agricultural markets. Constraints on technology, credit and production levels disconnect smallholder producers from consumers in more industrialized countries. In this context, contract farming is advocated as a pathway for the integration of smallholders from developing countries into more profitable value chains. However, surprisingly few academic studies provide empirical evidence that smallholder engagement in such contracts indeed benefits the farmers. A lack of comprehensive farm-level survey data may account for this shortcoming in literature. Additionally, past research has struggled to overcome the empirical challenge associated with non-random selection into most contracting schemes.

We study the question at hand on the basis of an extensive data set collected by the Danish Institute for International Studies (DIIS). Our analysis revolves around the development of coffee profits and farm practices in a group of 103 contract farmers relative to 101 similar non-contracting farmers between 2005 and 2008. We explicitly account for possible non-random selection into the scheme by running a series of Difference in Difference (DiD) estimations. Our results suggest that participating farmers respond strongly to scheme trainings. Participants adopt significantly more "good agricultural practices" (GAPs) in the process of coffee farming relative to the control farmers. In contrast, we find no evidence that scheme participation leads to an increase in household income from coffee farming.

Our findings add to the existing knowledge about smallholder participation in contract farming in developing countries. Such contracts should provide farmers and contracting companies with sufficient participation incentives and lead to sustainable benefits for both parties. Our results, for instance, show that the training component of a contract farming scheme can be effective in promoting new farm practices. Other schemes may draw on the IMPROVE example in the setup of trainings to spread innovation and know-how among farmers. The pertinent question of whether contract farming should be facilitated in developing countries depends largely on the welfare impact for participating farmers. Our findings can contribute to this debate.

We organize our work in eight sections. After the introduction, section 2 presents a brief overview of previous research on contract farming in developing countries. Section 3 provides relevant background information on the context of coffee farming in Uganda and the setup of the IMPROVE contract farming scheme. Section 4 contains our hypotheses. In section 5 we describe the specifics of our data, our empirical research strategy and the methods we use to estimate scheme impacts. We present the empirical results of our analysis throughout section 6. In section 7 we discuss the validity of our results with respect to the limitations in our research design. We provide a short summary of our work and end our thesis with concluding remarks in section 8.

2. Literature Review

Our analysis of the IMPROVE contract farming scheme belongs to the broad category of impact evaluation studies. One of the great challenges in impact evaluation is to identify the causal effects of a specific intervention. Doing so requires an understanding of counterfactual development. More specifically, how would the outcome variable of interest have changed if the treated population had not been exposed to or participated in the treatment of interest (Ravallion, 2001)?

The literature review consists of an overview of popular approaches used in the impact evaluation literature and selected examples of how these have been used in the past. We then proceed to outline the current state of research on the impacts of contract farming. We focus on important empirical studies and give a brief overview of opportunities and threats involved in contract farming in developing countries.

Over the past decades academic research has produced vast amounts of literature concerned with the establishment of causal impacts of policy change. Classification of these studies requires an understanding of the source of treatment variation. If treatment variation can be controlled completely, randomized controlled trials (RCTs)[1] can estimate the impact of a policy change accurately. Successful randomization of treatment ensures that the only difference between treatment and control group is the impact of receiving treatment itself (Duflo, Glennerster, & Kremer, 2007). That is, the control group delivers adequate information on the counterfactual development of the treatment group. Especially in the field of development economics, RCTs have become the method of choice throughout the last decade. Areas where RCTs have been employed include but are not limited to: health economics (Miguel & Kremer, 2004) , education (Angelucci, De Giorgi, Rangel, & Rasul, 2010; Duflo, Hanna, & Ryan, 2012), gender studies (Ashraf, 2009) and farming (Duflo, Kremer, & Robinson, 2008) to name just a few. However, in many cases ethical concerns, monetary restrictions or the unavailability of control groups obviate estimation by RCTs.

When assignment to treatment cannot be controlled researchers often turn to "natural experiments" or "quasi experiments" to evaluate the development of an outcome variable of interest in treated and untreated populations (Meyer, 1995). When characteristics for treatment selection are also relevant for the development of the outcome variable, estimation of the treatment impact can be biased.

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[1] RCTs were first introduced in clinical studies to test the effectiveness of medical drugs, but have lately become a popular tool to assess causality in impact evaluations. By randomly assigning subjects into treatment and control group selection bias is avoided and, given a large enough sample size, it is possible to measure the causal effect of treatments.

Selection bias can be reduced when control and treatment group are observed over multiple time periods. In this case panel data techniques can be useful to control for both observed and unobserved individual characteristics that influence participation and impact the outcome variable. Card & Krueger (1994) study the effect of a change in minimum wages in New Jersey on employment in fast food restaurants, using restaurants in Pennsylvania as control group. The differences in growth rates in employment before and after the policy change in both states are compared to get an estimate of the effect of the new policy. Meyer, Viscusi, & Durbin (1995) analyze the elasticity of time out of work with respect to changes in workers compensation during injury-leave. They construct the DiD estimator after raises in maximum benefits in Kentucky and Michigan. Changes in out of work time of employees affected by the raise are compared to the change for workers whose incentives were not changed by the new maximum benefit.

The above studies have in common that the source of variation in the main explanatory variable is apparent. Meyer (1995) points out that the reliability of findings from quasi-experiments improves when factors that influence participation or exposure to treatment are clearly understood. In the context of contract farming, selection of scheme participants often depends on a variety of factors. The contracting company may prefer farmers with certain favorable characteristics and farmers make an active choice to accept or reject a contract. Therefore, selection can depend on both observed and unobserved characteristics. When assigning causality to participation impacts, selection by the contracting company and the farmers must be accounted for. In an effort to control for selection into the scheme a handful of studies use instrumental variables, or the closely related Heckman correction, to establish the causal impact of scheme participation on farm profits (Bellemare, 2012; Miyata, Minot, & Hu, 2009; Rao & Qaim, 2011; Warning & Key, 2002). In these studies, the authors find a significant positive impact from contract farming on selected income variables. The estimates range from remarkable 20% to 50% income increase from scheme participation. However, all authors admit to imperfections and limitations in their respective identification strategies and emphasize that significance and sign of the estimate should be acknowledged, rather than the magnitude. In a study of contract arrangements of farmers' organizations in Nicaragua with the international retailer Wal-Mart Michelson, Reardon, & Perez (2012) investigate scheme impacts on producers mean output prices and price stability. They find that participating smallholders accept significantly lower average prices from the contract compared to average prices on spot markets. Although contracting with Wal-Mart reduces price volatility the authors suggest that farmers could be paying a too high price for this risk reduction. Minten et al. (2009) study the impacts of contractual agreements between vegetable farmers in the highlands of Madagascar and European supermarket chains. Their analysis of trainings, technology improvement and quality controls on small- and micro-farmers suggests a significant overall improvement of smallholders' welfare. However, their study relies on farmers self perceived changes in income and welfare versus that of a control group and does not incorporate an objective measurement of income. Ashraf, Gine, & Karlan (2009) in cooperation with DrumNet, a Kenyan NGO, run a randomized field experiment to examine the effects of adopting export-oriented crops on smallholder farms. DrumNet provides farmers with service packages designed to mimic those of a typical contractor and connects participants with exporting companies. In the experiment different service packages are randomly assigned to farmers and income is measured in baseline and follow up surveys. The study finds a positive and significant welfare impact for first time adaptors of the export crop. In contrast, there is no significant impact for farmers who already grew the respective export crop prior to the intervention. One year after the follow up survey was completed the DrumNet scheme collapsed. Due to repeated non-compliance with quality standards, exporting companies stopped purchasing the produce of DrumNet farmers.

In contrast to the small number of empirical studies on contract farming in developing countries, a broader body of literature explores contract farming using either a theoretical perspective or qualitative data from case studies. Most research agrees that contractual agreements provide the potential to improve smallholders' welfare perspectives in developing countries. Integration into larger supply chains can relieve constraints on credit and technology that otherwise restrain smallholders from participating in lucrative international agriculture markets (Key & Runsten, 1999; Reardon, 2003). Convergence of technology can improve productivity of contracted and non-contracted crops and social learning can spread innovations to non-contracted farmers (Foster & Rosenzweig, 1995; Minten et al., 2009; Weber, 2012). Additionally, positive spillover effects in terms of improved infrastructure and employment could foster rural development and poverty alleviation on a larger scale (Minten et al., 2009; Warning & Key, 2002).

However, some academic researchers also voice considerable concerns regarding social and economic impacts of contract farming arrangements in developing countries (Porter & Phillips-Howard✠, 1997; Singh, 2002; Watts, 1994). Watts (1994) points towards the potentially exploitative nature of contract arrangements in capitalist accumulation and provides examples of contract schemes that were overall harmful for participating farmers. Increased specialization on cash crops at the expense of subsistence farming can decrease the smallholder's future bargaining power and lead the way to an unfavorable dependence on the contractor (Porter & Phillips-Howard, 1997). Other papers indicate that contracting entities prefer better endowed farmers in terms of land, capital or landed capital, thus excluding small and micro farmers from participation (Gibbon, 2003; Kirsten & Sartorius, 2002; Reardon & Barrett, 2000). Such selection could potentially increase the inequality between the poorest farmers and those who are better off to begin with.

Overall, the existing body of literature does not provide a final answer to whether contract farming generates desirable welfare impacts for farmers in developing countries. Contractual arrangements have the potential to facilitate rural development and poverty alleviation, but only few studies can empirically confirm a welfare improvement for contracted farmers.

3. Background

3.1. Context of Coffee Production in Uganda

According to the Ugandan Coffee Development Authority (UCDA) the Ugandan coffee sector employs over 3.5 million households including about 500.000 small-scale coffee growers. 90% of the coffee producers are smallholder farmers with an average farm size ranging from 0.5 to 2.5 hectares of land. Farmers typically intercrop coffee trees with food crops to ensure subsistence consumption, shade coffee trees and maintain soil fertility (Daviron & Ponte, 2005). The level of farm inputs in terms of chemical fertilizers and machinery is low and production heavily depends on family labor (UCDA, 2013).

After plantation coffee trees require approximately two years before they carry beans. Optimal yields are realized after two to three additional years (Daviron & Ponte, 2005). Therefore, it takes about 5 years for a Robusta coffee tree to reach full productivity[2]. After each coffee harvest farmers can engage in coffee processing which involves cleaning the coffee from dirt and reducing the moisture content until the coffee can be roasted or exported. UCDA recommends that farmers dry the beans on mats or tarpaulins. However, recent studies show that most farmers dry coffee beans on the bare ground contributing to substantial post-harvest losses (Babigumari, 2007).

Around 80% of all coffee produced in Uganda is Robusta coffee and 20% of production corresponds to Arabica type coffee (UCDA, 2013). Arabica coffee beans are grown in the higher altitude areas close to the Kenyan border and in the mountain areas west of the country. Robusta beans grow at lower altitudes, contain more caffeine and their taste is bitter compared to Arabica coffee. Ugandan Robusta is known for its distinctive characteristics in the creation of foam layers in different coffee blends and has been sold at a premium over the world market price in the past (You & Bolwig, 2006).

Throughout recent years, one of the largest challenges for the Ugandan Robusta production is the spread of Coffee Wilt Disease (CWD). CWD infected about 45% of the original Robusta coffee trees between 1993 and 2005 (You & Bolwig, 2006). UCDA initiated a national program to destroy affected trees and teach good management practices to limit the spread of the infection (UCDA, 2013).

[2] All farmers in the contract farming scheme grow and sell Robusta type coffee which is especially common in Uganda.

Despite a higher diversification of the Ugandan economy in recent years, coffee accounted for about 20% of Uganda's total exports from 2008 to 2012 (COMTRADE, 2013). Since market liberalizations in 1993 total coffee exports have been fairly stable with a historic high in the 1996/1997 season and a downturn in 2003. In the period from market liberalization until present day Uganda has accounted for roughly 3% of the total world market export of coffee (FAOSTAT, 2013). The buyers of Ugandan coffee are primarily countries in the EU, but Uganda also exports a substantial amount of its coffee to the neighboring country Sudan. In the 2012/2013 season EU imported about 50% and Sudan around 20% of all Ugandan coffee (UCDA September, 2013). The world market price for Robusta coffee beans, measured as the New York ex-dock price, has been quite volatile. Prices hit a low in October 2001, but surged in subsequent years reaching a local maximum in March 2008. From January 2005 to December 2008 the world market price for Robusta beans increased by 112% (Index Mundi, 2013).

As of September 2013 Uganda has 34 registered coffee exporting companies. The ten largest exporters account for 70 % of all coffee exports. Ibero (U) Ltd (Ibero), the company operating the IMPROVE contract farming scheme, is the ninth largest coffee exporter with a market share of 4.2% of total Ugandan coffee exports, equivalent to 161.269 bags (60 kg) of coffee. Ibero specializes in the export of Robusta coffee beans from smallholder production. 93% of the company's exports consisted of Robusta coffee beans in the 2012/13 coffee season. (UCDA September, 2013).

3.2. IMPROVE Contract Farming Scheme[3]

The IMPROVE contract farming scheme is operated by the coffee exporting firm Ibero (U) Ltd a subsidiary of the Germany based Neumann Kaffee Gruppe. In 2003 IMPROVE started operating in Kisozi sub county, Kamuli district in Eastern Uganda. By 2005 the scheme had contracted 2349 smallholder coffee farmers corresponding to approximately 31% of all households in Kisozi. The number of contracted farmers increased to 3500 by 2008.

In 2003 Ibero project leaders together with a sub-county chairman determined communities with favorable coffee growing conditions in Kamuli district to participate in the scheme. At the community level a lead farmer and an appointed community chairman (often the same person) invited farmers to take part in the scheme. There was no formal membership fee or other explicit

[3] All information about the context of the IMPROVE scheme was obtained from notes and through personal interviews with Simon Bolwig who was at the time the person responsible at DIIS for managing the survey.

requirement for participation except a general interest in managing the coffee farm and to learn new farming practices. The scheme is structured in a hierarchy of project farmers, lead farmers, site coordinators and field officers. Each lead farmer is in charge of a production organization of 25 farmers and is responsible of coordinating the coffee sales. Site coordinators and field officers conduct annual and semi-annual farm inspections where they monitor the coffee production of participating farmers and give technical advice for the improvement of farm practices. Further, site coordinators and field officers organize training sessions for project farmers on coffee tree management and coffee production practices. Site operators and lead farmers jointly operate demonstration plots to give an example of how coffee production should be managed.

All project farmers sign the same contract with Ibero in which the farmers commit to apply "good agricultural practices" and follow "sustainable coffee farming principles". Some practices specified in the contract are meant to improve environmental sustainability and social responsibility while other practices aim at increasing the productivity and quality of coffee production. One key regulation is that coffee should be dried on raised platforms or tarpaulins. Although the contract does not contain any specific quality measures, it is Ibero's policy to purchase only clean and fully processed coffee i.e. with moisture content below 13%. Project farmers cannot sell undried or partly dried coffee to Ibero. The contract binds Ibero to buy the coffee and to provide participating farmers with trainings and support without explicitly specifying the nature or frequency of these inputs. However, trainings take place on a regular basis and a large proportion of farmers receive equipment and/or other inputs from Ibero such as seedlings, tarpaulins and fertilizers. Compared to other contract farming schemes the IMPROVE contract is relatively unbinding in the sense that it does not oblige farmers to sell their coffee to Ibero nor does it specify the premium over market price that is paid to the project farmers.

In 2005 coffee sales took place on a weekly basis at designated selling points, which were communicated through the lead farmer to the members of each production organization. Farmers transported the coffee to the selling points where it was unloaded to Ibero trucks. At delivery farmers were paid individually and in cash. In 2008 Ibero substituted the truck pickup system with fixed selling deposits in every project parish. Since 2008 farmers sell their coffee to the Ibero deposit managers. Recruitment of sales managers and organization of cash flow caused a delay in the 2008 selling season. Farmers started to sell to the project as late as mid November of 2008 instead of October. Ibero announces coffee prices on a daily basis. All project farmers receive the same price for processed coffee on any given day. There is no direct price negotiation between the farmers and Ibero. Since farmers are not bound by contract to sell to the IMPROVE scheme, Ibero is in constant competition with other buyers in the region. Middlemen purchase coffee at the farm gate, where the price is negotiated directly. In some cases middlemen can match or even exceed the price offered by Ibero. In contrast to Ibero, middlemen also buy unprocessed or partly dried coffee at considerably lower prices.

4. Hypotheses

Based on the nature of the IMPROVE contracting scheme we expect participation to impact the levels of profit obtained from coffee farming. Further, we are interested in the question whether project farmers adjust their agricultural practices in response to the trainings offered by Ibero. We define the following hypotheses:

> A: Participation in the IMPROVE contract farming scheme increases households profit from coffee production.

> B: Participation in the IMPROVE contract farming scheme leads to an adoption of good agricultural practices in the process of coffee production.

Since land endowment can vary substantially between individual households we measure profitability in terms of coffee profit per hectare of operated coffee land. We calculate coffee profit as revenue from all coffee sales in the season prior to the household survey, minus all costs for the production and marketing of coffee in the same time period. Profits from coffee production are therefore a function of coffee and input prices, coffee yields and households' decision to engage in coffee processing. We describe these subcomponents in some detail throughout the analysis. We measure good agricultural practices as a number ranging from zero to six according to how many of the following practices each household employs in the production of coffee: mulching, advanced soil fertility methods, use of synthetic inputs, regular pruning, light degree of weeding and planting of shade trees. For a more detailed description on how we define the variables of interest and other key variables refer to Appendix I.

Note that some of the participating coffee farmers obtain additional income from the cultivation of non-coffee crops and have income sources that are unrelated or not directly related to farming. Participation in the scheme may also have indirect effects on these income sources, which are not covered in our analysis. We accept this limit to our study with respect to the proposed scope of the thesis. While this may reduce our ability to draw conclusions on farmers overall welfare it will help to maintain a clear focus throughout our work.

5. Data and Empirical Strategy

5.1. Data Collection

The data was collected as part of a larger project gathering information on different agricultural practices and certifications for small-holder farmers in Uganda. The project was organized and funded by DIIS in collaboration with researches at Makarere University in Kampala, Uganda. The part of the survey relevant for our study is a household level survey of Robusta coffee farmers in the districts of Jinja and Kamuli in Eastern Uganda. 103 project farmers were surveyed with respect to a variety of different factors such as household and farm characteristics, production inputs and quality, quantity and price of the produced coffee. The same questionnaire was used on 101 control farmers from neighboring parishes where Ibero did not operate, but who had similar coffee growing conditions as farmers from the treatment area. The first household survey took place in 2006 when information on the 2005 farming season was gathered. The same farmers were resurveyed in 2009 with respect to the outcomes of the 2008 farming season. 11 farmers from the project group and 9 farmers from the control group did not participate in the resurvey. No farmers shifted from the control group to the project group from 2005 to 2008.

Survey participants from the treatment area were randomly sampled from a list of IMPROVE farmers provided by Ibero. Sampling covered eight out of the nine parishes where Ibero operated and was proportional to the number of farmers in each parish. Sampling in the control group was done through a two-stage random sampling method. In the five control parishes first 10 communities were randomly selected. Then approximately 10 households from each community were randomly sampled to participate in the survey.

5.2. Data Description

In this section we use descriptive statistics to examine whether there are any structural differences between control and project group farmers. Furthermore, we present the development in prices and share of coffee processing for both groups from 2005 to 2008.

Table 1 presents a comparison of mean values of household, farm and income characteristics for scheme participants and farmers from the control area in 2005. Household heads among the project farmers are more likely to be female and participant's average school education is slightly lower than in the control group. The difference of the latter is only borderline statistically significant. Project farms are significantly larger than control farms, but we find no statistically significant difference in the area designated to coffee production. Furthermore, project farmers maintain a larger number of coffee trees, implying that the density of productive coffee trees on coffee plots is larger among scheme members. In terms of farm practices we find that project farmers employ more good agricultural practices in coffee production and on average process a larger share of their coffee. The difference of coffee quantity per hectare between the two groups is statistically insignificant. In terms of the different income variables per hectare of coffee area we find no statistically significant differences between project and control farmers.

Note that differences in household characteristics cannot be attributed to impacts of the scheme. Age, sex and education of the household heads are determined before the households become eligible to participate in the contract farming scheme. We assume that any difference in household characteristics reflects non-random selection into the scheme. Similarly, it seems very likely that the difference in number of productive coffee trees can be attributed to selection effects rather than scheme impacts. Since coffee trees need about 5 years to mature, all productive coffee trees counted in the 2005 household survey must have been planted prior to the establishment of the scheme. Provided that project farmers could not anticipate their participation in the contract farming scheme, the number of productive trees should not change because of scheme eligibility. Unless project farmers acquired coffee plots from non-participating farmers after joining the scheme the difference in this category must be explained by non-random selection into the scheme. When comparing number of GAPs, share of coffee processed and income variables we cannot easily distinguish between scheme impacts and selection effects.

Table 1: Comparison of Mean Values for Project and Control Farmers

Variable	Project farmers	Control farmers	t-Stat.	P > |t|
Household characteristics				
Age of Household Head	49.50	49.01	0.23	0.82
Persons in Household	8.45	8.66	-0.47	0.64
Female Household Head (%)	0.17	0.08	2.06	0.04
Years of Education of Household Head	5.66	6.50	-1.66	0.10
Farm characteristics				
Farm Area (ha)	3.09	2.20	2.50	0.01
Coffee Area (ha)	0.77	0.68	1.23	0.22
Nr. of Productive Coffee Trees	366.59	246.69	2.77	0.01
Nr. of Income Sources	3.55	3.53	0.14	0.89
Processed Coffee (%)	0.83	0.71	2.34	0.02
Nr. of GAPs	2.41	1.87	3.25	0.00
Income				
Adj. Coffee Quantity (Kg/ha)[a]	515.50	602.40	-1.21	0.23
Coffee Revenue (UGX/ha)[b]	464.77	435.56	0.44	0.66
Coffee Costs (UGX/ha)[b]	47.00	52.13	-0.29	0.78
Coffee Profit (UGX/ha)[b]	417.25	382.21	0.59	0.55
Total Household Income (UGX/ha)[c]	565.43	616.05	-0.41	0.68

Source: Authors' calculations
Notes: All data refers to 2005. We use the Welch Two Sample t-test to account for possible unequal variance.
[a] Adjusted coffee quantity is standardized to fully dried coffee equivalents per hectare of operated coffee land
[b] in 1000 UGX per hectare of operated coffee land
[c] in 1000 UGX per hectare of Farm area

We have some indications of the previously hypothesized non-random selection into the scheme. Therefore, it is essential to account for the effects of non-random selection. Otherwise an unbiased estimation of the causal relationship between scheme participation and income is impossible.

The IMPROVE contract farming scheme is likely to impact price and the share of processed coffee. By looking into how these two key variables develop for control and project farmers from 2005 to 2008 we are able to get a descriptive insight in how project and control groups might differ over time. Table 2 gives information on share of processed coffee and coffee prices with respect to both treatment and control areas in 2005 and 2008. Note that Table 1 refers to household means, whereas Table 2 gives weighted prices and percentage shares according to the total amount of produced coffee in the respective areas. In general, both, the price and share of processed and unprocessed coffee converge in the project and control area from 2005 to 2008.

Table 2: Price and Quality of Coffee Sold

	2005		2008	
	Project	Control	Project	Control
Share of Processed Coffee Sold[a]	89.5%	77.7%	93.0%	82.2%
Sold to Ibero	74.0%	-	19.7%	-
Sold to Middlemen	26.0%	100%	80.3%	100%
Share of Unprocessed Coffee Sold[b]	10.5%	22.3%	7.0%	17.8%
Mean Price Processed Coffee (UGX/Kg)[c]	854.8	695.0	833.6	753.2
Bought by Ibero	893.4	-	897.3	-
Bought by Middlemen	688.4	695.0	757.5	806.3
Mean Price Unprocessed Coffee (UGX/Kg)	391.0	263.9	345.2	301.7

Source: Authors' calculations

[a] Quantity of coffee sold is standardized to fully dried coffee equivalents

[b] All unprocessed coffee is sold to middlemen. We exclude one observation of a project farmer who reports to have sold unprocessed coffee to Ibero

[c] All prices are in real 2005 UGX

From 2005 to 2008 the share of processed coffee increases in both project and control area. The share of processed coffee is highest in the project area in both years, but since the growth is larger in the control area (+5 percentage points) the difference declines slightly. Of all processed coffee sold in the project area the majority (74%) is sold to Ibero in 2005. The share goes down considerably in 2008, where Ibero only buys 20% of the processed coffee. Ibero started to buy later than usual in the 2008 coffee season which could, at least in part, explain this large decline.

Additionally, the premium Ibero pays for processed coffee declines from 2005 to 2008. We calculate the premium as the difference between the middlemen price and the Ibero price for processed coffee in the project area. In 2005 the premium amounts to 205 UGX whereas in 2008 the premium declines to 140 UGX. In absolute terms, the middlemen pay more for processed coffee in 2008, whereas Ibero's price remains stable. Nevertheless, in both years Ibero pays considerably higher prices for processed coffee than middlemen. One explanation for the premium decline could be that Ibero, in an attempt to gain market share, started operating with high premiums, which were gradually reduced in consecutive years. Another explanation could be found in the relative sensitivity to world market prices. The Ibero price premium may be less sensitive to higher world market prices than middlemen's prices due to a more certain supplier base. However, this must remain speculative.

All in all, the descriptive statistics give evidence for systematic differences between control and treatment group. At least some of these differences can be attributed to non-random selection into the scheme. With respect to the key variables coffee price and share of coffee processed we observe important differences between both groups. Project farmers process more coffee than control group farmers in both periods. Coffee prices are higher in the treatment area in both time periods. However, we observe that this difference decreases from 2005 to 2008. Project farmers sell much less coffee to Ibero in 2008 compared to 2005. This contributes to a drop in average coffee prices in the treatment area, while average coffee prices increase in the control area.

5.3. Methodological Restrictions

Firstly, we do not have random allocation to the scheme. Ideally, we would like to measure the effect of contract farming by running a randomized controlled trial where households are randomly assigned to participate in control and treatment groups. Random assignment of treatment would effectively solve the selection problem and it would be possible to estimate the causal effect in a simple OLS model (Angrist & Pischke, 2009). However, Ibero is a profit oriented firm that for obvious reasons does not select scheme participants at random. It is much more likely that community chairmen invite farmers with seemingly favorable characteristics to participate. Additionally, not all farmers that are offered a contract necessarily agree to participate which intensifies the problem of non-random participation. Since we must assume that selection also involves unobserved individual characteristics we cannot estimate the causal impact of the scheme by simple OLS regression with a large set of control variables.

Consider the naïve pooled OLS estimation shown in equation (1).

(1): $$y_i = \beta_0 + \delta D_i + \beta_1 X_i + \varepsilon_i$$

Where y_i is the income variable of interest (e.g. coffee profit), β_0 is the intercept, X_i is a set of control variables, D_i is a dummy on participation and ε_i is the error term.

(2): $$D_i = \alpha Z_i + u_i$$

$D_i = 1$ for scheme participants, $D_i = 0$ for non-participants

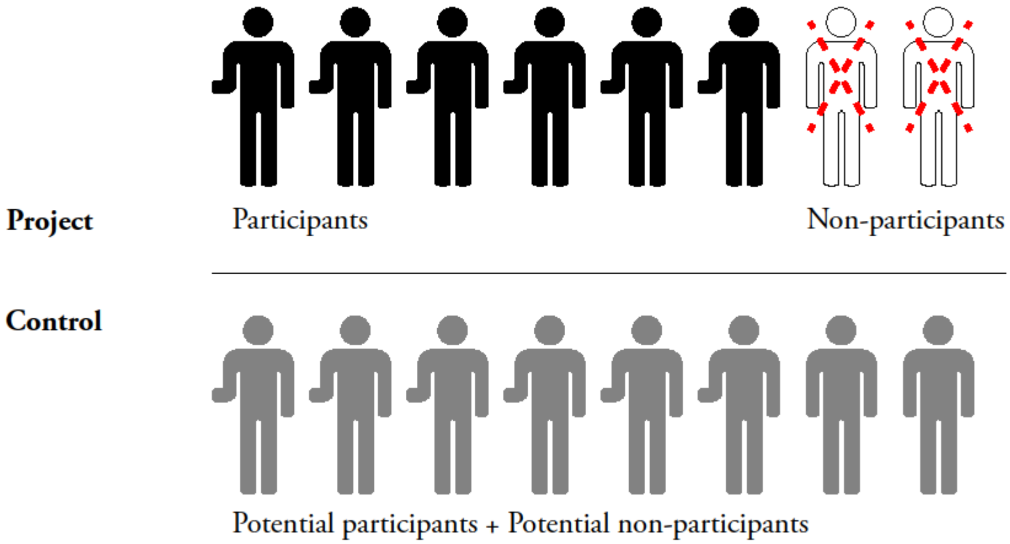
In the participation equation (2) D_i indicates individuals' participation status, Z_i is a vector of variables determining participation and u_i is the error term.

The coefficient on the participation variable δ, in equation (1), will be biased if there are omitted variables, which are both correlated with the outcome variable and the participation variable (Wooldridge, 2009). Assume for example that unobserved household characteristics such as farming ability, entrepreneurial spirit etc. increase the likelihood of participating in the scheme and increase household income. The error terms of equations (1) and (2) would be correlated, leading to a biased estimate of δ. In this case, a naïve estimation of equation (1) would lead to an overestimation of δ because we are not accounting for the possibility that scheme participants would have earned higher incomes in absence of the scheme.

In our specific case we do not know exactly what factors determine the likelihood of participation. It could also be the case that community chairmen systematically invite closest family members and friends to participate in the scheme. If they have below average farming abilities the bias goes in the other direction. We would underestimate the impact of the scheme. Either way, we cannot rely on a simple OLS estimation to define the causal impact of the scheme. We must identify a more suitable method to solve the problem at hand.

As pointed out in the literature review, empirical evaluation of contract farming often involves the identification of suitable instruments to replace the endogenous participation variable. IV approaches and Heckman selection therefore necessarily involve a first stage participation equation to determine the correlation between participation and other explanatory variables. Similarly, propensity score matching requires a participation equation to identify the underlying probability of participation. However, the particular structure of our data does not allow an unbiased estimation of the participation equation. Note that due to the way the survey participants were sampled we do not have any data on the non-treated in the treatment area, but only data on the treated in the treatment area. In contrast, survey respondents from the control area are either potential scheme participants or potential non-participants. If Ibero operated in the control area some surveyed farmers would participate in the scheme and others would not.

Figure 1: Sample Selection



Source: Authors' illustration

When applying a linear probability model or Probit/Logit model on only scheme participants from the treatment area and only non-participants from the control area, we have no variation in the error structure in both subsamples. More specifically, the error term of the participation equation is necessarily linearly dependent on the explanatory variables, which results in biased estimates. Therefore, any method involving a participation equation such as Heckman Correction, IV approach and propensity matching models are unfeasible in our case. Refer to Appendix II for a formal explanation.

We can avoid the participation equation by exploiting the panel data features of our data set. If selection into the scheme depends on time invariant characteristics we can relax the restrictive OLS assumption of an exogenous error term to a less restrictive equation. By adding a time dimension and rewriting the error term of equation (1) we get:

(3): $$\varepsilon_{i,t} = \varphi_i + \omega_{i,t} \text{ (for } t = 2005; 2008)$$

Where φ_i is the time invariant component of the error term, which is now allowed to be correlated with D_i. Panel data techniques have the attractable feature that they are robust to time invariant unobserved effects. Indeed, since we follow the same households over time we can show this by simply taking the differences between the two time periods on both sides of equation (1):

(4): $$y_{i,08} - y_{i,05} = \delta D_i + \beta\left(x_{i,08} - x_{i,05}\right) + (\varphi_i - \varphi_i) + (\omega_{i,08} - \omega_{i,05})$$

Provided that participation is uncorrelated with the time variant component of the error term, we can now estimate the scheme impact by OLS. Notice that in balanced panel data sets with two time periods Fixed Effects (FE), First Difference (FD) and DiD models give exactly the same estimates (Wooldridge, 2010) . However, we do not have a balanced data set so we are inclined to employ a DiD model to use the largest possible amount of available information. Since DiD models use the average development in the variables of interest, missing information from the 2008 household survey does not exclude households from the estimation. In contrast, FE and FD will exclude households with data from only one of the two time periods.

5.4. Difference in Difference Estimation for the IMPROVE Scheme

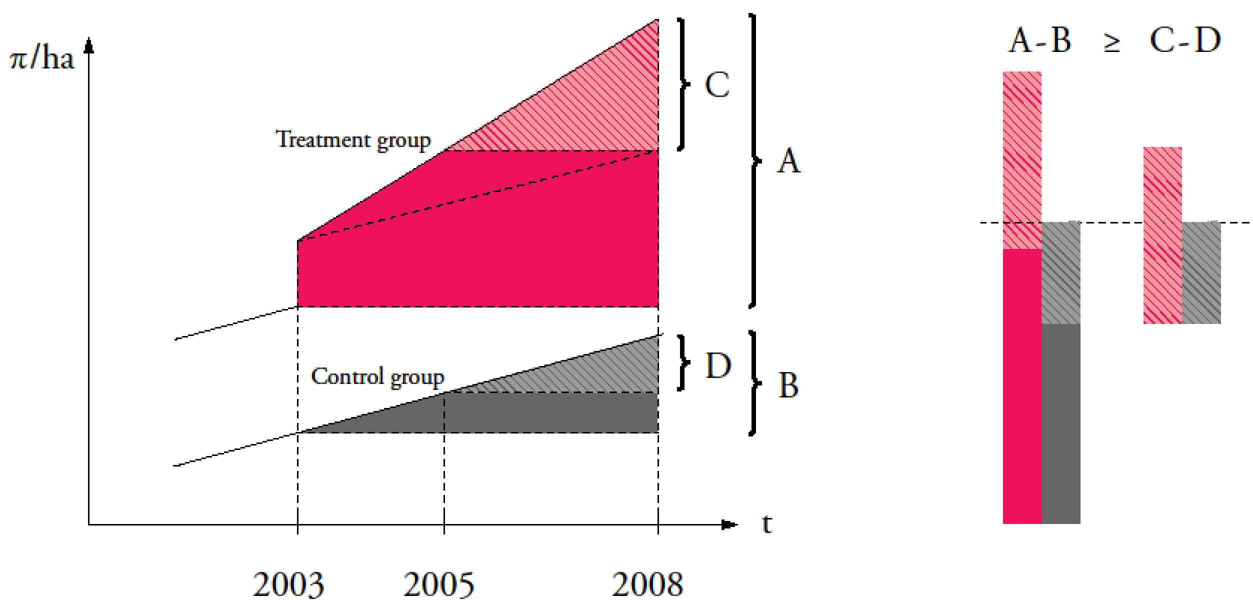
Typically a DiD estimation requires a baseline survey with information prior to the implementation of a treatment and a follow up survey at a later point in time. In our specific case pre-treatment data is not available. The first household survey refers to year 2005, two years after the implementation of the contract farming scheme. Therefore, we cannot capture the full impact of participation in the scheme, but only the impact of a continued participation between 2005 and 2008.

Missing pre-treatment data is especially problematic for the estimation of coffee profits. Everything else held constant, profit of the individual farmer will increase when the farmer starts to receive the Ibero premium for processed coffee in 2003. But we do not capture any level increase in profits prior to 2005 in our estimations. Furthermore, since we do not have data after 2008 we are not able capture any post-2008 effect of the scheme.

However, some of the scheme impacts will likely occur at some point between 2005 and 2008. Continued exposure to trainings may lead to a gradual adoption of GAPs by participating farmers. The effects of adopting GAPs on profits may be delayed so that we can measure the impact between 2005 and 2008. Participating farmers might decide to plant more coffee trees. Since it takes a couple of years before the first beans grow, the effect should start to kick in between 2005 and 2008. We are in principle able to capture any of these gradual adoptions and delayed effects by using a DiD estimation.

In Figure 2 we use a stylized DiD illustration to visualize how we could measure hypothetical scheme impacts on coffee profit over time. Assume that farmers who join the scheme in 2003 experience a jump in income due to the price premium for processed coffee. In subsequent years farmers' profit curve rotates upwards. The rotation of the profit curve can be explained by an increase in coffee farming productivity due to participation in scheme trainings and farm inspections. A gradual increase of the number of coffee trees on the farm should also contribute to an increase in the slope of the profit curve. Figure 2 portrays a scenario where both, the onetime increase and a rotation of the profit curve in subsequent years are realized. Assuming that the control group is completely unaffected by the treatment, the true impact of participation in the contract scheme is equivalent to $A - B$. Results are only consistent under the assumption that income in control and treatment group would have followed the same time trend, had it not been for the treatment.

Figure 2: Measurement of Scheme Impacts over Time



Source: Authors' illustration

Since no pre-treatment data is available we cannot measure the full impact of the scheme, but only the differences between the measurement periods. Instead of measuring $A - B$ we only observe $C - D$. Under the assumption that scheme impacts are non negative we have that $A - B \geq C - D$. We measure a minimum impact of scheme participation.

Theoretically, the scheme could also have a negative impact on profit over time. If Ibero promotes harmful farm practices participating farmers could experience a decline in productivity. In such a case the profit curve rotates downwards. Applying the same logic as before, we overestimate the impact of scheme participation. However, since neither Ibero nor the farmers have an incentive to promote or adapt harmful farm practices we do not regard this as a likely scenario.

5.5. Method

The DiD model captures effects on coffee profits and GAPs that occur between 2005 and 2008. We can estimate $C - D$ from a simple pooled OLS equation.

The unconditional baseline regression is based on:

(5): $$y_{i,t} = \beta_0 + \beta_1 T_t + \beta_2 D_i + \partial D_i \cdot T_t + \varepsilon_{i,t}$$

Where $y_{i,t}$ is the outcome variable of interest, β_0 is the intercept, T_t is a dummy which is one for 2008 and zero for 2005, D_i is the treatment dummy equal to one for project farms and zero for control farms, $D_i \cdot T_t$ is the DiD estimator or more specifically an interaction term between the time and treatment dummy and $\varepsilon_{i,t}$ is the error term. The coefficient ∂ on the interaction term is equivalent to $C - D$. It is an unbiased estimate of the treatment effect over time, provided that the DiD estimation indeed removes the effect of non-random participation in the scheme and that the underlying parallel trend assumption is not violated.

We check the robustness of our estimates from (5) by including control variables in the regression. This will sort out some of the pre-2005 differences between control and project farmers. Additionally, some components of the error term could be correlated with participation in the scheme and have time varying effects on the outcome variable. Therefore, inclusion of covariates could help to reduce this potential source of bias. We also include a set of dummies for each parish to control for any effects that are specific to the parishes and explain a change in the outcome variable of interest. The conditional model is depicted below.

(6): $$y_{i,t} = \beta_0 + \beta_1 T_t + \beta_2 D_i + \partial D_i \cdot T_t + \sigma X_{it} + \gamma P_i + \varepsilon_{i,t}$$

The added variable X_i is the set of control variables including fixed household level characteristics and time-varying variables. The added variable P_i is the set of parish dummies. The key in equation (6) is to include controls that are not affected by scheme participation status or are completely fixed before participation is determined. Inclusion of controls which themselves should be dependent variables such as average coffee price or share of coffee processing would prevent a causal interpretation of the participation effect (Angrist & Pischke, 2009).

Naturally, we would like to include a large number of controls to reduce the remaining threat of selection bias. In our first estimation of (6) we include a set of control variables that are clearly not affected by scheme participation, namely sex, age and years of education of household head. We also include number of persons in the household and area of the whole farm assuming that they are not affected by participation status.

In a second estimation of (6) we also include number of income sources and the amount of productive coffee trees, since they may be correlated to participation and explain coffee profits and number of GAPs. However, these variables could be affected by participation in the contract farming scheme in which case our estimates would be biased. The problem may not be too large since it takes five years for a coffee tree to fully mature. Coffee trees that are productive in 2005 were necessarily planted before farmers' learned their participation status. However, farmers could have planted coffee trees immediately after joining the scheme in 2003. These trees could mature right before the 2008 household survey so we may see some correlation between productive trees and participation in the scheme over time. A similar line of argumentation can be used for the number of income sources in each household. The number of income sources may be a factor that influences scheme participation and is correlated with the outcome variables of interest. However, participation in the scheme could cause a higher degree of specialization, which may decrease the number of income sources for the participating farmers. We cannot determine with certainty whether these variables should be included in the estimation. Therefore, we rely on both estimations of (6) for robustness of the unconditional model.

In all specifications we cluster standard errors at the parish level to account for serial- and intraclass correlation (Angrist & Pischke, 2009). As documented by Bertrand, Duflo, & Mullainathan (2004) serial correlation is a common problem in DiD estimations which is often not accounted for in academic literature using DiD estimations. Serial correlation is particularly troublesome when long time horizons and multiple time periods are studied. Since our data contains only two time periods we can regard serial correlation as a smaller problem in our estimations (Bertrand, Duflo, & Mullainathan, 2004). Nevertheless, we cluster standard errors at the parish level to limit the possibility of overestimating precision.

6. Results

6.1. Baseline Difference in Difference Estimation

Table 3 contains the baseline unconditional DiD estimations for coffee profit and key variables that should explain coffee profit. Between 2005 and 2008 the level of coffee profit per hectare of operated coffee land increases in treatment and control area. However, the increase is much larger in size for the control group than for the treatment group. The resulting DiD estimate is therefore negative, but statistically insignificant. The insignificant DiD estimate suggests that we have no evidence for a change in the slope of participating farmers' coffee profit curve. Contracted farmers do not earn higher coffee profits between 2005 and 2008 because of scheme participation.

Turning to other key variables we find that the average price for a kilogram of coffee sold in the control area increases considerably, whereas the average coffee price in the treatment area remains unchanged. The DiD estimate indicates that the control group experiences an increase in average coffee price of about 107 UGX relative to the treatment group over the observed period. The DiD estimate is statistically significant at the 10% level. Further, both control and treatment group farmers experience significant increases in the amount of coffee sold between the observed periods. Since the increase is comparable in magnitude the resulting DiD estimate is close to zero. We observe the same pattern in terms of coffee production costs per operated hectare of coffee land. Both groups see a sizeable increase but the difference in differences is far from statistical significance at any reasonable confidence level.

Table 3: Baseline DiD estimations

Variable	2005 Control (1)	2005 Project (2)	2008 Control (3)	2008 Project (4)	Difference [(2)-(1)] (5)	Difference [(4)-(3)] (6)	DID [(6)-(5)] (7)
Coffee Profit[a] (UGX/ha)	382.2	417.3	691.1	556.9	35.0 (56.9)	-134.2 (199.1)	-169.2 (167.2)
Average Price (UGX)	563.7	745.7	673.8	747.9	182.0*** (33.5)	74.2 (43.4)	-107.8* (50.9)
Coffee Quantity (Kg/ha)	828.4	589.53	1128.1	883.9	-238.9 (138.4)	-244.2 (159.8)	-5.31 (176.5)
Coffee Costs (UGX/ha)	52129	47000	80656	89629	-5129 (17488)	8973.4 (27946)	14102 (25893)

Robust standard errors in parenthesis (*** $p<0.01$, ** $p<0.05$, * $p<0.1$)

Source: Authors' calculations

Notes: Profit, Price and Costs are expressed in real 2005 UGX

[a] Profit in thousand UGX.

As pointed out before, the variables average coffee price and quantity of coffee sold are strongly influenced by farmers' decision to process coffee. Drying coffee increases the obtained prices, but reduces the kilogram amount of coffee that can be sold. The observed DiD estimate in coffee prices can, therefore, reflect a relative increase in control farmers' disposition to process coffee or be the result of an exogenous price increase in the control area relative to the treatment area. A similar reasoning must be applied in the interpretation of the DiD estimate on quantity of coffee sold. Assuming that both groups are exposed to the same farming conditions in terms of climate and infrastructure a relative change in the quantity of coffee sold can be explained by relative changes in coffee farming productivity or by a relative change in the disposition to process coffee in one of the two groups. Since this cannot be observed directly from comparing the absolute changes presented in Table 3, we standardize the weight unit of coffee so that coffee quantity is measured in units of fully dried coffee. Table 4 shows the results of the same pooled OLS regression on GAP's, adjusted coffee quantity and processing share.

Table 4: DiD estimates for GAP's, Adj. Quantity and Processing

	2005		2008		Difference		DID
	Control	Project	Control	Project	[(2)-(1)]	[(4)-(3)]	[(6)-(5)]
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Adj. Coffee Quantity[a] (Kg/ha)	602.4	515.5	889.6	771.6	-86.9 (99.7)	-118.0 (74.4)	-31.1 (99.3)
Processed Coffee (%)	70.7	82.6	69.1	87.8	11.9 (0.08)	18.7*** (0.06)	6.8 (0.04)
Nr. of GAPs	1.87	2.41	2.1	3.48	0.54*** (0.15)	1.38*** (0.16)	0.84*** (0.23)

Robust standard errors in parenthesis (*** p<0.01, **p<0.05, *p<0.1)

Source: Authors' calculations

[a] Quantity of coffee sold is standardized to fully dried coffee equivalents

Under the assumption that coffee farmers sell all of the harvested coffee in each season and do not store, consume or waste their produce, adjusted quantity of coffee sold is equivalent to coffee yield measured in units of fully processed coffee. We observe that there is no statistically significant difference in adjusted coffee quantity between control and treatment group in 2005. Adjusted coffee quantity increases in 2008 for both groups and, as in Table 3, the DiD estimate is close to zero and statistically insignificant. Since we now have accounted for the differences in the disposition to process coffee between the two groups we have a strong indication that on average the quantity of harvested coffee is not affected by participation in the contract farming scheme between 2005 and 2008. That is, we do not observe a productivity improvement in coffee yields for the participating farmers. Since the DiD estimate on the processing share is statistically insignificant and points in the direction of an increased processing share for the project farmers we rule out that processing is driving the price differences in Table 3.

The DiD estimate on GAPs in Table 4 suggests that participation in the scheme between 2005 and 2008 has a positive and statistically highly significant impact on the number of good agricultural practices employed. The spread in GAPs between the two groups is 0.54 in 2005 and increases by another 0.84 by 2008. The DiD estimate is statistically significant at the 1% level. The unconditional model explains as much as 20% of the variation in GAPs, although no control variables are incorporated (see Appendix IV).

6.2. Robustness – Conditional Difference in Difference Estimations

Proceeding to the two different conditional models formally introduced in section 5.5 we may be able to explain some of the 2005 differences between the groups and reduce the scope of potential parallel trend violators. In general the conditional estimations confirm the findings from the unconditional models in Table 3 and 4. In Table 5 we report the interaction term between time and treatment dummies as "DiD Estimator". The conditional DiD estimators of profit are smaller in size compared to the unconditional DiD estimator, but remain statistically insignificant. The DiD estimator of GAPs increases slightly in magnitude and remains statistically highly significant when household level characteristics (5) and more time-varying controls (6) are included in the regressions.

Table 5: Conditional DiD estimations for Profit and GAPs

VARIABLES	(1) Coffee Profit (UGX/ha)	(2) Coffee Profit (UGX/ha)	(3) Coffee Profit (UGX/ha)	(4) Nr. of GAPs	(5) Nr. of GAPs	(6) Nr. of GAPs
Treatment Dummy	35,042	36,313	-52,928	0.536***	0.731***	0.687***
	(56,871)	(94,740)	(72,717)	(0.145)	(0.130)	(0.135)
Post Dummy	308,937*	342,379	393,215*	0.228	0.247	0.202
	(155,151)	(194,581)	(213,698)	(0.186)	(0.194)	(0.196)
DiD Estimator	-169,226	-211,972	-237,668	0.843***	0.866***	0.913***
	(167,255)	(196,158)	(208,262)	(0.226)	(0.227)	(0.217)
Female Household Head		-106,016	-59,577		-0.0458	-0.0397
		(105,503)	(138,042)		(0.166)	(0.160)
Age of Household Head		-4,596	-4,480		-0.0123***	-0.0126**
		(3,774)	(3,909)		(0.00386)	(0.00501)
Years of Education		3,098	-8,698		0.0195	0.0125
		(7,549)	(12,151)		(0.0218)	(0.0234)
Persons in Household		9,600	-362.0		0.0934***	0.0837***
		(13,427)	(15,102)		(0.0263)	(0.0209)
Farm Area		1.471	-1.446		2.11e-06	-1.90e-07
		(1.645)	(1.638)		(2.55e-06)	(3.19e-06)
Nr. of Income Sources			77,932			0.0930
			(58,197)			(0.0554)
Log of Productive Trees			145,150*			0.127**
			(80,847)			(0.0443)
Parish level fixed effect	No	Yes	Yes	No	Yes	Yes
Observations	332	315	291	387	366	340
R-squared	0.026	0.136	0.183	0.202	0.319	0.333

Robust standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1)

Source: Authors' calculations

Notes: Profits are in real 2005 UGX

Going from the unconditional model to the most conditional model decreases the DiD estimator for profit by approximately 40%. Although statistically insignificant this reveals that the additional controls are important for our estimations of coffee profits. The adjusted R-squared also increases considerably going from the unconditional model to the conditional models. The DiD estimator of profit decreases slightly moving from (2) to (3), but as pointed out it remains statistically insignificant in both specification. The additional controls in (3) indeed have an effect on the DiD estimator. However, due to possible endogeneity of these controls the estimate of the latter specification may be biased. Taking a conservative stand, we are inclined to put more emphasize on the estimations presented in (2).

For GAPs the DiD estimator increases slightly in magnitude going from the unconditional to the more conditional models. As pointed out, the simple unconditional model explains as much 20% of the difference in variation in GAPs. This reveals that the contract farming scheme plays a crucial role in explaining the difference in number of GAPs employed. The DiD estimator increases somewhat in size, moving from (5) to (6). Again, the estimations of (6) should be interpreted with caution due to a possible endogeneity of the additional controls.

All in all, we do not have sufficient evidence to sustain hypothesis A. The DiD estimate on the development of coffee profit, which in principle can indicate a causal impact of continued scheme participation between 2005 and 2008, is statistically insignificant in all three specifications. Further, we find no causal impact of scheme participation on coffee farming productivity in terms of yield and production costs. If anything, the control group experiences a relative increase in average coffee prices compared to the project farmers. In contrast, we do find evidence in support of hypothesis B. The DiD estimate on the number of GAPs employed in coffee production is positive and statistically highly significant. Furthermore, the estimate is robust to different specifications. Continued scheme participation leads farmers to employ close to one extra GAP in addition to the observed 2005 difference to control farmers. Since this estimate can be understood as a minimum impact of scheme participation, we sustain the second hypothesis.

6.3. Robustness to Spillover Effects

Our assumption that the control farmers are in no way affected by the contract farming scheme may not hold. We use available farm level GPS data to map all of the farms from the 2005 survey (Appendix III). The geographic proximity of project and control farms lets us suspect that spillover of the treatment to the control group could occur. The control group farmers cannot participate in trainings and farm inspections offered by Ibero, so they should not have direct access to the information of innovative farm practices. Nevertheless, we cannot rule out that they eventually learn new practices from neighbor farmers who are eligible to participate in Ibero's trainings. Social learning literature provides many examples of know-how adaption in the context of agriculture in developing countries. Key to the analysis, in many studies, is the geographical distance between farms. The closer farms are to each other, the higher is the probability of technology adaption (Balineau, 2013). For other examples see also Weber (2012) on the diffusion of coffee pruning techniques in central Peru and Foster & Rosenzweig (1995) on spillovers in the choice of fertilizer in rural India.

In addition to the possibility of a know-how spillover we face the problem that the scheme may also affect the price level for coffee in the control area. Ibero's effort to buy coffee in the treatment area may crowd out some of the middlemen. If these middlemen in turn try to purchase more coffee in the control area, coffee prices are higher than they would have been in absence of the scheme. We have some evidence for this scenario from the results of the unconditional DiD estimation on coffee prices in Table 3, Section 6.1. The control group experiences a more favorable coffee price development than the project group which in principle could be explained by spillover effects.

The magnitude of the spillover effect may vary according to how close control farms are to project farms. Farmers living further away from the treatment area may be less contaminated by treatment effects since they have no direct neighbors who participate in the IMPROVE scheme. In this case we might improve our estimations by excluding control farms located close to the treatment area from our analysis. Farmers far from the treatment area may adopt fewer GAPs and be less exposed to a confounding price spillover. Following this logic we would expect that a reduction of the control group to consist of the far away farmers only, increases the DiD estimator on profits and GAPs. However, if spillover effects are lagged the effects of social learning and price diffusion will occur sooner for control farmers who live closer to the project area than control farmers who live further away. Know-how and coffee price transmission may reach further away control farms at some point between our measurement periods, whereas control farms closer to the project area may absorb a larger share of the spillover prior to 2005. If this is the case exclusion of control farms closer to the treatment area will not improve our estimates, in fact it may cause the opposite.

In Table 6 we compare estimates from the conditional estimations in Table 5 to estimates where we divide the control group according to distance to the project area. All farmers who live within 4 km to the closest project farm are excluded in columns (2) and (5) respectively. In columns (3) and (6) we show the regression results for a control group where all far away farmers are excluded. The DiD estimator on both coffee profit and number of GAPs becomes smaller when we use far away farmers as control group only. Farmers living further away from the treatment area adopt more good agricultural practices and have a more favorable development of coffee profits than control farmers close to the project area. The DiD estimator on coffee profits reduces to roughly -350 thousand UGX and the estimator on GAPs reduces to 0.62 and is now statistically significant only at the 5% level. In contrast, when we use farmers who are located close to the treatment area as control group the DiD estimator on GAPs increases to 1.121, statistically significant at the 1% level, and the estimator on profits increases to -76 thousand UGX.

Table 6: Robustness to Spillover

VARIABLES	(1) Coffee Profit (UGX/ha)	(2) Coffee Profit (UGX/ha)	(3) Coffee Profit (UGX/ha)	(4) Nr. of GAPs	(5) Nr. of GAPs	(6) Nr. of GAPs
Treatment Dummy	36,313	53,408	334,229***	0.731***	0.963***	0.220
	(94,740)	(162,886)	(65,423)	(0.130)	(0.150)	(0.177)
Post Dummy	342,379	484,539	203,264**	0.247	0.489**	0.00215
	(194,581)	(347,481)	(83,448)	(0.194)	(0.222)	(0.235)
DiD Estimator	-211,972	-349,004	-75,750	0.866***	0.622**	1.121***
	(196,158)	(344,300)	(97,441)	(0.227)	(0.247)	(0.266)
Female Household Head	-106,016	-108,561	-183,077	-0.0458	0.0281	-0.177
	(105,503)	(125,550)	(103,610)	(0.166)	(0.179)	(0.186)
Age of Household Head	-4,596	-7,113	-1,267	-0.0123***	-0.0109*	-0.0158***
	(3,774)	(5,513)	(2,202)	(0.00386)	(0.00511)	(0.00452)
Years of Education	3,098	-2,486	-1,692	0.0195	0.0230	0.0101
	(7,549)	(9,354)	(8,741)	(0.0218)	(0.0256)	(0.0262)
Persons in Household	9,600	9,577	15,282	0.0934***	0.0910**	0.0996**
	(13,427)	(15,114)	(14,008)	(0.0263)	(0.0318)	(0.0367)
Farm Area	1.471	1.467	1.561	2.11e-06	3.15e-06	2.04e-06
	(1.645)	(1.853)	(1.935)	(2.55e-06)	(2.98e-06)	(2.99e-06)
Controls by Distance to Treatment Area[a]	**All**	**> 4 km**	**≤ 4 km**	**All**	**> 4 km**	**≤ 4 km**
Parish level fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	315	242	236	366	277	276
R-squared	0.136	0.148	0.087	0.319	0.354	0.335

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: Authors' calculations
Notes: Profits are in real 2005 UGX
[a] Distance is measured in km on dirt road or road of better quality.

From Table 6 we see that our results vary when we divide the control group by distance. If this effect indeed goes back to lagged spillover of know-how and transmission of prices from project to control area we underestimate the impact of the IMPROVE scheme and the results in Tables 3, 4 and 5 are biased downwards. We understand Table 6 as an indication that spillover effects may bias our results but the evidence is not unambiguous. Other factors than the IMPROVE scheme correlated with distance to the project area could also explain the more favorable development in profit and number of GAPs for the far-away control farmers. A further study of potential confounders that are correlated to distance, however, lies outside the scope of our work.

6.4. Attrition

Out of the 204 farmers who participated in the 2006 survey 184 farmers were resurveyed in 2009. This amounts to a non-response rate of roughly 10% for the second household survey. In Table 7 we compare the farmers who were not resurveyed in 2009 with the averages of all farmers in 2005. The breakdown of mean values for key variables suggests that project farmers who did not participate in the second survey have below average coffee profits and employ slightly fewer GAPs. In contrast, non-respondents from the control area enjoy above average coffee profits and use more GAPs in coffee production than the average control farmer in 2005.

Table 7: Mean comparison of dropout farmers to average farmers in 2005

	Project		Control	
Year	Dropouts	All	Dropouts	All
N	11	103	9	101
Coffee Profit (UGX/ha)[a]	288.02	417.25	560.42	382.21
Adj. Coffee Quantity (Kg/ha)[b]	388.69	515.51	910.27	602.40
Average Price (UGX/Kg)	728.11	745.72	592.4	563.74
Coffee Costs (UGX/ha)[a]	50.36	47.00	181.36	52.13
Processed Coffee (%)	0.82	0.83	0.67	0.71
Nr. of GAPs	2.18	2.41	2.33	1.87

Source: Authors' calculations
Notes: All data refers to 2005
[a] in 1000 UGX
[b] Adjusted coffee quantity is standardized to fully dried coffee equivalents

This means that our DiD estimates could be biased upwards. A conservative guess would be that project farmers who do not respond to the second survey are still below average farmers in 2008. And non-respondents from the control area remain above average in 2008. The missing 2008 information for the project group leads to an increase in the 2008 group average, which biases the first difference. Accordingly, non-responses of above average control farmers bias the 2008 control group average downwards. Taking the difference in differences therefore leads to an upwards bias of the DiD estimator from attrition.

We take comfort in the fact that the number of dropouts is relatively small in both groups. Therefore, the effect on the respective group averages may not be particularly large. Nevertheless, it seems likely that, all else equal, attrition is a factor that could introduce a slight upwards bias to our DiD estimates on coffee profits and GAPs.

7. Discussion

Our results are robust across different specifications. We show that the estimations from the unconditional DiD models are robust moving to more conditional specifications. After adding covariates to the model the DiD estimator for coffee profits remains statistically insignificant. The estimator for GAPs increases slightly in size and remains statistically significant at the 1% level. In Appendix IV we include Fixed Effect estimations for profit, price, coffee quantity, production costs and GAPs. As pointed out in section 5.3 Fixed Effect estimations will exclude all unbalanced observations i.e. all observations where information for one of the time periods is missing. The FE estimations confirm the pattern of the conditional DiD models with respect to magnitude and significance of the profit and GAPs estimators. This is comforting in principle, since the FE estimations should be less vulnerable to attrition. On the other hand, measurement error may be larger in the FE estimation since we also exclude all other observations were information for one of the two household surveys is incomplete.

We cannot confidently reject hypothesis A. We reject that continued scheme participation between 2005 and 2008 increases farmers' coffee profits. However, coffee price and know-how spillover from the project group to the control group seem plausible. Therefore, our estimates might carry a downward bias. More importantly, project farmers could accrue revenue effects directly after joining the scheme in 2003. Since we cannot measure these potential impacts we cannot falsify hypothesis A. We find additional support for a potential underestimation of scheme impacts in the farmers own perception of the development of coffee profits since joining the scheme. In the 2005 household survey 70% of project farmers report that their income has increased since joining the scheme, whereas only 18% see a reduction in income from coffee production. When asked to report the single most important reason for the perceived income change more than 50% of farmers name Ibero as most influential factor for the positive development. Naturally, self perceived income changes should be interpreted with caution since farmers perception may be imperfect. Most importantly, farmers may not accurately consider how their income would have changed had they not participated in the scheme. Nevertheless, this additional insight adds to our presumption of a potential underestimation of the participation impact on profits. Refer to Appendix V for a breakdown of self perceived changes reported in both household surveys.

With respect to our second hypothesis we have strong evidence that participation in the scheme increases the number of good agricultural practices employed in coffee production. Somewhat surprisingly, the estimation of close to one extra GAP does not seem to coincide with an increase in coffee farming productivity. Hence, we neither find a reduction in coffee production costs nor an increase in the quantity of coffee output, controlling for the decision to process coffee. The most straightforward explanation is that those GAPs promoted by Ibero simply do not affect farmers' productivity. Alternatively, GAPs may be effective in increasing productivity in the long run but due to the small time horizon of our data we are not able to capture this effect. In constructing the GAPs variable in our analysis we weigh each agricultural practice equally. However, it is not evident that these practices are indeed equally important in explaining coffee farming productivity. We believe that a further analysis of how GAPs may affect productivity and ultimately profits would be highly rewarding. However, since this is not the main purpose of our paper this must remain subject to future investigation.

The largest threat to the validity of our results lies in our identifying assumption. We rely on the assumption that project and control farmers would have followed the same trend in absence of the IMPROVE contract farming scheme. Typically, academic literature goes to great lengths to show that a parallel trend is rightfully assumed. Since data on additional time periods or other study populations is not available to us we cannot comprehensively underlie this assumption for our case. The geographic proximity of project and control group ensures that climate conditions, the development of world market prices for coffee or other economic shocks should affect both groups in the same way. Additionally, we control for some observable household characteristics which reduces the scope of potential parallel trend violators. Still, there may remain time variant unobserved variables with uneven effects on both groups.

Bias may also arise from measurement error. In fact, we do not know whether project and control farmers have incentives to over report data. All farmers were informed that the surveys are completely independent from Ibero and no information on individual performances would be passed on to the company. Additionally, any misreporting of data would only lead to bias if the level of misreporting changed in the 2009 household survey. Therefore, we have reason to believe that measurement error may not at all be a source of bias in our estimations. Still, we cannot completely rule out that survey respondents knowingly misreport. It seems more likely that project farmers would have a more direct incentive to do so, since there is no indication that Ibero planned to recruit farmers from the control area. If project farmers' incentives to misreport increase over time this may lead to an upwards bias of participation effects.

Attrition can also introduce an upwards bias to our DiD estimates. Reducing our data to a balanced panel does not alter our results importantly. When comparing the estimates of the development of coffee profit from DiD and Fixed Effects estimations, we see that the coefficient on coffee profit indeed reduces slightly in size but remains statistically insignificant at the 10% level. The FE treatment estimator on GAPs is very similar to the conditional DiD estimator both in magnitude and statistical significance. Combining this information with the small number of dropouts we are confident that attrition is not a large threat to our interpretation of the profit impacts of continued scheme participation. Following the same line of argumentation, attrition is unlikely to invalidate our findings on the number of good agricultural practices employed in coffee production.

All in all, we show that our results are robust across different specifications. Due to attrition and potential measurement error, we believe that our estimates might carry an upwards bias. On the other hand, know-how and price spillover to the control group would lead to an underestimation of scheme impacts between 2005 and 2008. Since we have no further evidence on these effects we cannot convincingly determine the overall direction of the bias. However, it seems likely that an upwards bias would not be very large in size. We therefore conclude that a continued participation in the IMPROVE scheme does not have a significant impact on profits from coffee production. This means that we have no evidence in support of hypothesis A. However, we cannot falsify hypothesis A since pre-treatment data is missing. In contrast, we have strong evidence in support of hypothesis B. Participation in the IMPROVE scheme between 2005 and 2008 leads farmers to adopt more good agricultural practices. It is likely that the scheme works in the same direction prior to 2005, so the overall participation impact on GAPs is most probably larger than our estimate.

8. Conclusion

This thesis investigates the effects of participation in the IMPROVE contract farming scheme in Eastern Uganda. We examine the participation effect on farmers' coffee profits and their adoption of good agricultural practices. Our analysis shows that continued scheme membership between 2005 and 2008 does not increase farmers' income relative to non-scheme members. Missing pre-treatment data obstructs an assessment of participation effects prior to 2005. Since the IMPROVE scheme operates with a considerable coffee price premium, project farmers may have accrued significant profit increases before 2005. Consequentially, we cannot categorically reject the hypothesis that the IMPROVE scheme has a positive overall effect on farmers' coffee profits. However, we find strong evidence that scheme membership increases the extent to which farmers use good agricultural practices in coffee production. Our results show that project farmers adopt close to one additional GAP in the period from 2005 to 2008 relative to non-contracting coffee farmers. Again, this estimate can be interpreted as a minimum impact since we cannot measure GAP adoption prior to 2005.

In our data more GAPs do not seem to coincide with an increase in coffee farming productivity. Nevertheless, we are inclined to believe that at least some of the GAPs may have a positive effect on productivity in the long run. Additional shade trees, for instance, might not affect harvest until after 2008. The effects of pruning coffee trees also do not become apparent immediately. In the analysis on the adoption of coffee pruning techniques in Peru, Weber (2012) suggests that optimal pruning can lead to a long term yield improvement of up to 50%. Naturally, these findings might not translate perfectly to our Ugandan setting. Nevertheless, it provides some perspective on the benefits that could eventually arise from adopting close to one additional GAP.

Our findings carry valuable insights. Other contract farming schemes could draw on the IMPROVE example of how contracting can be combined with trainings in order to spread know-how and innovation. The distribution of best practice production methods has the potential to affect productivity and welfare of participating farmers. Naturally, such positive effects could also extend to non-participating farmers provided that they are indirectly affected by scheme trainings.

Nevertheless, generalizing our findings on coffee profits and GAPs due to scheme participation should be done conservatively. Each contract farming scheme is unique in its regulations and with respect to its geographic and socioeconomic background. Therefore, we cannot simply assume that the setup of comparable schemes in other regions or other populations would have similar effects. In fact, policy makers are well advised not to jump to hasty conclusions based on evidence from any single study on contract farming.

Comparing our findings to other empirical studies we do not confirm the large and positive revenue impacts suggested by Bellemare (2012), Warning & Key (2002) and Miyata, Minot, & Hu (2009) for example. On the other hand, we do not have reason to believe that participants in the IMPROVE contract farming scheme are exploited or substantially harmed as suggested in some of the case studies by Watts (1994). A causal interpretation of our findings relies on import identifying assumptions. Additionally, missing pre-treatment data leads to an assessment of scheme impacts over time rather than a full evaluation of the impacts from participating in the IMPROVE contract farming scheme. We recommend keeping these factors in mind when comparing our results to other studies. All the same, we are optimistic that our work on the IMPROVE contract farming scheme adds relevant insights to the pool of knowledge about the effects of contract farming in developing countries.

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Appendix I

Table 8: A 1 - Description of Key Variables

Name	Unit	Description
Coffee Profit per hectare of operated coffee land	UGX/ha	We define coffee profit as revenue from all coffee sales in the season prior to the household survey, minus all marketing and production costs. We express coffee profit per hectare of operated coffee area as to account for differences in land endowment between farmers. Profits are expressed in real 2005 UGX.
Average Coffee **Price**	UGX/kg	The average coffee price is calculated as coffee revenue divided by total quantity of coffee sold. All prices are expressed in real 2005 UGX prices.
Coffee Quantity Sold per hectare of operated coffee land	Kg/ha	This variable is the Kg sum of all coffee sales in the report period divided by coffee area.
Total **Coffee Costs** per hectare of operated coffee land	UGX/ha	Includes costs of inputs such as seedlings, fertilizer and equipment and transportation of coffee to the market or other selling points. Further we include costs of food and wages for hired labor used on coffee production. No data on family labor was collected in the household survey. Therefore, household labor costs are not included in the estimate. All costs are expressed in real 2005 UGX.
Adjusted Coffee Quantity per hectare of operated coffee land[4]	Kg/ha	Corresponds to Quantity of Coffee Sold where Kg units of all coffee sales are standardized to fully dried coffee equivalents. Conversion rates are: 1 Kg of fully dried coffee=1.5 Kg of partly dried coffee; 3.0 Kg of undried coffee. Under the assumption that farmer's sell all of their coffee produce this variable is coffee yield in fully dried unit equivalents.
Nr. of GAPs	Unit	Represents the number of good agricultural practices employed in coffee production. We define six GAPs: 1. Mulching: Is mulch applied on the coffee plots (Yes/No) 2. Advanced soil fertility methods: Is animal manure or compost applied on coffee plots (Yes/No) 3. Use of synthetic inputs: are synthetic soil fertilizers, herbicides or pesticides used on coffee plots (Yes/No) 4. Regular pruning: Two or more pruning sessions on coffee plants per season (Yes/No) 5. Light degree of weeding: are coffee plots weeded at least four times per season (Yes/No) 6. Shading: are any additional shade trees planted during the season (Yes/No)
Share of **Processed Coffee**	%	We define processed coffee as the amount coffee sold as fully dried out of all coffee sold.

[4] Conversion rates were obtained from key informant interviews of DIID employees with local farmers

Appendix II

Methodological restrictions

The derivation below is based on notes from Associate Professor Arne Henningsen, Department of Food and Resource Economics at Copenhagen University.

In this appendix we use the properties of the linear probability model (as a simplification of the probit model) to show why it is not possible to run a participation equation with treatment status as the dependent variable.

The LPM is given by:

(1) $$P_i = \beta_0 + \beta_1 X_i + u_i$$

where i denotes individuals, P is a dummy which is 1 for participation and 0 for non-participation, X is a vector of variables that explain participation and u is the error term.

Solving (1) for the error term:

(2) $$u_i = P_i - (\beta_0 + \beta_1 X_i)$$

We assume that the linear probability model (1) would also apply to households in the non-treatment area if they had the opportunity to participate. Since we have no non-participants from the treatment area $P = 1$ in the econometric estimation of (1) for all households in the treatment area. The error term in equation (1) in the treatment area is:

(3) $$u_i = 1 - (\beta_0 + \beta_1 X_i)$$

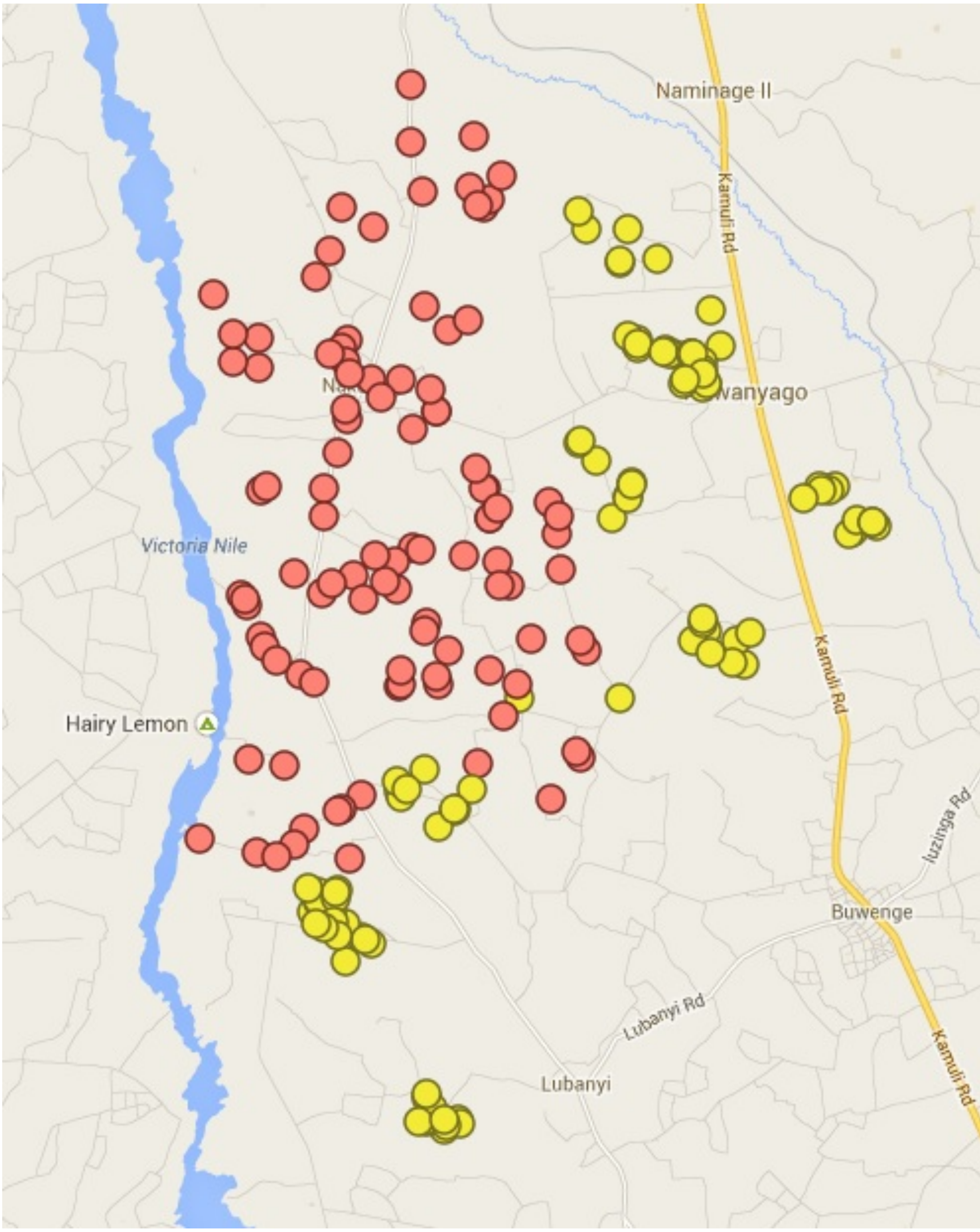
And the error term for all households in the control area is given by:

(4) $$u_i = 0 - (\beta_0 + \beta_1 X_i)$$

The error term in both subsamples is linearly dependent on the explanatory variables which results in biased estimates of β_1 because the zero conditional mean assumption is violated. Therefore, the estimation of a model for adoption of contract farming is unfeasible when comparing only participants from the treatment area to only non-participants from the control area.

Appendix III:

Figure 3: Map with all households in the 2006 household survey



Kamuli and Jinja district, Eastern Uganda
Source: Based on Google Maps, street map 2013

Project Farms
Control Farms

Appendix IV:

Robustness check

In this appendix we run Fixed Effects estimations, similar to specification (2) and (5) in Table 6 for coffee profit, coffee price, coffee quantity, coffee cost and for GAPs. We control for household level characteristics and include the set of parish dummies.

Table 9: Fixed Effects - Profit, Price, Quantity, Costs

VARIABLES	(1) Coffee Profit (UGX/ha)	(2) Average Price (UGX)	(3) Coffee Quantity (Kg/ha)	(4) Coffee Costs (UGX/ha)
Post Dummy	341,206**	121.5***	369.1*	40,408**
	(156,209)	(36.01)	(188.8)	(13,803)
FE Estimator	-268,450	-115.3**	-157.7	-10,808
	(190,099)	(47.09)	(199.5)	(24,320)
Female Household Head	-223,862	-172.6*	-284.4	-88,844**
	(225,779)	(85.31)	(242.2)	(35,257)
Age of Household Head	6,043	-0.940	4.424	2,104
	(14,334)	(3.221)	(15.73)	(2,829)
Years of Education	-8,593	-12.96	9.611	3,130
	(11,958)	(12.46)	(27.24)	(4,898)
Persons in Household	7,678	-6.717	6.575	-1,079
	(43,757)	(15.09)	(31.56)	(3,722)
Farm Area	2.507	0.000659	0.00315	0.712
	(3.217)	(0.00103)	(0.00430)	(0.507)
Parish level fixed effects	Yes	Yes	Yes	Yes
Observations	315	362	316	316
R-squared	0.075	0.067	0.098	0.084
Number of ID	177	203	177	177

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: Authors' calculations

Table 10: Fixed Effects - GAPs

VARIABLES	(1) Nr. of GAPS
Post Dummy	0.189
	(0.221)
Treatment Estimator	0.853***
	(0.229)
Female Household Head	0.0534
	(0.488)
Age of Household Head	0.0249
	(0.0258)
Years of Education	-0.0790
	(0.0465)
Persons in Household	0.118***
	(0.0393)
Area Whole Farm	1.95e-06
	(5.07e-06)
Parish level fixed effects	Yes
Observations	366
Number of ID	203
R-squared	0.290

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: Authors' calculations

Table A4.5: Additional information for Table 3 and Table 4

Dependent variable	Adj. R^2	Nr. of observations
Coffee Profit	0.026	332
Average Price	0.065	381
Coffee Quantity	0.045	333
Coffee Costs	0.016	335
Nr. of GAPs	0.20	387

Source: Authors' calculations

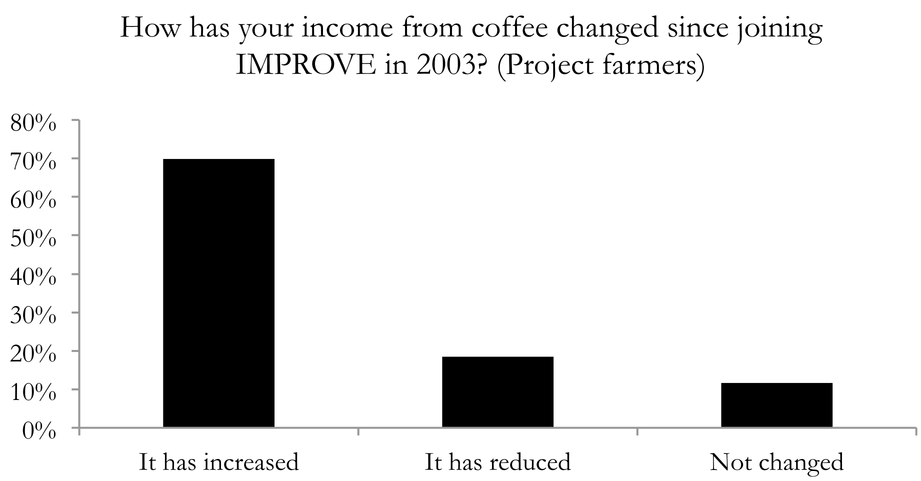
Appendix V:

Qualitative estimates

In the surveys carried out in 2006 and 2009 project and control farmers were asked a series of questions regarding the development and stability of their income. Some of these questions did not apply to control farmers in the 2006 survey.

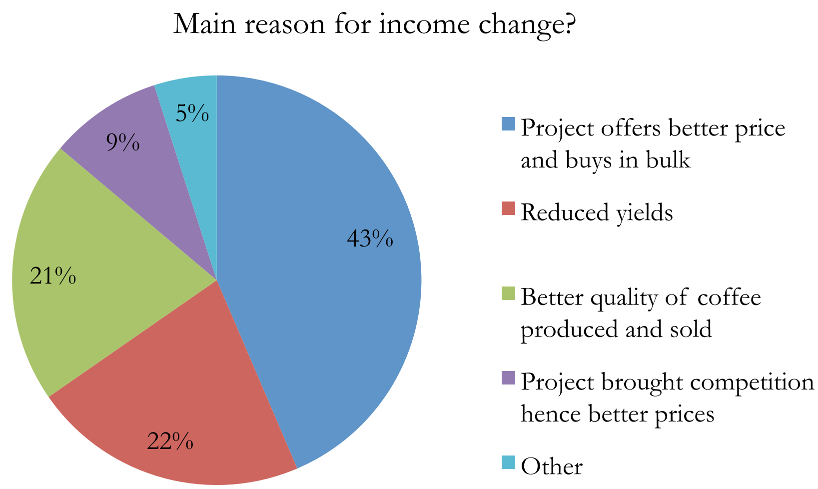
Project farmers in the 2006 survey:

Figure 4: A5.1 Income (project farmers)



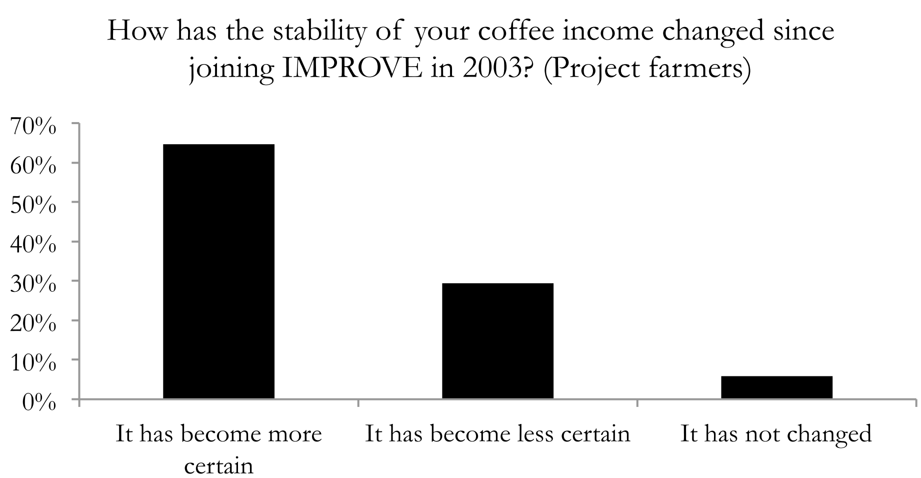
Source: IMPROVE Survey 2006 (N=103)

Figure 5: A5.2 Main reason for income change



Source: IMPROVE Survey 2006 (N=101)

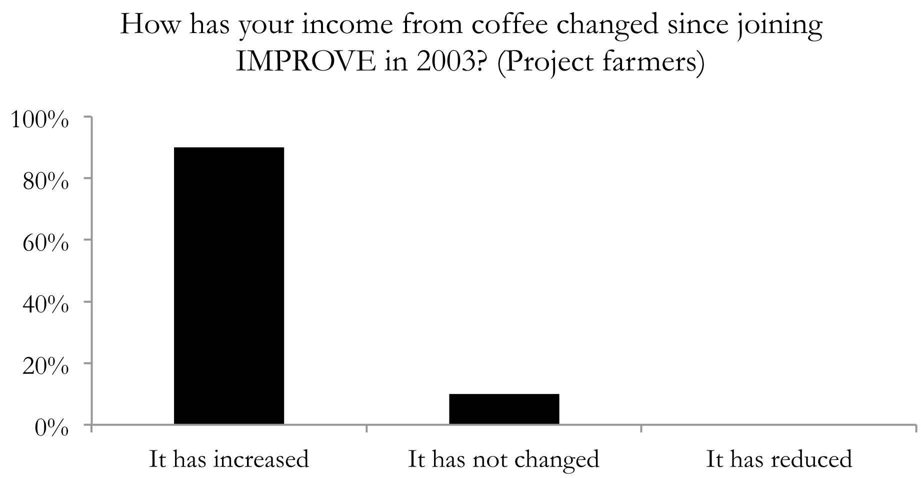
Figure 6: A5.3 Stability of income (2006 project farmers)



Source: IMPROVE Survey 2006 (N=102)

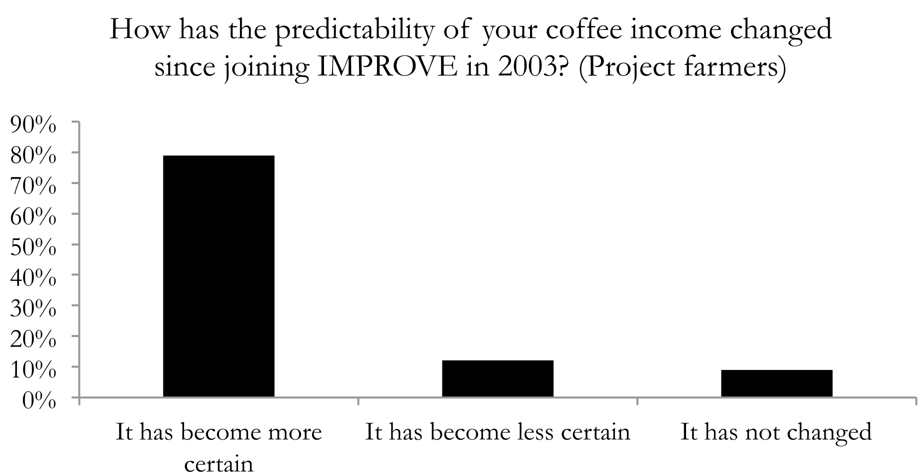
Project farmers in the 2009 survey:

Figure 7: A5.4 Income (2009 project farmers)



Source: IMPROVE Survey 2009 (N=90)

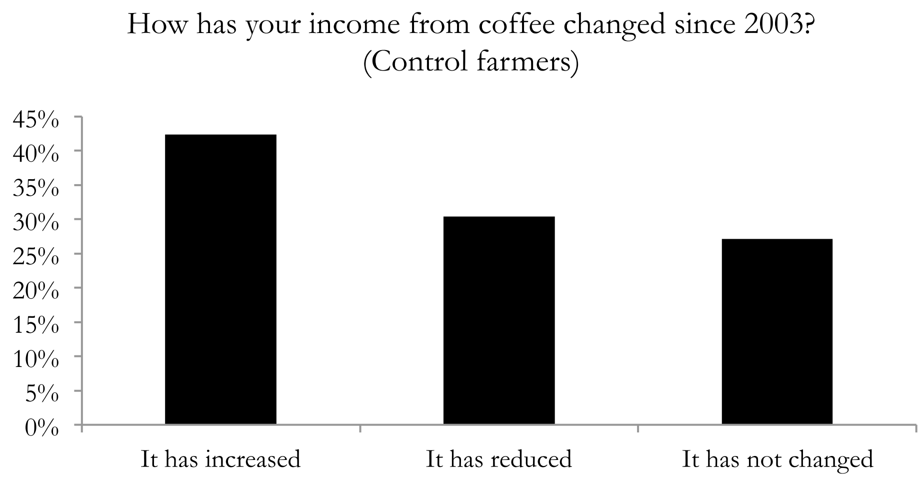
Figure 8: A5.5 Predictability of income (2009 project farmers)



Source: IMPROVE Survey 2009 (N=90)

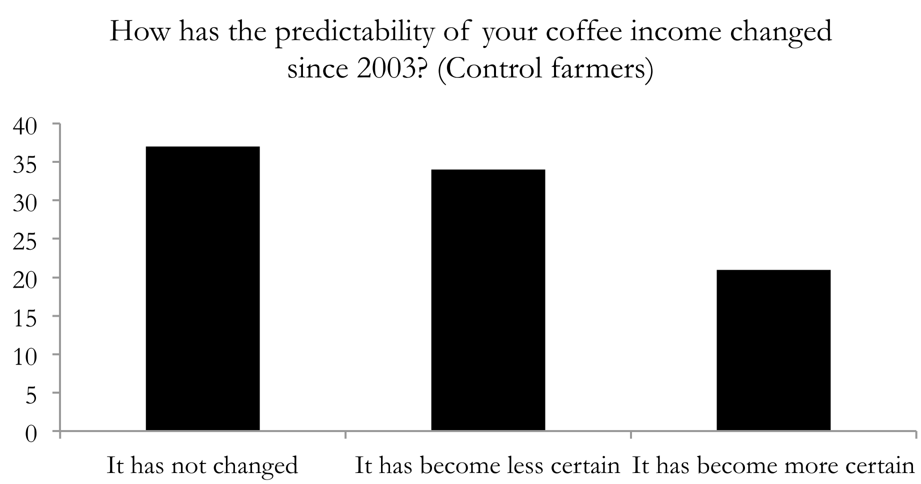
Control farmers in the 2009 survey:

Figure 9: A5.6 Income (2009 control farmers)



Source: IMPROVE Survey 2009 (N=92)

Figure 10: A5.7 Predictability of income (2009 control farmers)



Source: IMPROVE Survey 2009 (N=92)