Developing agricultural markets in sub-Saharan Africa: organic cocoa in rural Uganda

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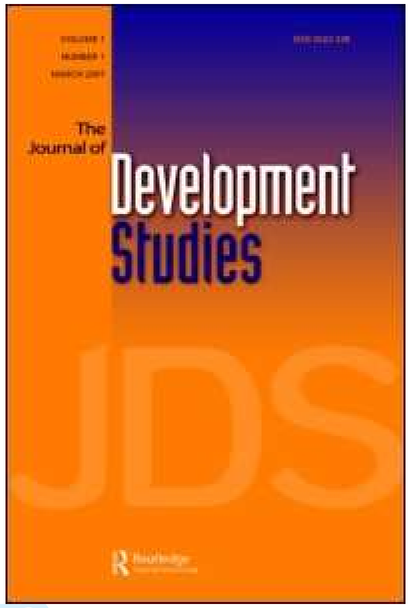

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This paper investigates the process of development in a traditional African export market, focussing on a contract farming scheme for organic cocoa in rural Uganda. Based on a repeated household survey, we measure the impact of the scheme on the income of participants and the economic mechanisms behind these effects. We find substantial benefits from the scheme, driven primarily by the establishment of credible incentives for farmers to adopt technologies which improve cocoa quality. There is also evidence of broader trends of market deepening and increased productivity, likely due to positive spillovers.

1. Introduction

The development of deep and efficient agricultural markets remains a key challenge across sub-Saharan Africa (hereafter, Africa) (World Bank, 2008). This paper examines the effects of a specific market intervention, namely an organic cocoa scheme introduced and operated by an exporter. The analysis is based on surveys of cocoa smallholders conducted in the remote Bundibugyo region of western Uganda in 2005 and 2009. The survey design incorporates 'treatment' and 'control' households, corresponding to smallholders from locations eligible and not eligible for organic certification. This enables us to evaluate the welfare impact of the scheme and the corresponding economic drivers. Although we do not have repeated observations on the same households, the repeat dimension of the design allows changes over time to be considered. These include market developments common to all farmers, as well as the persistence of scheme effects.

The contribution of this paper is threefold. First, we address areas where past research efforts have been thin. With some exceptions (e.g., Minten et al., 2009; Maertens and Swinnen, 2009), neither contract farming in liberalised markets nor organic farming have received much academic attention in Africa. This is despite that conversion to organic status often involves only small changes to farm processes but can enhance access to niche export markets and, hence, to substantial price benefits. Meanwhile, with respect to technology adoption, existing studies mainly focus on the diffusion of specific yield-enhancing as opposed to quality-improving technologies (Doss, 2006). These tend to treat technology adoption decisions as static and binary. Scant attention has been given to incremental improvements in farm practices which, as in the present case, can enhance product quality thereby improving access to export markets. Second, we provide a comprehensive and practical review of the empirical challenges involved in evaluating the welfare effects of "modern" contract farming schemes. Third, the paper is unique in going beyond aggregate effects; rather, we provide a simple and intuitive decomposition of the scheme's impacts.

The rest of the paper is structured as follows: Section 2 provides an overview of organic cocoa contract farming in Africa. Section 3 introduces the scheme. Section 4 discusses the economic mechanisms associated with the scheme and relevant empirical methods. Section 5 reports the results, including descriptive statistics from the surveys. Section 6 summaries the findings and reflects on general lessons from this case. A summary of data collection methods and further regression robustness checks are found in appendixes (available online).

2. Organic contract farming for cocoa in Africa

2.1. Overview

Smallholder contract farming is the main source of certified organic exports from Africa. Assessment of its impacts requires an understanding of the specific characteristics that differentiate it from other types of contract farming on the sub-continent, as well as from organic farming outside the continent. Glover's (1983, 1987) classic definition of contract farming refers to annual contracts typically specifying production calendars, minimum/maximum delivery volumes, inputs and services provided by the buyer, quality requirements and prices. Producers are obliged to sell all output of a designated crop to the buyer, who in turn pledges to purchase it all, subject to quality. This definition implicitly refers to large, state-sponsored schemes for bulk export products (e.g. tea, sugar, tobacco, groundnuts), usually where smallholders were resettled on land cleared for this specific purpose (Little and Watts, 1994). While some schemes of this form persist, more modern forms involve private companies making arrangements with established farmers either for non-traditional exports, such as fresh produce (c.f., Dolan and Humphrey, 2000; Gogoe, 2002; Maertens et al., 2007), or for traditional export crops with new 'sustainable' qualities.

While fresh produce schemes inherit several characteristics from traditional contract farming, newer schemes of the second type tend not to, reflecting the nature of both end and local markets.

In the case of traditional export crops, many local markets are now highly competitive. Thus, contractual monopsony is difficult to enforce and buyers are reluctant to supply inputs on credit. At the same time, end markets for traditional products with certified 'sustainable' qualities remain thin. Buyers adopt risk minimization strategies that emphasise strict quality control and price incentives, sometimes backed by farmer training to attain required quality dimensions. Hence, buyers normally only commit to provide smallholders with certification and to pay a premium for product of the required quality. In turn, smallholders promise to deliver produce and observe production rules.

Organic farming prohibits the use of synthetic inputs and promotes reliance on local inputs. Its distinctive feature is building soil fertility and controlling weeds, diseases and pests through rotations and using naturally occurring organisms and materials. Attaining organic certification mainly involves demonstrating non-use of synthetic inputs rather than following prescribed techniques. This is because organic standards emerged in developed countries with widespread use of synthetic inputs. In these cases, if synthetics are withdrawn, yields collapse unless alternative methods are adopted. Thus, there is no need to explicitly require specific crop management techniques. Use of synthetics, however, has been low and stagnant across much of Africa (Kelly et al., 2005). Most smallholder agriculture is 'organic by default' and certified farmers can, theoretically, earn price premiums without major changes to farming processes. Even so, because most smallholders are poor, certification costs typically have to be met by the buyer – although financial assistance has been available from the donor community.

The literature evaluating organic farming in the tropics is small (Bray et al., 2002; Damiani, 2002; Van der Vossen, 200; Lyngbaek et al., 2001; Bacon, 2005; Eyhorn, 2007; Bolwig et al., 2009).[i] Virtually all studies report results from Latin America where 'organic by default' agriculture is uncommon. Most are based on small samples and only two report comprehensive farm budget data (Eyhorn, 2007; Bolwig et al., 2009). Whilst two studies employ econometric techniques (Bacon, 2005; and Bolwig et al., 2009), the range of issues investigated is limited. For example, Bolwig et al. (2009) analyze survey data from an organic coffee contract farming scheme in Uganda. Controlling for a range of factors, they find positive net revenue effects from both participation in the scheme and, more modestly, from applying organic farming techniques. The authors do not formally investigate the economic mechanisms through which the observed revenue effect is produced. Moreover, to date no study has considered changes over time or wider spillovers that organic contract schemes may generate.

2.2. The international cocoa market

Global cocoa prices have risen since 2000 and remain resilient despite the 2008/09 financial crisis. The upward trend, although erratic, has become more consistent during the last three years. This relates to repeated global supply deficits and a growing consensus that production in Cote d'Ivoire (historically the leading supplier) faces long-term problems. By the 2008-09 season, prices had reached levels not seen since the mid-1980s. Recent years also have witnessed a growing emphasis on product quality and value-chain sustainability. Two of the three major global players have made explicit commitments to support sustainable production and this remains an area of expansion. In 2009, global cocoa output certified to 'sustainable' standards reached ca. 40,000 tons or 1.2% of world production. Organic cocoa production was even lower, at ca. 20,000 tons (Tropical Commodity Coalition, 2009). The price premium for organic cocoa ranges from US$100 to US$300 per ton (ICCO, 2006). However, due to its niche and 'luxury' status, demand for organic cocoa is discontinuous and production capacity exceeds demand. Hence, exporters must ensure that organic cocoa also has quality attributes that command premiums in the conventional market.

Various attributes are captured under the rubric 'cocoa quality', including moisture content at shipment, flavour, and acid contents. Critical to attaining them is to optimise ripeness by harvesting pods every 2-3 weeks during the season and opening them within 3-5 days of harvesting. Extracted beans should be fully fermented and then sun dried (ICCO, 2007), requiring a minimum mass of raw beans (ca. 50 kg.) and a moderate commitment of labour time.[ii] Scientific research (e.g., Hii et al., 2009) and market opinion indicate that the highest quality beans depend on careful and timely natural fermentation and drying by smallholders, rather than this being delayed and then carried out mechanically.

3. The Bundibugyo scheme

Cocoa production in Uganda dates from the 1950s but had minor importance until recently. Exports stood at 2,130 tons in 2001, reaching 5,386 tons in 2005 and 10,090 tons in 2009.[iii] Production today is by 15-18,000 smallholders, overwhelmingly in Bundibugyo District bordering the Democratic Republic of the Congo. The cocoa area lies at an altitude of 700-1050 m. with average rainfall of 1400 mm. per year and average temperatures of 28-35°c. The District is remote and has neither mains electricity nor any tarmac roads. To date, few of the plant health problems plaguing cocoa in other regions of Africa have been experienced.[iv]

In late 2005 there were four companies buying cocoa in Bundibugyo, one of which operated a contract farming scheme. By early 2009 there were six, three of which operated such schemes. All schemes were either certified organic or technically 'in conversion'. The scheme operated by Esco (U) Ltd is the oldest. Farmer (re-)registration and certification was undertaken with support from a Sida project and the first exports occurred in 2002.[v] In addition to cocoa, vanilla production is certified although Esco has made little attempt to encourage it since international prices collapsed in 2003-04.

Esco has used the characteristics of different cocoa-growing areas to determine eligibility to join their scheme. At the outset, the company selected a number of parishes for inclusion based on an informal assessment of numbers of cocoa farmers and their specialization in cocoa production.[vi] All households in these parishes were permitted to register as scheme members. Given zero entry costs, the vast majority of households in selected parishes initially did so. With respect to organic certification of these households, an 'internal control system' (ICS) has been used. This entails farm inspections by company field officers trained in organic farming methods. Inspections are also used to provide technical advice. In 2005, there were only four field officers and formal training was confined to 30-40 'contact farmers' with demonstration plots in each village. Training has emphasized farm practices – partly but not exclusively organic – that should enhance yields. Annual third party certification involves reviewing records of cocoa purchases from individual farmers against ICS documentation on potential output, as well as visits to selected farms.

By 2005, the scheme comprised 1,721 farmers in two adjacent parishes (Ngamba and Burondo). Farmers were required by contract to follow organic standards and sell to Esco, who in turn provided some subsidized inputs (including cocoa seedlings), but only to 'contact farmers' and only in 2001-02.[vii] Meanwhile, Esco buying posts in the scheme area only accepted cocoa that had been fully fermented and properly dried. In 2005 Esco offered scheme members a fermented cocoa price (at Ush 1,900/kg = US$1.07), 30% higher than that for fermented 'conventional' cocoa outside the area, and 100% higher than for unfermented cocoa. Nonetheless, in 2005 Esco procured only 269 tons of organic cocoa. Farmers complained that sometimes they could not sell to Esco as its buying posts ran short of cash.

Between 2005 and 2009 the scheme underwent important changes. Despite support from Sida elapsing, it was physically extended and the number of certified farmers grew to over 5,000.[viii] Farmers not selling to the scheme were expelled – growers' lists examined for 16 villages in the original scheme area showed that around 26% of farmers certified in 2001-02 were struck off between 2005 and 2009.[ix] Certification now covered US as well as EU standards. Instead of being managed from a rented store in Burondo parish, its base was a well-equipped town office. The field staff now numbered 30-35, training had been stepped up – principally in tree management and crop processing – and Esco had initiated a savings society.[x]

Esco now bought cocoa on a continuous basis. Backed by a new bye-law, farmers were strongly encouraged to harvest cocoa every two weeks. Buying posts always had cash and now employed technical instruments to read moisture levels, rather than relying on subjective assessment. An 'organic premium' was paid only to certified farmers presenting cocoa with <8% moisture content, with a further premium for moisture <7.5%. Discounts at roughly Ush 200 intervals were applied for each degree of moisture content above 8%, until cocoa was determined (at 13%) to be unacceptable. The same system (except for the 'organic premium') was applied by Esco to conventional farmers outside the scheme and appears to have been copied by other buyers. Importantly, conventional farmers have been able to sell to Esco on a spot basis only, and therefore have not benefited from any contractual certainty. In 2008-09 the organic premium price was around 16-18% higher than what a 'conventional' farmer would have received for the same crop. All prices had moved upwards since 2005, with good quality organic commanding Ush 3,300 ($1.86)/kg. As a result of these changes, Esco's organic purchases dramatically increased to 2,593 tons in 2008.

4. Analytic framework

4.1. Economic aspects

Esco's Bundibugyo scheme is structured so as to provide incentives to scheme members to process their cocoa crop to a high grade, following recommended organic techniques, and sell the processed crop to Esco. In turn, Esco provides a guaranteed price premium, a commitment to buy all high grade cocoa offered by farmers, and transparent measures of quality. Nevertheless, scheme members freely choose whether to process cocoa and how much to sell to Esco. Thus, the impact of participation in the scheme is of interest as opposed to formal membership.

These characteristics suggest four mechanisms through which scheme participation may affect household welfare. First, certified farmers may choose to sell properly processed cocoa to Esco, rather than to another intermediary, in order to benefit from an organic price premium. For farmers that already produced all cocoa to a high grade, farm practices would be largely unchanged. Second, for other farmers the scheme may induce greater specialisation in production of high grade cocoa, entailing a shift away from production of a more diverse range of crops or different standards of cocoa. If farmers previously sold only raw beans, they may also adopt cocoa processing technologies for the first time. Third, organic certified farmers may adopt recommended organic practices. As noted in Section 2, however, these specific practices are not required for ongoing certification.

Finally, various indirect benefits may accrue to scheme participants and other cocoa farmers. Perceptions of participation risk and coordination costs may fall as the benefits of adopting different technologies are revealed. Such information and social network externalities have been shown to be important drivers of change in smallholder agriculture (Conley and Udry, 2001; Besley and Case, 1993). Additionally, intensification of local economic activity, initially driven by growth in one product market, can generate productivity gains in other markets through *inter alia* private investment in local infrastructure and enhanced access to credit (Ravallion, 2003). Previous studies of commercialization and technology adoption highlight the potential for such household and regional spillovers from crop-specific interventions. Govereh and Jayne's (2003) study of cotton commercialization in Zimbabwe, for example, suggests that higher incomes and improved access to inputs from participation in cash crop schemes can relax constraints to other household production activities (also von Braun and Kennedy, 1994).

However, in the presence of multiple market failures, a superficial reading of incentives often provides a poor guide to behaviour. Farmers may resist full commercialization or improved technologies when these jeopardize a minimum level of food crop production or expose the household to large income fluctuations (Ellis, 1993). In contrast to selling raw cocoa, the decision to process beans to a high grade is not risk-free. Farmers must defer sale for at least two weeks, during which prices can change or buyers disappear from the market. Smallholders also may reject increased specialization in cocoa production and/or processing if, for example, farmgate cocoa prices or local food markets are unreliable. Indeed, producers often respond to price volatility by reserving a share of land for food production, despite higher expected returns from other crops (Fafchamps, 2003; Byerlee et al., 2006).

The structure of intermediation between smallholders and the world market is also of direct importance to quality-related technology decisions. Longitudinal research among cocoa producers in Ghana has identified sources of increased productivity in higher levels of effective competition among local buying companies in conjunction with a specific institutional complex (Teal et al., 2006). In contrast, the experience of other cocoa-producing countries (e.g., Cameroon and Cote d'Ivoire) suggests that unregulated competition in the context of a fully liberalized cocoa market may correspond to a decline in cocoa quality and, thus, to discounting against the world price (Losch, 2002). In the absence of institutions that can credibly regulate farmgate quality and/or entry into the intermediary market, a decline in quality can arise from free-riding on the public goods nature of implicit or explicit quality standards (cf., Poulton et al., 2004).

Bringing these ideas together, two specific questions merit attention. First is whether the Esco scheme generates welfare benefits for those who participate in it. In theory, because individuals are free *not* to participate, and Esco does not have a monopsony position, we do not expect negative effects. Nevertheless, the scheme may be ineffective if its price incentives are negligible or if it fails to encourage farmers to (further) adopt quality-enhancing cocoa processing technologies and/or organic practices. Thus, the second empirical question focuses on the economic mechanisms at play – namely, whether the scheme has induced the adoption of post-harvest processing and/or organic practices.

4.2. Empirical strategy

With respect to the empirical challenges of the analysis, three potential sources of bias must be addressed. The first is endogenous programme placement. As discussed in Section 3.1, location-based characteristics were used by Esco managers to decide which households were eligible; thus, placement of the scheme was non-random. Second, there is self-selection bias, which refers to the choice to participate in the scheme. Formal registration as a scheme member was cost-free and does not guarantee actual participation, which refers to selling organic grade cocoa to Esco. It may be the case that more entrepreneurial or risk-taking households choose to participate, and that these are better-off regardless. Finally, there is geographic heterogeneity due to, *inter alia*, differences in soil productivity or access to infrastructure, which may be correlated with the other two sources of bias.

As has been established in the impact evaluation literature (e.g., Angrist and Krueger, 1999), one way to address these problems is to fully and directly control for all sources of bias. Specifically, consider a general formula for the impact of actual scheme participation ("esco") on a welfare outcome (y) for household i in location j at time t:

(1) $$y_{ijt} = \alpha_i + \mu_j + \theta t + \delta \text{esco}_{it} + \boldsymbol{X}'_{it}\beta + \varepsilon_{it}$$

where $\boldsymbol{X}$ is a vector of time-varying household characteristics, representing elements of a household production function and ε is a white noise error term. Causal identification through 'selection on observables' requires that all elements of the equation are observed without error, including fixed differences in motivation or productivity across individual households (α_i) and geographic fixed effects (μ_j).

The available data does not permit direct estimation of equation (1) as the household-specific effects are not observed. Similar difficulties have been confronted in the extensive micro-finance evaluation literature, from which further methodological inspiration is taken (e.g., Tedeschi, 2008 and references therein). One strategy is to find proxy variable(s) for the household-specific effects, and include them alongside the other elements of equation (1) in a simple OLS regression. Such an approach is found in Coleman's (1999) quasi-experimental study. To evaluate the impact of a group lending programme in Thailand, he constructs a variable for scheme membership potential which includes both current borrowers as well as non-borrowers located in control villages who wish to join the programme. This variable is included to control for selection bias, allowing a distinct variable for participation to capture scheme impact.

A similar approach can be implemented here. *Actual participation* in the scheme is measured as the volume of organic grade cocoa sold to Esco as a share of the total volume of cocoa sold.[xi] This is zero for all non-eligible households; and for eligible households ranges from zero (eight cases) to one (24 cases) with a mean of 0.35 and standard deviation of 0.42. *Participation potential* is measured as the share of fully processed cocoa sold to all buyers out of all cocoa sales by the household, constructed in the same way for eligible and non-eligible households. This reflects a revealed preference to engage in cocoa processing regardless of scheme eligibility. Thus, using previous notation and denoting the proxy for household-specific effects by "potential", a feasible estimating equation is:

(2) $$y_{ijt} = \alpha_0 + \mu_j + \theta t + \delta \text{esco}_{it} + \pi \text{potential}_{it} + \boldsymbol{X}'_{it}\beta + \varepsilon_{it}$$

Despite the viability of this approach in theory, in practice it is not fail-safe. First, mismeasurement of either the actual or potential participation variables would generate attenuation bias. This is relevant in this study as both these variables depend on recall. Second, correlation between the same variables, as well as with the geographical fixed effects, may generate multicollinearity, leading to imprecise estimates for the main coefficient of interest (δ). Third, there is no reliable way to verify whether the proxy for household-specific effects is adequate. It also may be confounded with scheme impacts. For instance, the decision to process cocoa by a non-member household could reflect positive spillover effects from the Esco scheme, rather than innate household characteristics. Thus, by including the proxy on the RHS of equation (2) we run the risk of over-controlling and, thus, underestimating δ (see Wooldridge, 2005). Similarly, inclusion of a full set of location fixed effects may over-control for spillovers arising from participation within each location (see Morduch, 1998).

An alternative estimation strategy is to find a valid instrument for scheme participation, enabling us to omit the proxy for household-specific effects. In a wide range of studies, exogenous aspects of scheme eligibility or availability have been used in this way. For example, Pitt and Khandker (1998) construct instruments for participation in microfinance programmes in Bangladesh from asset ownership criteria employed to exclude participants. Such an approach is also possible here. By virtue of the scheme's design, geographic rather than household-specific factors were used to determine scheme eligibility. As the latter is conditionally independent of the former, it represents a potential instrument for scheme participation. However, this only deals with selection bias, meaning that location characteristics must be included to address placement bias and geographic heterogeneity. This is problematic as all households in each chosen location were eligible to join the scheme. Consequently, a dummy variable for scheme eligibility, the potential instrument, is collinear with the vector of location-specific fixed effects, regardless of whether we specify geographic fixed effects at the sub-county, parish- or village-levels.

To address these concerns, the instrumental variable (IV) procedure can be adapted by defining a set of (continuous) proxies to substitute for location fixed effects. Assuming these are not collinear with the eligibility instrument, an IV approach is feasible. This method of using a vector of location characteristics in place of fixed effects is not new and also has been employed in the micro-finance literature (e.g., Pitt and Khandker, 1998). It also carries some advantages relative to the previous method – the adequacy of the location proxies can be directly tested; there is a lower risk of over-controlling; there are potential efficiency gains; and the IV approach can address any bias from mismeasurement of the participation variable. More formally, the second approach looks like:

$$\text{esco}_{ijt} = \alpha_3 + \vartheta t + \gamma \text{eligible}_{jt} + \boldsymbol{X}'_{it}\varphi + \boldsymbol{Z}'_{it}\omega + \upsilon_{it} \quad (3)$$

$$y_{ijt} = \alpha_4 + \theta t + \delta \text{esco}_{ijt} + \boldsymbol{X}'_{it}\beta + \boldsymbol{Z}'_{it}\lambda + \varepsilon_{it} \quad (4)$$

where $\boldsymbol{Z}$ is a vector of proxies for the omitted location fixed effects. Equation (3) is the first-stage participation prediction equation, in which "eligible" is a dummy variable for scheme eligibility and varies only at the location level. Under the assumption $\text{E}[\upsilon_{it}\varepsilon_{it}] = 0$, unbiased estimates of equation (4) can be acquired by replacing the "esco" variable by its fitted values from the first stage, (as per two stage least squares). Practical aspects of this approach are described below.

5. Results

5.1. Descriptive statistics

Following the discussion of Sections 3 and 4, the distinction between eligible and non-eligible households is not the principal focus of analysis. More pertinent are differences in the extent of households' actual participation in the scheme, as well as their potential participation, used to proxy for unobserved household effects. Before presenting econometric results, it is helpful to investigate whether observed household and location characteristics vary with these two measures. As they are continuous, however, Table 1 presents descriptive statistics using a set of 2×2 categories created by splitting the potential participation variable (share of cocoa processed) at is mean and the measure of actual participation at 50%. In each case a value of one is given to observations above the split point, and zero otherwise.

With respect to the rows of Table 1, all monetary values are expressed in 2005 local prices, using a common deflator based on official information.[xii] Net revenues are calculated as gross sales minus variable costs of equipment and labour, including exchange labour.[xiii] All estimates are calculated using sample weights. Sample design weights were calculated directly from the sampling frame for each village. However, to ensure that no specific time period or group of eligible/non-eligible households is over-represented, design weights were subsequently recalibrated to ensure an equal balance across the eligible/non-eligible households and over time periods. If nothing else, this is important due to the smaller absolute sample in the 2005 round. The final two columns of the table report whether there are significant differences in the conditional expectation of the row variable according to actual ("A") or potential participation categories ("P"), controlling for any common changes over time. This derives from separate regressions of each row variable against dummy variables representing the actual/potential participation categories of the table, plus a time period dummy equal to one in 2009 and zero otherwise. The column stars denote the significance of individual Wald tests that the actual/potential participation dummy variables are equal to zero.

[INSERT TABLE 1 ABOUT HERE]

The results indicate that the econometric concerns raised in Section 4.2 are pertinent. Four points merit comment. First, scheme eligibility guarantees neither actual nor potential participation in the scheme. For instance, around 11.2% of households who process a below-average level of cocoa (low potential) and sell less than 50% of their cocoa to Esco as organic are scheme eligible. Second, there are systematic differences between households according to their actual *and* potential participation rates. For example, high potential participants appear to own more cocoa trees and have larger farms than those who process a below average share of their cocoa. They also appear to have higher net cocoa revenues when compared to households within the same category of actual participation. Consequently, to avoid biased estimates, any assessment of scheme impacts must control for differences in (pre-existing) household characteristics. Third, location characteristics are important. Controlling for potential participation, the most active actual participants appear to live further away from Bundibugyo town. Finally, not shown in the table, one also notes distinct changes over time across all households, eligible and non-eligible. For example, use of hired labour, processing intensity and total number of sales have all increased substantially, pointing to possible dynamic processes at the regional level. We discuss these briefly in subsection 5.4.

5.2. Income effects

The results of Table 1 motivate the use of multivariate techniques to separate out the complex determinants of welfare differences between households. Thus, Table 2 presents results from a range of models for the welfare impact of the Esco scheme, focussing on the first of the two empirical strategies presented in Section 4.2. In all cases the dependent variable is the logarithm of net cocoa income. The null hypothesis of interest is that actual participation in the scheme has no causal effect on net cocoa income on average. Columns (I) to (V) correspond to different versions of equation (2). Column (I) represents a naïve model, which excludes household and location fixed effects; column (II) adds the potential participation measures to proxy for unobserved household effects; and columns (III) to (V) include sub-county, parish and village fixed effects respectively. The core explanatory variables include standard elements of a cocoa production function (e.g., Teal et al., 2006), as well as the number of times the household makes cocoa sales (of all types to all buyers). The latter is included to control for changes in harvesting frequency over time (see above); it may also reflect a range of other household characteristics, such as households' commitment to cocoa farming, market orientation and/or commercial acumen. Due to space limitations, however, results for this vector of control variables are not discussed at length.[xiv]

A principal result is that the point estimate for the coefficient on actual participation (δ) is positive but declines and loses significance as we include a more complete set of controls. The introduction of the proxy for household-specific effects (column II) has a negligible effect vis-à-vis the naïve model. Nonetheless, location fixed effects are highly significant in all relevant specifications. For instance, when defined and included at the parish or village levels (columns IV and V), they yield an estimate for δ that is insignificant and is around half the size of the naïve estimate. This supports the need to control for endogenous program placement and suggests that once location effects are included, the null hypothesis remains intact.

Nevertheless, the suitability of this approach remains in doubt (see Section 4.2). Aside from possible defects with our chosen proxy for household-specific effects, a relevant concern is multicollinearity between actual participation and other explanatory variables, especially location fixed effects and participation potential, making it difficult to identify the effects of actual participation precisely. The variance inflation factor (VIF) for actual participation, reported in the table, increases sharply from less than two in column (I), to around six in column (V). This is directly reflected in the magnitude of the standard errors on the point estimates for actual participation, which roughly doubles once the location fixed effects are included.

[INSERT TABLES 2 & 3 ABOUT HERE]

These concerns motivate application of the second empirical strategy. If nothing else, this provides a robustness check on the previous results. We focus on finding proxies for spatial fixed effects at the village level as this exploits the maximum amount of information available in the data. Despite the fact that Esco managers selected entire parishes as eligible, village heterogeneity may correlate with relevant unobserved (household-specific) variables. Focusing on villages also corresponds to the least restrictive assumptions regarding the amount of information used by Esco in its selection of eligible zones. Proxies for village fixed effects (VFEs) are generated from observations of specific village characteristics, as well as village means of household-level variables. With respect to the first type, we use the altitude of the village above sea level, its distance from Bundibugyo town, and the number of primary and secondary schools in the parish. With respect to the second type, we calculate village means of the household characteristics used in the model (taken from vector $\boldsymbol{X}$), as well as the share of cocoa in total household revenue, the share of land devoted to cocoa production, the number of non-farm labourers in the household and a dummy for recent acquisition of land. These variables are intended to capture specific factors that Esco considered in its selection of eligible areas.

Column (I) of Table 3 gives results for a modified version of equation (2), where a full list of fifteen proxies is used in place of the individual VFEs. Column (II) repeats this model, but employs a reduced number of proxies based on a stepwise exclusion procedure.[xv] Following Coleman (1999; also Pitt and Khandker, 1998), various tests can be applied to examine the adequacy of these proxies. The results of a Hausman test comparing the model in Table 2 column (V) against that of Table 3 column (II) cannot reject the null hypothesis that there are no systematic differences between the estimates for the common variables (probability = 63.8%). Auxiliary regressions, which regress the predicted VFEs taken from estimates of Table 2 column (V) on the full set of regressors in Table 3 columns (I) and (II) are also highly supportive. They indicate that once either the full or reduced set of location proxies are added, there is no remaining correlation between the household regressors and the estimated VFEs, which would have signalled the existence of omitted variables bias.

The estimates in columns (I) and (II) of Table 3 suggest a positive and significant welfare impact from scheme participation. The point estimates are highly comparable to those in the final three columns of Table 2, but are now significant. This reflects a reduction in multicollinearity (i.e., smaller standard errors) as well as an increase in degrees of freedom. From these estimates we expect that a 10% increase in the volume of sales of organic cocoa to Esco (as a share of total) generates around a 6% increase in net cocoa revenue. Using the selected subset of VFE proxies, column (III) of Table 3 presents the results for the IV approach. The table reports results for the second-stage, corresponding to equation (4), based on the GMM two-step procedure. Various auxiliary under- and weak-identification tests are passed comfortably, attesting to the overall strength of the excluded instrument (Kleibergen-Paap Wald F-statistic = 142.06). The point estimate for δ is significant and within (99%) sampling error of previous estimates, at 1.17 log points. Other coefficients are consistent with previous results.

Two robustness checks are run on these IV results. Column (IV) adds the measure of potential participation, which in principle is orthogonal to the instrument and, thus, should not alter the estimates for δ. Column (V) employs additional instruments – namely, four dummy variables corresponding to quintiles of mean village cocoa revenue share.[xvi] This permits a test of over-identification (Hansen J-

Test), which gives no cause for concern (probability = 54%). In both sets of results the estimates for δ remain around one and all other estimates are broadly unchanged. This provides strong confirmatory evidence against the null hypothesis. It suggests that the first empirical strategy, which delivers lower point estimates for δ, may suffer from over-controlling bias.

The reduced form associated with the latter IV approach is of independent interest. This is reported in the final column of Table 3, derived by substituting equation (3) into equation (4). The positive and significant coefficient on scheme eligibility confirms its strength as an instrument (see Baum et al., 2007). Moreover, given that the effect of eligibility on scheme participation is expected to be greater than zero and less than one (as the scheme continues to operate and we assume rational agents), then the reduced form gives direct and unbiased information about the sign and magnitude of the welfare effect from scheme participation (δ).[xvii] There are additional reasons to focus on these estimates. As Dave and Kaestner (2002) note in a different context, reduced form results often hold greater policy relevance than structural estimates because they capture the overall impact of the underlying policy tool, taking into account the efficacy of this tool in altering behaviour. This is pertinent here as the reduced form captures the average welfare impact associated with scheme eligibility (the policy tool), which is the joint (multiplicative) effect of eligibility on participation (γ) and participation on welfare (δ). Interpretation of the reduced form also is intuitive in this case as the 'treatment' variable is binary. The results in column (VI) tell us that, on average, households that have been eligible to participate in the scheme are now 0.70 log points wealthier than non-eligible households, controlling for endogenous program placement, village heterogeneity and (observed) household characteristics. (See Appendix B for additional robustness checks).

5.3. Economic mechanisms

The previous sub-section established a positive and significant welfare impact from the scheme. The remaining question is whether this is attributable to a price premium alone, or to changes in productivity associated with farm practices (technology). Together these two channels are expected to be exhaustive.

Controlling for common changes over time, as well as household and geographic characteristics, no other economic mechanism should generate changes in real net cocoa income. Their effects are also algebraically additive, because both final and intermediate outcome variables are expressed in natural logarithms. Thus, the individual effects of prices and technology are linear in logs. Consequently, by focussing on these two intermediate outcomes, one is able to disaggregate the aggregate net income effect. Empirically this is implemented by separately regressing: (i) the log of the average price received by each household; and (ii) a measure of real cocoa output (specifically, the log of the quality-adjusted cocoa weight) on the regressors taken from the reduced form specification (column VI, Table 3). This specification is chosen for its direct policy relevance (see Section 5.2) and for its simplicity, being estimated by OLS. However, in employing the same approach as before, any remaining bias in the reduced form estimates is likely to transfer to estimates for these intermediate outcomes.

The results are given in Columns (I) and (II) of Table 4. Scheme eligibility has a 0.15 log. point effect on prices, which is highly consistent with the magnitude of the price premium discussed in Section 3. It also has a 0.47 effect on quality-adjusted output (column II). Adding the estimates for these two channels gives a combined effect of 0.61, which is closely in line with the aggregate estimate of 0.70 taken from column (VI) Table 3. This confirms the exhaustive nature of the price and technology mechanisms, which respectively account for around 23% and 77% of the measured welfare effect. Of further interest is the extent to which productivity improvements derive from changes in post-harvest processing or use of pre-harvest organic practices. Thus, Table 4 examines the impact of scheme eligibility on different aspects of technology uptake, measured here as the share of all cocoa processed to a high grade (column III) and the number of organic practices employed (column IV). These estimates show a strong positive effect running from scheme eligibility to post-harvest processing. However, the effect on pre-harvest technologies is indistinguishable from zero and there appears to be a negative change in the average of number of practices employed over time. Consequently, the principal driver behind the observed income effects has been adoption of quality-enhancing post-harvest practices.

[INSERT TABLE 4 ABOUT HERE]

5.4. Spillover effects

As a final element in the analysis, we consider feedback effects from the scheme. An initial insight is gained from Table 4, which shows strong positive period effects on real prices (column I) as well as on the use of post-harvesting technologies (column III). These reflect positive changes for all farmers – e.g., average prices have risen by over 40% and *all* households have increased their use of post-harvesting technologies by over 20%. Appendix Figure C1 provides further evidence of positive general trends. The figure plots the cumulative distribution of the intensity of cocoa processing for farmers, differentiated by scheme eligibility and survey round. It shows a marked increase in use of processing technologies across all farmers over time, strongly led by eligible households. In 2005 around 50% of non-eligible farmers processed none of their cocoa to at least a semi-fermented standard; in 2009 this had fallen to 10%. Similarly, in 2005 around 20% of eligible farmers processed all of their cocoa; in 2009 this had increased to 90%. Thus, it appears that low-cost post-harvest processing has diffused throughout the cocoa farming community, backed by a general increase in demand for high grade cocoa as well as more accurate measurement of cocoa quality. This trend is supported by the establishment of other organic cocoa schemes in the locality (see Section 3).[xviii]

6. Conclusion

We have undertaken a detailed study of an organic contract cocoa scheme in rural Uganda. The scheme is of broad interest because, in contrast to older models of contract farming in Africa, it operates only on the basis of a pared-down contract between the scheme operator and members. Existing literature provides limited guidance regarding the economic dimensions of such a scheme. To fill this gap, an analytical framework was presented. This drew attention to a range of pre- and post-harvest technology choices and their relation to output quality. Following the micro-finance literature, two empirical approaches were developed to deal with possible sources of bias. These yield broadly consistent results, although the statistical significance of the main coefficient of interest remains sensitive to specific methodological choices. In addition, a simple decomposition of the scheme effects was provided. Together, this represents the most comprehensive and careful analysis of organic contract farming in Africa to date. Nevertheless, we recognise that in observational studies such as these there is no guarantee that all sources of bias have been eliminated.

Based on the reduced form results, which are of direct policy interest, we find that the average effect of scheme eligibility on net household cocoa income is around 100% (0.70 log points). This is primarily driven by increased post-harvest cocoa processing, which improves product quality and enables farmers to access a price premium. Changes in farm methods, however, have not been restricted to participants in the scheme. Evidence points to a general pattern of market deepening and demand-induced technology adoption. While it would be rash to attribute this solely to Esco, the latter has played a leading role in providing a consistent and credible source of demand for high quality cocoa. It also has stimulated widespread adoption (by other buyers) of tools to measure cocoa quality.

We end by reflecting on the general lessons from this case. On the one hand, institutional, temporal and market specificities make generalisation problematic. The surveys occurred over a period of rapid increases in international cocoa prices and growing demand for high quality 'sustainable' cocoa. While the prospects for organic cocoa are reasonable, this does not extend to many other markets where international price trends remain uncertain. Moreover, (organic) contract farming schemes are not all alike; much depends on the details of the effective incentive structure as well as management quality.

Even so, a first lesson is that the benefits of the Esco scheme derive primarily from incentives to adopt quality-enhancing techniques rather than from its specifically 'organic' aspects. Ongoing success has been due to a credible commitment to purchase high grade cocoa, transparent quality measurement and improving scheme management. Nevertheless, certification as organic has been essential for the scheme to access a (more stable) premium niche export market, thereby enabling Esco to maintain attractive purchase commitments to members. Such market access also was critical for Esco to establish the scheme in the first place.

A second lesson, *pace* Poulton et al. (2004), is that there are likely to be trade-offs between market power and competition in niche agricultural markets. The ability to establish some market power through farming contracts provides incentives for intermediaries to invest in supplier relations (e.g., certification) and make buying commitments based on upstream export expectations. Competition, on the other hand, provides outside options that can protect sellers against aggressive pricing. It is notable in this case that many eligible and non-eligible farmers continue to sell some of their cocoa crop to other buyers. Thirdly, technology adoption need not be considered a binary step-change. Technologies such as quality-enhancing farm practices can change gradually over time, stimulated by social learning as well as direct dissemination. However, basic incentive compatibility constraints always apply, and (expected) market conditions remain fundamental drivers of household behaviour.

Similar considerations apply to whether the experience of the scheme is replicable. International market conditions are likely to continue to make it profitable to invest in smallholder contract farming for high quality cocoa (and similar versions of other traditional export crops) in the medium-term, even if the markets for specific qualities such as organic or Fairtrade remain limited. Thus, whether 'aid for trade' of the kind provided to Esco by Sida continues to be available should not greatly affect replicability. Nevertheless, the benefits of such schemes can be eroded by market saturation.

Finally, a pertinent question is whether other quality-related contract farming schemes can function as effectively as has Esco's since 2005. This depends on local and institutional success factors, of which two are worth underlining. The first is corporate. Esco is part of a small international trading house focused on the Great Lakes Region, and increasingly on cocoa as a commodity. Its international status meant that its resources were adequate to upgrade the scheme even after Sida support elapsed – something that would not be possible for most Ugandan-owned beneficiaries of the same Sida programme. Meanwhile, the company's narrow geographical and crop focus meant the scheme's success was critical for Esco's overall profitability in a way that would not be the case for companies on the scale of Cargill, ADM or Barry Callebaut (who may therefore have approached it in a different way). The second group of factors is local. Cocoa in Bundibugyo is free from serious plant health problems and Bundibugyo is a remote district with few other income streams. Hence, cocoa production can be a full-time activity and competent field staff can be recruited at low cost. While not all of these conditions are likely to be reproducible, some replication should be possible given foreseeable market conditions.

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[i] The economic literature on smallholder schemes in the tropics certified to other sustainability standards, including Fairtrade, is even smaller (cf., Pariente, 2000; Becchetti and Costantino, 2006; Giovannucci et al., 2008).

[ii] A mass of beans is necessary to achieve an optimal fermentation temperature. Fermentation and sun drying are not labour intensive, but require ongoing care and monitoring.

[iii] These figures are based on EU import data (Market Access Database), which is considered to be more reliable than official Ugandan export data.

[iv] The commonest cocoa plant health problems are black pod, witches broom and swollen shoot diseases. According to Bowers et al. (2001) these are commonest where production is in large mono-cropped plantations. They are more common where it is grown in small stands in more bio-diverse contexts as in Bundibugyo. Furthermore, the commonest of these diseases (black pod) is spread by windborne rain. Thus the remoteness of Bundibugyo from other centres of cocoa production also plays a benign role.

[v] The scheme was originally set up in 1998 by a Sudanese company but quickly abandoned due to an insurgency (1999-2001), during which the population of the scheme area were evacuated to IDP camps. For a description of the insurgency, its impact and of its wider political context see Hovil and Weber (2005). Esco is a subsidiary of the international trading house Schluter SA. It received technical assistance worth about $100,000 from Sida during 2001-05 to set up the scheme.

[vi] District administration in Uganda operates at the county, sub-county, parish and village levels.

[vii] Most farmers in the area nevertheless obtained drying tarpaulins free, as a by-product of their period in the IDP camps, where these were provided for shelter. Later, the Sida project provided shade tree seedlings free to all farmers for a time.

[viii] 6,950 were certified at the time of the second survey (January-February 2009) but not all of these had been certified at the start of 2008/09 season, whose results are covered.

[ix] An analysis of survey data from 2005 comparing expelled farmers with those remaining in the scheme did not indicate any bias in terms in factor endowments or even total cocoa output in favour of those remaining in the scheme (details available on request from authors).

[x] Farmers depositing money when making sales to Esco received a additional premium of Ush 100/- per kg. A further premium of Ush. 100/- per kg. was paid to farmers delivering over 2 tons a year.

[xi] The main results are robust to alternative definitions of actual participation, such as the percentage (by number) of sales of high grade cocoa to Esco. Results available on request.

[xii] Specifically, we use the 'all items' consumer price index for Mbarara (the nearest town in Western Uganda for which an index is available) taken from Uganda Bureau of Statistics (2009b).

[xiii] We do not impute estimates of household labour costs. Besides difficulties of recall and of arriving at a common metric for adults and non-adults, family labour is frequently employed for tasks where paid labour is never or almost never used (e.g., supervision, monitoring the drying of beans and fermentation). Therefore there is an absence of reference material on which cost estimates could be based. Further details about variable construction are available from the authors on request

[xiv] None of the results for control variables are unexpected. Note also that the two survey rounds are pooled. The appropriateness of pooling is confirmed by running an extended version of the fixed effects model given in Column (V) of Table 2, including period-specific variants of each time-varying covariate. A joint F-test of these period-covariate interaction terms is insignificant at the 10% level.

[xv] We employ a general-to-specific (backward selection) approach. The general model is given in column (I) of Table 3; the probability threshold for retaining the VFE proxies is 25%. None of the core independent variables are affected by this selection procedure.

[xvi] In other words, villages are placed into quintiles according to mean household revenue share. Choice of this variable accords with Esco's location eligibility decision rule (see Section 3). Each quintile includes both member and non-member villages.

[xvii] See Angrist and Krueger (1999) also Baum *et al.* (2007) for further discussion of the properties of the reduced form. The 95% confidence interval for γ in equation (3) taken from the first stage corresponding to the estimates reported in column (IV) of Table 3 ranges from 0.48 to 0.82 with a point estimate of 0.65.

[xviii] Other spillover effects include a general increase in membership of savings associations, also led by scheme members. Results available from authors on request.

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Table 1: Descriptive statistics by potential and actual participation

	Potential (P) → Actual (A) →	Low Low	Low High	High Low	High High	*Overall*	Significance P	Significance A
Household characteristics								
a.	Scheme eligible	11.2%	100.0%	29.2%	100.0%	*60.1%*		***
b.	Farm size (log. acres)	1.3	1.2	1.7	1.7	*1.5*	***	
c.	Cocoa trees (log)	6.6	6.4	7.0	7.1	*6.8*	***	
d.	Age of household head	40.2	43.2	44.9	49.2	*44.4*	**	
e.	Household farm workers	1.5	1.9	1.7	1.7	*1.7*		
f.	Use of hired labour	54.4%	30.2%	65.4%	72.4%	*55.6%*		
Location characteristics								
g.	Village altitude (log m.a.s.l)	6.7	6.6	6.7	6.7	*6.7*		
h.	Village distance to local capital (log)	2.3	3.0	2.3	2.9	*2.7*		***
i.	No. of primary schools in parish	5.9	0.0	4.6	1.0	*2.9*		***
j.	No. of secondary schools in parish	1.0	0.0	0.6	0.0	*0.4*		***
Revenue variables								
k.	Total farm revenue (gross, 10^3 USH)	701.7	1252.4	1169.7	2050.3	*1293.5*	***	***
l.	Cocoa revenue (gross, 10^3 USH)	660.9	1090.6	1073.1	1917.8	*1185.6*	***	***
m.	Cocoa revenue (net, 10^3 USH)	507.2	960.2	840.3	1779.3	*1021.7*	***	***
n.	Cocoa / total revenue (gross)	95.0%	87.6%	91.8%	90.8%	*91.3%*		
Variable costs								
o.	Labour costs (USH/tree)	62.2	48.6	81.8	70.0	*65.6*		
p.	Equip. costs (USH/acre)	28.0	15.9	21.6	23.8	*22.3*		
Intermediate outcome vars.								
q.	Cocoa volume (log. kg of FDE)	5.7	6.3	6.0	6.6	*6.2*	*	**
r.	Price received (USH / kg)	1240.7	1312.5	1630.2	1901.8	*1521.3*	***	***
s.	Sales to Esco (FFC+SFC / all)	11.8%	45.0%	53.9%	89.6%	*50.1%*	***	***
Technology indicators								
t.	Cocoa processing	37.8%	63.5%	98.1%	97.7%	*74.3%*	***	*
u.	Organic grade cocoa (volume share)	6.2%	63.7%	23.8%	81.1%	*43.7%*	**	***
v.	No. of sales	3.0	3.2	3.3	5.7	*3.8*		***
w.	No. of organic practices	0.4	1.3	0.4	0.8	*0.7*		***
Observations (unweighted)		78	49	9	86	*222*		
Sum of inverse probability weights		87.7	16.7	46.8	70.8	*222.0*		

significance level: *** 1%, ** 5%, * 10%

Notes: actual (A) and potential (P) participation categories are binary dichotomisations of their continuous counterparts (see text); high (low) A is defined as observations above (below) 50%; high (low) P is defined as observations above (below) its mean; all statistics are calculated using sampling weights (calculated as the inverse probability of selection, adjusted to balance the sample between eligible and non-eligible households); for all non-revenue items, descriptive statistics are group means; all revenue variables are expressed in 2005 prices; final two columns reports results of tests for whether the actual and potential participation dummies are equal to zero in simple OLS regressions of the row variable, controlling for period effects; MASL refers to metres above sea level (variable *g*); FDE refers to fermented dry equivalent (variable *q*); variable *s* gives the number of sales to Esco of fully- and semi-fermented cocoa (FFC and SFC) as % number of all cocoa sales; total number of sales (variable *v*) is used as a proxy for harvesting frequency.

Sources: own analysis based on Bundibugyo cocoa surveys (2005, 2009).

Table 2: Estimates of net cocoa income effect, household-specific effects proxy method

	(I) OLS	(II) OLS	(III) OLS	(IV) OLS	(V) OLS
Actual participation	0.998***	0.974***	0.608**	0.463	0.524
	(0.10)	(0.14)	(0.29)	(0.30)	(0.33)
Potential participation	-	0.059	-0.010	0.062	0.074
		(0.30)	(0.30)	(0.29)	(0.26)
No. of sales (log.)	0.267***	0.269***	0.265***	0.270***	0.245**
	(0.07)	(0.07)	(0.07)	(0.07)	(0.10)
Period dummy	-0.098	-0.109	0.012	0.028	0.134
	(0.15)	(0.16)	(0.14)	(0.15)	(0.21)
Cocoa trees (log)	0.737***	0.736***	0.718***	0.673***	0.684***
	(0.14)	(0.15)	(0.16)	(0.16)	(0.15)
Farm size (log)	0.105	0.100	0.129	0.203	0.229
	(0.14)	(0.13)	(0.14)	(0.13)	(0.17)
HH farm labourers	0.186*	0.186*	0.169	0.154	0.132
	(0.10)	(0.10)	(0.10)	(0.10)	(0.12)
Hired labour (1=yes)	0.186	0.185	0.200	0.187	0.182
	(0.21)	(0.22)	(0.23)	(0.23)	(0.16)
Age of hhld head	-0.004	-0.004	-0.005	-0.004	-0.004
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
Hhld head male	0.363*	0.362*	0.362*	0.347*	0.334*
	(0.21)	(0.20)	(0.20)	(0.19)	(0.20)
Fixed effects	None	None	3 subcounties	5 parishes	30 villages
Participation VIF	1.17	1.67	4.40	4.71	5.96
Number of obs.	222	222	222	222	222
R-sq.	0.63	0.63	0.64	0.65	0.64
RMSE	0.72	0.72	0.71	0.71	0.72
F-stat.	45.23	40.66	59.75	37.70	34.69

significance level: *** 1%, ** 5%, * 10%

Notes: dependent variable is the log. of real net cocoa revenue; specification is as per equation (2) in the text; 'Potential participation' is a proxy for unobserved household characteristics; fixed effects at different geographic levels are indicated below the coefficients but are not reported; intercept also not shown; all estimates are by OLS; standard errors (in parentheses) are robust to arbitrary heteroscedasticity and clustering at the village level.

Sources: own analysis based on Bundibugyo cocoa surveys (2005, 2009).

Table 3: Estimates of net cocoa income effect, location fixed effects proxy method

	(I) OLS	(II) OLS	(III) GMM	(IV) GMM	(V) GMM	(VI) OLS
Actual participation	0.529**	0.656***	0.989***	0.986***	0.899***	-
	(0.22)	(0.15)	(0.19)	(0.28)	(0.25)	
Scheme eligibility	-	-	-	-	-	0.700***
						(0.11)
Potential participation	0.328*	0.328	-	0.004	0.043	-
	(0.18)	(0.24)		(0.29)	(0.22)	
No. of sales (log.)	0.222***	0.283***	0.260***	0.260***	0.283***	0.301***
	(0.08)	(0.07)	(0.07)	(0.07)	(0.08)	(0.08)
Period dummy	0.036	-0.053	-0.008	-0.009	-0.041	0.120
	(0.20)	(0.10)	(0.12)	(0.14)	(0.16)	(0.13)
Cocoa trees (log)	0.574***	0.584***	0.694***	0.694***	0.669***	0.679***
	(0.10)	(0.10)	(0.14)	(0.14)	(0.12)	(0.15)
Farm size (log)	0.221*	0.176	0.190	0.189	0.187	0.214
	(0.13)	(0.12)	(0.12)	(0.12)	(0.14)	(0.14)
HH farm labourers	0.091	0.157*	0.162*	0.162*	0.118	0.142
	(0.10)	(0.09)	(0.09)	(0.09)	(0.10)	(0.10)
Hired labour (1=yes)	0.072	0.063	0.154	0.154	0.143	0.199
	(0.12)	(0.14)	(0.21)	(0.21)	(0.14)	(0.24)
Age of hhld head	-0.003	-0.003	-0.005	-0.005	-0.006	-0.004
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Hhld head male	0.260	0.293*	0.369*	0.368*	0.357*	0.401**
	(0.22)	(0.16)	(0.20)	(0.20)	(0.21)	(0.19)
Village land acquisition	-0.384	-0.354	-0.555	-0.556	-0.654*	-0.605
	(0.41)	(0.41)	(0.37)	(0.37)	(0.37)	(0.37)
Parish primary schools	-0.123**	-0.144***	-0.130***	-0.130***	-0.141***	-0.174***
	(0.06)	(0.05)	(0.04)	(0.05)	(0.05)	(0.04)
Parish secondary schools	0.615**	0.581***	0.534***	0.535***	0.574**	0.789***
	(0.29)	(0.18)	(0.17)	(0.18)	(0.24)	(0.15)
Participation / eligibility VIF	4.51	2.39	2.96	4.06	3.60	2.17
Number of obs.	222	222	222	222	222	222
R-sq.	0.64	0.65	0.64	0.64	0.64	0.64
RMSE	0.67	0.66	0.69	0.69	0.69	0.71
F-stat.	16.75	30.23	45.74	42.36	34.99	70.19

significance level: *** 1%, ** 5%, * 10%

Notes: dependent variable is the log. of real net cocoa revenue; column (I) includes a set of fifteen proxy village fixed effects (11 not shown); column (II) onwards includes a sub-set of three of these (reported) selected by stepwise exclusion; column (III) employs a two-step GMM estimator in which scheme eligibility is used as an excluded instrument; column (IV) runs the same model adding the potential participation measure; column (V) includes four additional excluded instruments (see text); column (VI) is the reduced form associated with the model estimated in column (III), derived from equations (3) and (4) in the text and estimated by OLS; intercept not shown; standard errors (in parentheses) are robust to arbitrary heteroscedasticity and clustering at the village level.

Sources: own analysis based on Bundibugyo cocoa surveys (2005, 2009).

Table 4: Decomposition of income effects associated with scheme membership

	(I) Price recvd OLS	(II) Cocoa volume OLS	(III) Processed OLS	(IV) Practices Poisson
Scheme eligibility	0.146***	0.467***	0.388***	-0.134
	(0.04)	(0.11)	(0.06)	(0.32)
No. of sales (log.)	-0.012	0.265***	-0.015	0.076
	(0.02)	(0.07)	(0.02)	(0.18)
Period dummy	0.439***	-0.237**	0.211***	-0.774***
	(0.02)	(0.10)	(0.05)	(0.23)
Cocoa trees (log)	0.071**	0.627***	0.037	-0.070
	(0.03)	(0.13)	(0.04)	(0.17)
Farm size (log)	0.018	0.191	0.102**	0.423**
	(0.03)	(0.14)	(0.05)	(0.17)
HH farm labourers	-0.054	0.185*	-0.004	0.118
	(0.06)	(0.09)	(0.06)	(0.19)
Hired labour (1 = yes)	0.071***	0.361*	0.029	-0.122
	(0.02)	(0.18)	(0.05)	(0.23)
Age of hhld head	-0.001	-0.004	-0.001	0.003
	(0.00)	(0.00)	(0.00)	(0.01)
Hhld head male	0.093**	0.220*	0.039	0.288
	(0.04)	(0.13)	(0.08)	(0.48)
Number of obs.	222	222	222	222
R-sq.	0.63	0.66	0.30	-
RMSE	0.22	0.59	0.31	-
F-stat. / Chi-sq.	104.41	43.26	91.38	161.53

significance level: *** 1%, ** 5%, * 10%

Notes: the dependent variable in column (I) is the natural log. of the average price received by each household from cocoa sales (in 2005 prices); the dependent variable in column (II) is the log. of the volume of cocoa output, measured in kilograms of fermented dry equivalent (FDE); the dependent variable in column (III) is the share of cocoa processed to a high grade; in column (IV) the dependent variable is the number of organic practices adopted; all specifications estimated using sampling weights; the intercept and four proxy village fixed effects (see Table 3, column V) are included but not shown; columns (I) to (III) estimated by OLS; column (IV) estimated via a Poisson regression; standard errors (in parentheses) are robust to arbitrary heteroscedasticity and clustering at the village level.

Sources: own analysis based on Bundibugyo cocoa surveys (2005, 2009).

WEB APPENDIX A: DATA COLLECTION METHODS

Surveys of households eligible to participate in the scheme and non-eligible households were undertaken in late 2005 and early 2009 using a questionnaire administered to household heads by one of the authors assisted by enumerators. This covered household demographics, factor endowments, agricultural revenue and expenditure, marketing behaviour and selected aspects of consumption. Data also was collected on farmers' use of a range of farm technologies, in most cases through physical observation.

Two-stage random sampling was used to select eligible and non-eligible households in both surveys. In 2005 three of the 38 'organic' (eligible) villages in Ngamba and Burondo parishes were purposively selected to reflect the range of local agro-ecological conditions. 30 farmers were then randomly sampled from a total of 199 farmers registered by Esco in these villages. The 2005 control (non-eligible) group was randomly sampled from a list 163 cocoa farmers prepared by local leaders in three villages in Busaru parish, a nearby 'conventional' area. These villages were chosen for their similarity to the agro-ecological conditions represented by scheme participants, in terms of soils, mix of altitudes and rainfall patterns.

In 2009, 16 of the original 38 villages in the scheme were selected for the 'organic' sampling frame, including all three sampled in 2005. 697 farmers living in these villages had been certified continuously since 2001-02, of which 90 were sampled at random from growers' lists. It was not possible in 2009 to select a control group from Busaru parish again, since by this time most farmers there also had become certified organic. Although these might represent a useful sample for pre- and post-treatment analysis, these villages were only certified organic in 2008. Hence, while they were no longer 'conventional', they had not been certified organic for long enough for any scheme participation effects to be evident. Instead, eight villages in Bundinyama parish and three villages in Kaghema parish were selected to represent the District's remaining conventional area. A control group of 78 non-eligible households were randomly selected from a list of 825 cocoa farmers prepared by local leaders in these villages. Thus, over the two survey rounds, a total of 222 households from 30 separate villages were interviewed.

WEB APPENDIX B: PLACEBO REGRESSIONS

As a robustness check on the results in Table 3 we run a set of 'placebo' regressions (e.g., Card, 1990). The idea is to investigate whether pre-treatment or highly persistent variables are systematically related to the treatment variable. If so, then it is likely that relevant variables have been omitted from the specification, meaning that estimated treatment effects will be biased. As no pre-treatment variables are available, five variables that are expected to be uncorrelated with scheme participation or eligibility are examined. Each row of Table B1 reports selected results from two separate OLS regressions in which the dependent variable is one of these 'predetermined' row variables, and the RHS variables are based on the reduced form model from column (VI) of Table 3. The first two columns report the coefficient on scheme eligibility and associated probability that this is equal to zero for a regression without any proxies for village fixed effects (VFEs); the third and fourth columns report results when the selected subset of VFE proxies employed in Table 3 (column II onwards) are included.

Two results confirm the necessity of including these proxies to address endogenous placement bias. Non-cocoa revenue is presumed to be independent of scheme eligibility as the majority of sampled households strongly specialise in cocoa production; nevertheless, it may be correlated with certain unobserved (village) characteristics. This is supported by the significant result when the VFE proxies are not present; once included, however, the partial correlation coefficient on scheme eligibility is not significantly different from zero. The same pattern is repeated for village altitude – once the VFE proxies are included as controls, the partial correlation coefficient on scheme membership is insignificant. All other placebo regressions support the preferred specification.

Appendix Table B1: Summary results of placebo regressions

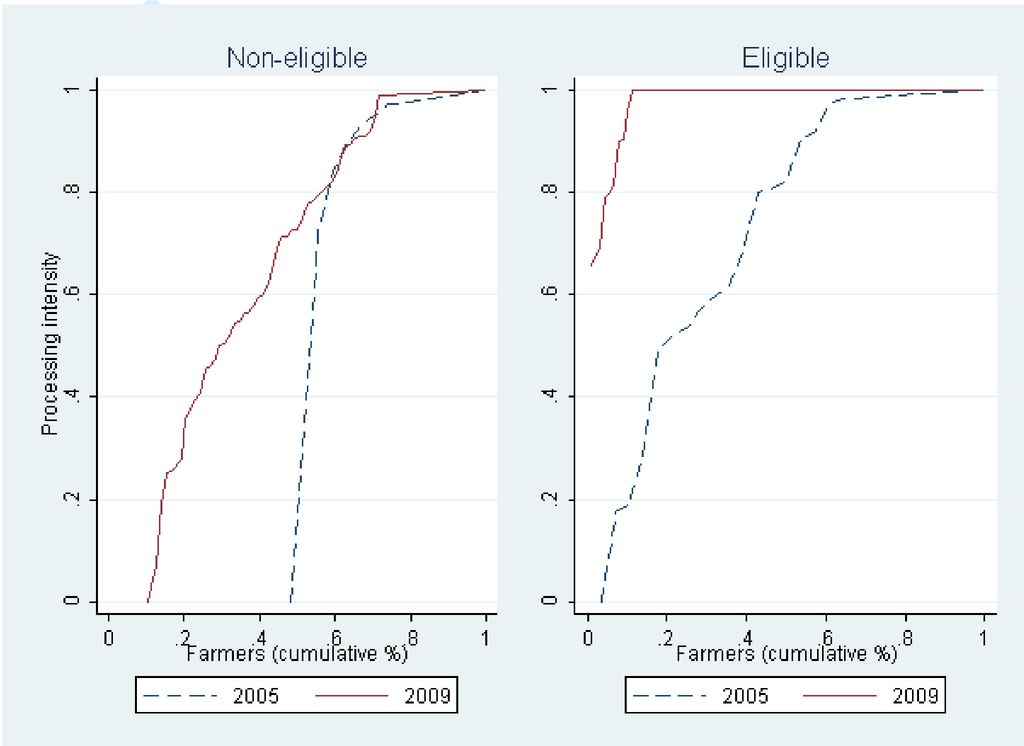
	Without VFE proxies		With VFE proxies	
Dependent variable	Coefficient	Probability	Coefficient	Probability
Non-cocoa revenue	2.78	0.00	1.28	0.28
Dependency rate	0.01	0.59	-0.04	0.42
Household non-farm labourers	0.06	0.42	0.16	0.34
Exclusively agricultural hhld	-0.02	0.72	-0.02	0.81
Village altitude	-0.07	0.02	0.00	0.95

Notes: each row reports summary results from two separate OLS regressions where the dependent variable is the indicated row variable and the explanatory variables follow the specification in column (VI) of Table 3, respectively without and with the selected subset of VFE proxies; columns give the estimated coefficient on scheme eligibility from each of these regressions and the corresponding probability that this is equal to zero (from a Wald test).

Sources: own analysis based on Bundibugyo cocoa surveys (2005, 2009).

WEB APPENDIX C: ADDITIONAL FIGURE

Appendix Figure C1: Cumulative distribution of farmers by cocoa processing intensity (in %, vertical axis) with farmers distinguished by scheme eligibility and survey period



Source: own analysis based on Bundibugyo cocoa surveys (2005, 2009).