Does Fair Trade Help to Avoid Poverty Traps? The effect of Fair Trade on Producers' Income and Schooling Decisions

Does fair trade help to avoid poverty traps? The effect of Fair Trade on producers' income and schooling decisions [1]

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Abstract
We evaluate the impact of fair trade (FT) affiliation on a sample of (treatment and control) producers from two different fair trade projects in a poorer and a relatively better off area of Peru. In both projects, we find that producer's income is significantly associated with years of affiliation after controlling for spillovers/externalities. Estimates on the determinants of schooling decisions and education gap on backcast panel data are not at odds with the *luxury axiom* hypothesis, showing that the impact of affiliation years on the dependent variable is stronger in the project with relatively better-off producers. This result is also consistent with the relatively higher returns on (parental) education estimated in the same project.

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1. Introduction

There is well-established empirical and anecdotal evidence that an unlimited supply of labour (Deaton, 1999) and the excess market power of local intermediaries and moneylenders often lead marginalized primary agricultural and textile producers to low (below the marginal product value) earnings which prevent their escape from poverty (Ray, 2000; Becchetti and Trovato, 2005). In this framework low bargaining power along the product chain, low productivity, and insufficient household income leading to child work may create poverty traps which last for generations.

Fair trade is an initiative promoted by European and North American trade organizations aimed at fighting poverty traps with trade diversification benefits and the creation of an alternative value chain which provides higher economic value and social benefits for these producers. [2] The fair trade

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[2] IFAT (the main association gathering producers and fair trading organizations) defines Fair Trade criteria as follows: i) Creating opportunities for economically disadvantaged producers; ii) Transparency and accountability; iii) Capacity

"package" includes capacity building, a price premium which compensates producers for their low market power and never falls below a defined "threshold", an anticipated financing scheme intended to break the monopoly of local moneylenders, and an extra premium to finance local public goods (via training courses, health facilities, schooling support) provided by the local producers' association.

The FT initiative has significantly gained momentum in recent years (Moore, 2004; Hayes, 2004). In the 2003-2007 period net sales in Europe grew by 40 percent per year and FT products (sold not only by specialized retailers such as "world shops", but also by many supermarket chains)[3] have acquired significant market shares (49 and 25 percent of bananas in Switzerland and the UK respectively, around 20 percent and over 3 percent of ground coffee in the UK and the US respectively (FAO, 2009).[4]

The application of FT criteria, however, is widely questioned by the general public and the media: does the FT premium exist? Does FT generate the promised effects on producers' well-being, and does it contribute positively to producers' decisions to send their children to school? These concerns demonstrate that the fair trade debate urgently requires empirical evidence, which can be obtained with impact analyses on the effects of FT affiliation on local producers.

The literature on this topic is scant and limited to a few well-structured case studies (Bacon, 2005; Pariente, 2000; Castro, 2001; Nelson and Galvez, 2000; Ronchi, 2002; Yanchus and de Vanssay,

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building; iv) Promoting Fair Trade; v) Payment of a fair price; vi) Gender Equity; vii) Working conditions; *(healthy working environment for producers. The participation of children (if any) does not adversely affect their well-being, security, educational requirements and need for play and conforms to the UN Convention on the Rights of the Child as well as the law and norms in the local context.); viii)* The environment; ix) Trade Relations. (*Fair Trade Organizations trade with concern for the social, economic and environmental well-being of marginalized small producers and do not maximise profit at their expense. They maintain long-term relationships based on solidarity, trust and mutual respect that contribute to the promotion and growth of Fair Trade. Whenever possible producers are assisted with access to pre-harvest or pre-production advance payment).*

[3] One of the most interesting features of the fair trade phenomenon is the involvement of big players in the production and distribution industry (Nestlè introduced a fair trade product in its product range in October 2005, Co-op UK has launched its own fair trade product line, Starbucks has rapidly became the main purchaser of FT coffee in recent years). Becchetti et al. (2008b) show that partial adoption of FT practices is the optimal strategy for profit maximising incumbents after a fair trader's entry in a horizontal differentiation duopoly in which firms compete on prices and ethical features of the product (for a discussion on competition between fair trade dedicated retailers and supermarkets see also Kohler, 2007).

2003). The only exceptions are the econometric analyses by Ronchi (2006) and Becchetti and Costantino (2007). Ronchi (2006) finds on a panel 157 mill data that FT helped affiliated Costa Rican coffee producers to increase their market power. He concludes that FT benefits are of the vertical integration type and that "the decision to support fair trade requires other information about its costs and benefits". Becchetti and Costantino (2007) show that, after controlling for selection bias, fair trade affiliation has a significant impact on several well-being indicators, even though it does not seem significantly to improve human capital investment among FT/Meru Herbs producers in Kenya.

Our aim in this paper is to take the analysis further by means of three main innovations.

First, we directly address the problem of externalities by evaluating whether: i) fair trade affiliation leads to improved sale conditions also on other trading channels for affiliated producers; ii) the introduction of fair trade affects the well-being of non-affiliated producers in the area.

Second, we propose a methodological innovation consisting in the use of a retrospective panel data approach which makes it possible to build panel data retrospectively without requiring unreasonable memory efforts by respondents. Differently from McIntosh et al. (2010), who consider house restructuring events, we build our retrospective panel data on simple questions about children's age and schooling years which enable us to reconstruct the pattern of household schooling decisions over a long time interval.

Third, we compare returns on FT affiliation and the effects of the latter on schooling in two different areas (a very poor one, with an average daily income below the one-dollar PPP threshold, and another, more developed, area with average daily income above 6 dollars). The last effect is evaluated by reconstructing all schooling decisions taken by affiliated producers in the previous years with a backcast panel approach.

[4] For an evaluation of the effects of FT from the perspective of traditional trade theories see Maseland and De Vaal (2002). Other relevant papers dealing with various aspects of the impact of FT are those by LeClair (2002), Moore (2004), Hayes (2004) and Redfern and Sneker (2002).

On this last point, consider that both labels which ban child work (i.e. Rugmark) and Fair Trade aim to sell ethical intangibles to concerned consumers, but their approaches are quite different: in the first case, we have a no-child-labour constraint with market clearing prices of the intermediate product sold by primary producers, while, in the second case, we have an exogenous price premium (and the additional benefit of contribution to producers' capacity building), with affiliated producers being free to choose whether or not to send their children to school. Hence, FT may realize the goal of reducing child labour only indirectly by helping to create a virtuous circle between the *substitution* and *luxury axioms* (Basu and Van, 1998).

The *substitution axiom* states that the diffusion of child labour may affect the local labour market by depressing the level of adult wages. The *luxury axiom* states that parents send children to school if they overcome a given income threshold. The combination of the two may create a vicious circle in which household income below a subsistence threshold triggers child labour and the diffusion of child labour in the area depresses adult wages, so that child labour becomes more necessary.

Because fair trade increases household income (due to the price premium, the price stability effects, and the medium-term productivity effect generated by its capacity building approach), it may turn the vicious circle into a virtuous one. This happens if the reduction of child labour in the area due to the (price premium) income effect,[5] generates a positive effect on adult wages in the area. The latter reinforces the initial shock, pushing such wages farther from the luxury axiom threshold.[6]

Our test of the impact of fair trade on child labour will draw on the existing literature and, more specifically, on several empirical papers evaluating the effect of conditional cash programs intended to discourage child labour by lowering the cost of schooling via educational transfers (Progresa in Mexico, Bolsa Escola in Brazil, Mid-day meals program in India) (Edmonds, 2007). Such programs

[5] In fact, this is not just a pure income effect since fair trade may alter the employment opportunities available in communities, thereby affecting returns to education, or at least to literacy.

[6] Besides these partial (income and substitution) effects, it is obvious that additional indirect changes induced by FT in affiliated and non-affiliated producer labour markets in a general equilibrium framework also matter and should be taken into account when evaluating the total impact.

have proved effective in reducing child labour (Schultz, 2004; Skoufias and Parker, 2001; Schady and Araujo, 2006).

It is however impossible to disentangle in these empirical findings the effect of household income from the reduction of schooling cost effect. The advantage of our analysis is that fair trade generates an income effect (albeit from different sources) without any change in the cost of schooling. Another significant difference with respect to these well-known programs is that they provide subsidies conditional on the schooling choice and thereby assume that it is worthwhile to encourage schooling (Baland and Cedric, 2007). In fair trade, an additional degree of freedom is given to households, which may decide whether or not to send children to school.

What are our a priori on the effects of FT on child schooling decisions? Notwithstanding the widespread agreement on the importance of the *luxury axiom*, there is no consensus in the literature on the impact of income effects on child labour. Most papers find the expected negative relationship between income and child labour (Psacharopoulos, 1997; Cartwright, 1999 and Edmonds, 2005). There are, however, cases in which the effect is not significant (Deb and Rosati, 2002) and theoretical models in which the income effect may be offset by a substitution effect due to the increase in children wages (Bhalotra and Heady, 2003; Psacharopoulos, 1997).

We therefore aim to verify which of these three possibilities is supported in our empirical study.

The paper is divided into five sections including the introduction and conclusions. In the second section we describe the characteristics of the two projects exhibiting the specific FT features. In the third section we present descriptive evidence on sample characteristics and FT externalities. In the fourth section we document econometric findings on returns from affiliation and the effects of the latter on child schooling by means of a backcast panel approach controlling for selection bias and endogeneity. The final section concludes.

2.1 The Juliaca project characteristics

The fair trade importers involved in one of the two projects considered (those working in the Juliaca area) have a long-term relationship with a second-level[7] producer association called Minka.[8] The goal of Minka is that of "promoting the development of textile producers' organizations by respecting their cultural traditions and the local environment together with the full satisfaction of consumers". To achieve this goal Minka operates with Fair Trade organizations.

Minka buys products from 61 (first-level) groups[9] of producers in various areas of the country (Piura, Lima, Ayacucho, Puno, etc.). The second largest group (after the one working in Lima) creates wool products and operates in the District of Juliaca (Department of Puno) located around Lake Titicaca.

The living conditions of producers in the district of Juliaca are close to the subsistence level. The traditional activity in the area is agriculture, but its extremely low returns and high risk due to the local climate (drought in summer and frost in winter) have induced producers to add craftsmanship to farming, and for Minka and fair-trade affiliates it has gradually become their main activity.

Minka declares that it adopts the following key operating principles in order to fulfill its mission: i) payment of a price premium to producers, unlike the traditional local intermediaries, who generally exploit information asymmetries among producers with respect to market prices, demand conditions, and final consumer tastes; ii) commitment to buying the highest quality wool for producers; iii) pre-financing up to 50 percent of the production by the first-level groups with which it concludes contracts; iv) use of part of its earnings to meet the needs of producers' groups and generally to finance training courses and other local public goods (for instance, the restoration of local council buildings, construction of bathroom facilities, etc.).

2.2 The characteristics of the Chulucanas project

[7] A second-level organisation provides services and enforces quality standards on first-level producer groups. As in the case of Minka, such services generally include training courses, marketing, and pre-financing and export facilities.

[8] The word "Minka" in the Quechua language can be translated as "reciprocal help", in the sense of social cohesion within a village or among a group of people.

The second project considered involves producers in a village called Chulucanas (Department of Piura) and affiliated to a trading company called ALLPA.[10] ALLPA was originally a Trading Project initiated in 1981 by the Peruvian Institute of Research and Development (IPID). In 1986 ALLPA became a private company owned by IPID with other individual partners. ALLPA's objective is to enhance the market accessibility of low-income handicraft producers by enabling them to improve their standards of living through the pursuit of a customer-oriented vision.

ALLPA works both with Fair Trade organizations and with private companies, market importers, department stores and distributors in Europe, North America, Mexico and Australia. Its main market is Europe (60% of total exports) and the Fair Trade share in exports is 60-70 percent.[11]

ALLPA's market share was 2 percent of total handicraft exports from Peru in 2006.[12] Its craftsmen are distributed among 20 communities and 100 handicraft workshops operating under a subcontracting system in different areas of Peru (Lima, Cusco, Huancavelica, Chulucanas, Junìn, Pucallpa, Puno, Ayacucho). The production consists of five main lines: pottery (Chulucanas), jewelry, knitwear, wooden furniture, and painted glass.

ALLPA deems it important to ensure that all workshops produce high-quality handicrafts while maintaining social well-being and complying with Peru's labor law. In this regard, one of the first actions taken consisted in creating social benefits, such as legal assistance, health insurance and paid leave, for employees and artisans.

To achieve its goals of efficiency and competitiveness, ALLPA endeavors to enhance orders and to support technological innovation and investment in all the workshops, the purpose being to increase productivity, to improve quality, and therefore to ease entry by artisans into the market. ALLPA

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[9] Besides these groups there may be various other organizational forms (family workshops, cooperatives, microenterprises).

[10] In the Quechua-Ayacuchan language, *allpa* means "earth" (soil). This name was chosen because it symbolizes the raw materials used for all the company's handicraft products: clay, metal, wood, stones, cotton and alpaca.

[11] ALLPA, "Plan de negocios 2007-2011".

[12] http://www.perumarketplaces.com/ing/noticias_sector02.asp?Id=2743&c_sector=3&TitPage=Craft%C2%A0-%C2%A0News

usually pre-finances up to 50 percent of the production by workshops. It runs training courses for artisans and employees, it provides loans, and it offers technical support and supervision.

Chulucanas (the locality in which we conducted our survey) is a small village whose economy is based on highly organized pottery production. The production system is very well developed, and artisans are organized into small-to-medium sized workshops with an average of 15 employees. Local artisans have developed a type of ceramic pottery, based on ancient techniques of "negative painting",[13] which has placed Chulucanas at the forefront of fine ceramic production in northern Peru.

In the Chulucanas project, ALLPA has conducted studies on oven performance refinement, and it has improved productivity by introducing electric wood-turning in place of the traditional "paddle-made" method[14].

3. Descriptive findings

As shown in the previous section, the rationale for the joint analysis of the two projects resides in the fact that they both concern marginalized producers living in the same country but with three qualifying differences: i) products are different (pottery in Chulucanas and apparel in Juliaca); ii) the relationship of Chulucanas producers with FT is much more recent; iii) the standard of living is significantly higher in Chulucanas than in Juliaca, where the population lives at around the subsistence level. Our research may then be particularly useful in verifying how FT impact changes with different years of affiliation and living standards. In the econometric section, we will explicitly test whether the two projects can be considered jointly (under the null of the equality of the impact of different factors) or on what dimensions they should be analysed separately.

[13] Chulucanas pottery can be considered a legacy from the Pre-Columbian art of the Vicus, not only because of its quality but also because its artists have revived? techniques first developed more than 2000 years ago on the northern coasts of Peru. The negative-positive technique has been refined and its renewed use has now evolved into a wide variety of shades ranging from light to a dark, almost black, ocher. The color is obtained by mixing various fuels, such as tender or ripe (fresh or dry) leaves from banana and mango trees.

[14] New electric ovens have been introduced to improve baking capacity (up to 80 items at a time).

Our survey was based on direct interviews[15] conducted in August 2007 by two researchers on randomly selected control and treatment samples from the two projects.[16] Overall, 240 producers were interviewed, distributed into three groups: i) 80 producers from Juliaca affiliated with Minka (FT organization); ii) 80 producers from Chulucanas affiliated with Allpa (FT organization); iii) 80 producers from Juliaca and Chulucanas (40 from each project) not affiliated with the two fair trade organizations (control sample). Additional details on the survey design and on the characteristics of the two projects are provided in Appendix 1.

Descriptive statistics on the main survey variables provide a synthetic overview of the characteristics of our sample (Table 2).[17] At the time of the survey, the average (including treatment and control samples from both projects) monthly yield of the main productive activity was 396.7 soles per month, while weekly food consumption amounted to 81.9 soles. If we sum all activities we obtain a total monthly yield of 436.9 soles. Only 54 percent of producers declared that they had saved something in the past year. 64 percent of them were home-owners. Producers were 35 years old on average, and their average number of children was around 2.5. The last child born had been vaccinated in 95 percent of cases.

These mean values conceal marked differences across subgroups. To illustrate them, we present (separately for each treatment and control group) the mean values for the most relevant variables (Table 3).

A first point to be mentioned is the difference in terms of affiliation years between the two (Juliaca and Chulucanas) treatment samples (around 15 against 3). This allows us to compare the impact of

[15] The questionnaire is available from the authors upon request.

[16] We created lists of producers for each group exceeding our target number of interviews by 30 percent in order to compensate for non-responses. The response rate was very high, being close to 100 percent for treatment producers and around 85 percent for control producers. Our samples were representative of the population of affiliated (non-affiliated) farmers in terms of affiliation year, age and gender (age and gender). All other representativeness problems were addressed by the econometric analysis described below. The research was developed according to the following timetable: i) July 28 – August 2, 2007: Lima - Allpa and Minka offices: research beginning with definition of the lists of treatment and control sample producers from which we randomly extracted those to interview; ii) August 5 – 17, 2007: Juliaca - community analysis and interviews; iii) August 20 - 28, 2007: Chulucanas - community analysis and interviews; iv) August 29 – September 10, 2007: Lima - Allpa and Minka offices: organizations analysis and conclusion of research.

[17] The variable legend is presented in Table 1.

FT on producers' welfare at two markedly different phases of FT affiliation. On looking at living standards we observe that income from the main activity (*wagefirstact*) is 199 against 50 soles (219.9 against 64.2 soles for all activities) in the Juliaca case, against 663.7 and 599.5 soles (746.6 against 622.2 for all activities) in the Chulucanas case, for the treatment and control sample respectively. Considering the exchange rate with the dollar at the time of the survey (3.18 soles per one dollar), this means that the control group of Juliaca producers living in the Lake Titicaca area was well below the poverty line of one dollar per day, while FT affiliates in the same area were slightly above it, with two dollars per day. Economic conditions in the Chulucanas area were much better because the control group earned slightly less than seven dollars, and the treatment group above eight dollars per day.

In line with these data, the average weekly food consumption expenditure (*foodcons*) is 53 against 15.9 soles in the Juliaca case, and 91.4 against 95.6 in the Chulucanas case for the treatment and control sample, respectively. As a consequence, the food consumption share (*consshare*) is 49.6 percent against 61.3 percent in the Chulucanas case, and 96.1 against 99.4 percent in the Juliaca one. Another significant difference concerns age. The Chulucanas treatment group was significantly younger than the control group in the same area and, more generally, than all the other three groups. With regard to the other descriptive variables, 45 percent of FT affiliates had saved money in the past year (*lastysavtot*) in the Juliaca project against 30 percent of control sample producers, while the figures were 79 against 46 percent in the Chulucanas case. We did not observe significant differences between control and treatment groups within each project in terms of proxies for "wealth" such as ownership of a radio, a television, electricity, bathroom in the house and drinkable water (although the percentage of radio owners was markedly higher for treatment producers in the Juliaca case (93 against 77 percent of the control sample)).[18] Note that some of these variables (such as electricity and drinkable water) depend on the local supply of infrastructure, which is beyond FT control.

Finally, there were no significant differences in the number of children (*Numson)* in Juliaca (around three in both the treatment and control sample), while the treatment group in Chulucanas had significantly smaller families than the control group. Overall, Chulucanas producers also had smaller families than those in Juliaca. Adults' school years (*schoolyears*) were not significantly different between control and treatment groups in the two projects, while Chulucanas producers (more than 8 years in the control and more than 9 in the treatment sample) had on average around two years more of education than Juliaca producers (around 6 years). In the Juliaca sample, land extension (*landsize*) was larger in the treatment than in the control group. Average property size for all Chulucanas pottery producers was, in its turn, lower than in the Juliaca control group (in the Juliaca area, unlike Chulucanas, the main activity before FT was agriculture).

3.1 Price premium and FT externalities

One of the main goals of our descriptive analysis was to verify the existence of a price premium consistent with FT criteria. In the case of handicraft production, it was difficult to find a standard product on which comparisons between prices of FT and traditional intermediaries could be made. In the Juliaca project we could identify s standard product in a typical model of local wool gloves sold by both FT and local intermediaries. We found that FT importers paid a price (11.938 soles) for this product which was 4.7 times higher than the average price paid by local intermediaries to the overall (treatment and control) sample (that is, the weighted average of 3.11 and 2.068 soles in Table 3). It was not possible to make the same comparison with the control sample for Chulucanas producers owing to a lack of a sufficient number of observations on a common standardized product.

As we explained in the short survey in the introduction, another critical point discussed in the fair trade literature concerns spillovers and externalities. If FT affiliates are made better off and increase the total number of worked hours supplied locally, does FT reduce local market prices, thereby

[18] Data on these variables are omitted and available upon request.

reducing the welfare of non-FT producers (LeClair, 2002)? Indirect evidence is provided by the answers that non-FT producers gave to a direct question on the effects of FT entry on their welfare, and which showed that it had not been reduced for around 58 percent of respondents in Juliaca[19] and 38 percent of those in the Chulucanas control sample (*Improveminkarea*). The evidence is therefore mixed, and the LeClair effect seems to apply to the majority of producers in one project but not in the other , at least according to this very simple descriptive evidence, which is not free from interview biases.

A second important indirect effect relates to the impact of FT affiliation on sale conditions with local intermediaries. In this regard we have consistent evidence from two different sources. First, *local intermediaries paid higher prices for wool gloves to FT affiliates than to the control sample in the Juliaca project*, where the comparison between homogeneous products was possible (3.11 against 2.1 soles).[20] Second, 58 percent of Juliaca producers confirmed in direct qualitative questions that FT affiliation had improved sale conditions with local intermediaries in the area (*sinceminkaext*). The share of positive answers to the same question was even higher (75 percent) for Chulucanas affiliated producers, even though our limited data did not allow a direct price comparison.

Consider that the two observed indirect effects also affected our comparison between treatment and control groups and will therefore be taken into account in the econometric part of the paper. The first effect (FT also improves the well-being of non-affiliated producers) tends to narrow differences and may lead to an underestimation of the FT effect. The second one (FT increases the bargaining power of affiliated producers also with local intermediaries) tends to widen differences. However, this second positive external effect is due to fair trade and therefore does not produce a

[19] The phenomenon may be explained by two factors: training courses open to non-FT affiliates and increased bargaining power of both FT and non-FT producers (the latter can include FT affiliation as an outside option when bargaining with local intermediaries) in the area. Very strong anecdotal evidence on this second point is provided by the transformation of the local wholesale market after FT entry. Before, there were two weekly market sessions (one of them illegal, held late at night with less favourable conditions for sellers), afterwards the supply to local intermediaries dropped and the night sessions were eliminated.

[20] The comment in footnote 19 on the transformation of local wholesale markets since FT entry in the area is consistent with this evidence.

bias in terms of evaluation of the total FT effect. We therefore needed to control for this kind of bias only if we wanted to disentangle the different components of the overall FT effect on producer's well-being.

A final interesting descriptive finding relates to the perceived relative standard of living with respect to the average standard in the area (*standlivcomptot*). Here the producer was asked whether his/her standard of living was far superior, slightly superior, equal, slightly inferior or far inferior to the average in the area. We gave a value of four to the first answer, three to the second, up to zero to the last one (2 being a declared standard of living equal to the local average). The results show that Juliaca producers judged their standard as slightly above average (an average value of 2.19), while the control sample judged it as more than slightly below average (an average value of .75). The distance is narrower in the Chulucanas case (2.09 against 1.83). These results are particularly interesting because the observed difference in the perceived relative standard of living does not depend only on the declaration of FT affiliates; it depends also on that of control sample producers, who acknowledged that their standard of living was below the average (to which FT affiliates significantly contributed since they represented approximately one fourth of producers in both areas).

4. Econometric findings

The aim of our econometric analysis is to test the statistical and economic significance of the observed differences in target performance variables (income, children's education) between the treatment and control samples of producers in the two different areas.

Our base OLS specification (column 1, Table 4.1) is

$$Log(Wagefirstact)_i = \alpha_0 + \alpha_1 FTage*C_i + \alpha_2 FTage*J_i + \alpha_3 FTagesq*C_i + \alpha_4 FTagesq*J_i + \alpha_5 Age*C_i +$$
$$+\alpha_6 Age*J_i + \alpha_7 Landsize*C_i + \alpha_7 Landsize*J_i + \alpha_8 Female_i + \alpha_9 Selfprod_i + \alpha_{10} Numtredechan*C_i +$$
$$+\alpha_{11} Numtredechan*J_i + \alpha_{12} Othincome_i + \alpha_{13} Secactno_i + \alpha_{14} Married_i + \alpha_{15} Divorced_i + \alpha_{16} Separated_i +$$
$$+\alpha_{17} Schoolyears*C_i + \alpha_{18} Schoolyears*J_i + \alpha_{19} Numson + \alpha_{20} Secactno_i + u_i \quad [1]$$

where producer's income is the dependent variable. The specification includes as regressors standard demographic dummies such as age, gender, education (schooling years of the respondent), family status and number of children. C and J are shorthand for Chulucanas and Juliaca dummies interacted with main variables (for the variable legend see Table 1). The last two types of variables are important in order to know the number of household members. Finally, we considered whether the individual had more than one productive activity or other sources of income, and we used land size as a proxy for wealth. Given the focus of our analysis, we added the number of trading channels as a measure of trade diversification. Significance of FT affiliation years implies that we rejected at least one of the following null hypotheses $H_{01}: \alpha_1=0$, $H_{02}: \alpha_2=0$, $H_{03}: \alpha_3=0$, $H_{04}: \alpha_4=0$.

For each performance variable considered, we first provided an estimate on the overall sample with separate regressors for each of the two projects and then tested the hypothesis that coefficients are not significantly different from each other. When this hypothesis was rejected we moved to separate estimates for the two projects.[21]

Our first performance indicator was the producer's income from its main activity. The reference theoretical framework was that of Mincerian (1974) equations where the dependent variable is in logs and coefficient magnitudes measure returns of different factors (schooling years, FT affiliation, etc.).

The results from the joint estimate for the two projects presented in Table 4.1 (column 1) show that FT affiliation years have a positive and significant nonlinear (concave) effect on the yield of the main producer activity in the overall sample estimate. This nonlinearity is an unexpected finding. Two possible interpretations for it may be that capacity-building effects of FT have decreasing marginal returns and that the positive externality of the higher bargaining power with local intermediaries is typically concave (and mainly determined by a strong initial effect).

Consider, however, that this finding may in part be also due to cohort effects, which are difficult to account for in cross-sectional estimates. By comparing young and old affiliates we found that

farmers with fewer than 6 affiliation years were significantly younger and less educated than those with more than 11 affiliation years, even though the age at which the two groups affiliated on average was not significantly different. Cohort effects should be in part controlled by the introduction of age and education as separate regressors implying that the affiliation impact is net of age and education effects.

With regard to control variables, the significance of age and education years is what we usually expect from wage equations. Finally, a crucial variable in the Juliaca, but not in the Chulucanas subsample, is the number of trading channels. The coefficient magnitude shows that the return on an additional trading channel is 32 percent. Consider that large part of this effect may be attributed to FT affiliation, since FT adds one trading channel to the existing ones.[22]

Surprisingly, the test on the equality of coefficients did not reject the null that the returns from FT affiliation in the two projects were not statistically different from each other.[23] On the contrary, the trade diversification effect was highly relevant in Juliaca, while not in the Chulucanas project. These findings document, on the one hand, that affiliation benefits have similar returns in completely different environments.[24] On the other hand, they highlight that the opportunity of having an additional trading channel was essential in the much poorer context of Juliaca producers, but not for Chulucanas producers, who were much better off and already had a good capacity for selling on different channels, given the widely acknowledged quality and originality of their products.

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[21] All the estimates which follow are with White (1980) heteroskedasticity robust standard errors.

[22] The results concrning the effect of FT affiliation on the total return from all producer activities were analogous to those registered on the income from the first activity. They are omitted for reasons of space and available upon request.

[23] The slight difference in the magnitude of returns, however, turned into a larger difference if we calculated the cumulative ten-year return on the basis of our coefficients (42.8 percent in Juliaca and 62.8 percent in Chulucanas).

[24] We also ran estimates in which the dependent variable was not in logs to calculate the FT effect in absolute values. We found that the marginal effect of FT affiliation was of 23 soles in the first year (12.5 for Juliaca producers and 86 for Chulucanas producers) and fell bys up to 10.42 soles in the 8th year (7.18 for Juliaca producers and 17.39 for Chulucanas producers). The estimated cumulative ten-year effect was of 151 soles in the same year (90 for Juliaca producers and 418 for Chulucanas producers).

With regard to non FT related variables, significant differences were found for age (3 against -1.5 percent) and education (6 percent against 0), both significantly higher in the Chulucanas area. Unconstrained estimates for the two projects confirmed our previous findings.

In a final robustness check of the estimated returns to FT affiliation we wanted to check whether the external effects documented in the descriptive findings may have affected our econometric outcomes. We again estimated the aggregate model with project specific variables by adding two regressors. The first, (*extbargtreat),* measured the effect of the self-declared increased bargaining power with local intermediaries of affiliated producers. The variable was the product of the (0/1) affiliation dummy with a unit dummy for those who responded affirmatively to the relative question. The second (*extcontr*) proxied the impact of the declaration provided by control producers about their improved/non-improved economic conditions in the area after FT entry. The variable was the product of the (0/1) dummy of participation in the control group with a unit dummy for those who responded affirmatively to the relative question. The two variables were not significant in the estimates and their introduction did not alter previous results on other regressors.

In Table 4.2 we provide evidence on some robustness checks on the base specification and on the specification augmented for the externality variables. In a first check we introduced a quadratic specification for age by including squared age; in a second, the number of trading channels was in logs; and in a third we combined the two changes. The results on affiliation years and on the main variables of interest were unaffected.

4.1 The selection bias problem

As well known, a first-best impact analysis should compare the observed effects of a given factor with the counterfactual situation (what would have happened to the same individual in the absence of the treatment). Unfortunately this was not possible in our case, since the same individual could not be exposed to two different situations simultaneously. Consider also that we could not apply more sophisticated approaches such as randomized treatment (Duflo and Kremer, 2004) (FT already

existed before we started our analysis) or regression discontinuity design (Angrist and Levy, 1999) (we did not have a continuous variable on which a threshold determined inclusion in the treatment or control group).

A second-best approach is that of building control samples which are as homogeneous as possible to the treatment ones. We did this by using producers working in the same field of activity and living in the same area of FT affiliates as the control sample, and also by looking at the impact of treatment length (affiliation years) on the group of FT farmers. In this respect we exploited a small advantage with respect to many standard impact studies where is not possible to examine the gradual effects of the treatment.

However, a major problem remained: as well known, in the presence of non-random program placement, the positive performance of the treated may not depend on the effectiveness of the treatment, but on an ex ante qualitative difference between producers in the treatment and control sample. The second effect is almost inevitable in, for instance, fields such as that of microfinance studies in the presence of a proper screening activity by the financial intermediary.[25] The risk is less severe in FT impact studies if access to the producers' association affiliated to FT is not based on the evaluation of the quality potential of the applicants (explicit selection), or if it is proved that not only the most enterprising producers decide to affiliate (implicit selection).[26]

To tackle the selection bias problem we estimated a treatment regression model using an approach which is standard in the literature and applied to impact studies of this kind (see for instance Becchetti and Costantino, 2007 and Bolwig and Gibbon, 2009).

The treatment-effects model was a full maximum likelihood estimator which considered the effect of the endogenously chosen binary treatment of FT affiliation on another endogenous continuous variable (producer's income), conditional on two sets of independent variables.

The estimated model was

[25] Among the main contributions tackling the issue see Hulme and Mosley (1996), Pitt and Khandker (1998) and Coleman (1999).

$$Log(Wagefirstact)_i = \alpha_0 + \alpha_1 Age*C_i + \alpha_2 Age*J_i + \alpha_3 Landsize*C_i + \alpha_4 Landsize*J_i + \alpha_5 Selfprod_i + \alpha_6 Secactno_i + + \alpha_7 Schoolyears*C_i + \alpha_8 Schoolyears*J_i + \alpha_9 FT*J + \alpha_{10} FTage*C_i + \alpha_{11} FTage*J_i + \alpha_{12} FT + v_i$$

[2.1]

$$FT_i = b_0 + b_1 Othincome_i + b_2 Married_i + b_3 Numson + z_i \qquad [2.2]$$

where variables are defined as in (1) (see the variable legend in Table 1), C and J are shorthand for Chulucanas and Juliaca (0/1) dummies and FT is a (0/1) dummy measuring FT affiliation status. In the two equation system (v) and (z) are bivariate normal random variables with zero mean and covariance matrix $\begin{bmatrix} \sigma & \rho \\ \rho & 1 \end{bmatrix}$. Note that, if ρ is positive (negative), the OLS coefficient on FT will be biased upwards (downwards). The likelihood function for the joint estimation of [2.1] and [2.2] was provided by Maddala (1983) and Greene (2003). The estimates of $\alpha_9 - \alpha_{12}$ should be consistent if the assumption that η and ε are jointly normally distributed holds.

We estimated the model by verifying that two conditions which make treatment regression models valid held. First, the selection regressors were not factors directly affecting the performance variable in the first equation (see Table 4.1). Second, we omitted them from the first equation. In order to satisfy condition one we had to omit age among regressors of the second equation.

The treatment regression model was estimated on the overall sample by introducing slope dummies for affiliation years, education and age. We estimated a unique specification for both projects allowing for project-specific variables where hypotheses on the equality of regressors effects were rejected in the estimate presented in Table 4.1 (column 1). In order to avoid over-parametrisation problems we also eliminated variables which proved to be insignificant in previous estimates and, in particular, in the estimate which controlled for externalities (Table 4.1, column 4). The additional difference, with respect to the model which did not correct for selection bias, was that the treatment regression model imposed the presence of the affiliation/no affiliation dummy, which was the same for both projects and was significant. This variable inevitably captured a once-for-all FT effect

[26] Becchetti and Costantino (2007), in their impact analysis of FT affiliated farmers belonging to the Meru Herbs

analogous to that of the number of trading channels. We therefore omitted also this last variable from the estimate.

The treatment regression model confirmed that returns to schooling and age are significant in the Chulucanas but not in the Juliaca project. Note that a once-for-all FT effect seems to prevail in the Juliaca, against a weaker affiliation year effect in the Chulucanas case. The test on the independence of the equations (H_0: $\rho=0$) in the treatment regression model did not reject the null. Based on our estimate, the lack of correlation between residuals of the first and second equation therefore signals that there is no selection bias between affiliated and non-affiliated, given the specification adopted. We therefore continue to refer to the estimate presented in Table 4 in evaluating the returns of FT affiliation.[27]

We also evaluated what happened to the estimate of the first equation without the joint estimate of the selection equation. Coefficients were almost the same for all variables except the impact of the FT dummy, which was larger in the treatment regression model (Table 5, column 1). This seems to indicate a downward bias, but the lack of significance of the residuals correlation tells us that the bias may not be significant.

In commenting on our results, however, we acknowledge that the vector of factors in the selection equation is poor. Hence we cannot rule out that, with the addition of unobserved controls in the selection equation, there is a selection bias which may act in two different directions. First, we can reasonably assume that farmers with higher skills ex ante are more likely to affiliate. If this is the case the coefficients of affiliation years in Table 4.1 are likely to be overestimated and should be considered the upper bound of the true effect. Second, given the characteristics of cooperatives, affiliation is generally related to risk aversion (less risk-averse individuals prefer to risk on their own behalf and not be obliged to confer the product to the cooperative central in the good state of

association in Kenya, find anecdotal evidence of an implicit selection, and control their findings for this effect.

[27] Setting aside information from the second step, we also tried with different socio-demographic variables as controls in the second equation. The result on the independence of residuals from the two equations was robust to these changes.

nature when market prices are high), as well as to a willingness to cooperate with others and to subordinate one's own will to that of an organization. If these characteristics are inversely correlated with a producer's skill and productivity, the effect of affiliation in Table 4.1 should be downward biased.

4.2 The effect of FT affiliation on child schooling

In measuring the effect of FT affiliation on human capital we had an important advantage compared to previous estimates. We could in fact create "backcast" panel data by asking each producer about the number of his/her offspring, their age, and the number of schooling years for each of them. To complete our information, we also asked producers the age at which each child started school and whether there were cases of exits and re-entries. As well known, answers on past events are reliable when such events are not too difficult to remember (the level of income in past years, for example, may be difficult to recall). Consider as well that also standard cross-sectional surveys data require a certain degree of memory from respondents (all questions refer to past events even though they occurred in the same year). From this perspective we argue that the assumption that producers remembered the age and schooling years of their children was not too demanding.[28] With this information we could reconstruct year by year (from 1987 to 2007) schooling decisions by the household and regress them on a set of controls which were time invariant during the panel period (gender, father's and mother's schooling years, participation in the treatment or in the control sample in one of the two projects), or whose variation could be reconstructed without problems (age, FT affiliation years).

[28] On the methodology for the construction of retrospective panel data with memorable events see (McIntosh, Villaran, and Wydick, 2007).

Once we had built the database we could calculate for each producer a time varying index based on the ratio of effective to potential schooling decisions (the latter being represented by the number of children in the schooling age cohort in the given year).

More formally, our household schooling index (HSI) is given by the following expression

$$HSI_{it} = \frac{\sum_{j=1}^{n_i} TOTSCH_{ijt} \left| Entryage_{ijt} \le Age_{ijt} \le Endage_{ijt} \right.}{\sum_{j=1}^{n_i} TOTPOTH_{ijt} \left| Entryage_{ijt} \le Age_{ijt} \le Endage_{ijt} \right.} \quad (1).$$

The HSI_{it} index is therefore the number of the j children of the i-th producer in the school age cohort[29] who actually went to school in a given period t ($TOTSCH_{ijt}$), divided by the number of children of the i-th producer in the school age cohort[30] in the same period ($TOTPOTH_{ijt}$).[31]

The index was regressed as the dependent variable in a fixed effect model on year dummies and years of FT affiliation (the effect of other variables such as respondent schooling years and age cohort were captured by fixed effects which also captured other non-measurable individual time-invariant characteristics).[32] The specification adopted is the following

$$HSI_{it} = \alpha_0 + \alpha_1 FTage_{it} + \sum b_t Year_t + u_i + v_{it} \quad [3]$$

where u_i is the fixed family effect and was estimated separately in the Chulucanas and Juliaca samples. The introduction of year dummies was important because the latter captured business cycle effects and country level institutional changes which are generally expected to increase schooling rates across years. Consider as well that, given the distribution of the affiliation variable (one third of FT producers had fewer than three years of affiliation, and half of them fewer than five years), omission of year effects would produce a downward bias on the effect of FT affiliation on the dependent variable.

[29] *Entryage* is five or six according to the respondent's declaration and *Endage* is 18 (17 when school entry was at 5), or below when we are interested in schooling decisions up to a threshold below 18.

[30] The school age cohort has a lower bound in *Entryage* (5 or 6 years according to the questionnaire declaration). The upper bound varies according to our investigation goals (we move it from 10 to 18 years according to different estimates).

[31] The total number of children for each farmer (n_i) was indexed to account for heterogeneity in household size.

The introduction of fixed effects also helped to control for all vintage factors which might create selection bias in the estimate. Consider that, in a parallel paper on Chilean Apicoop honey producers, Becchetti et al. (2008a) found that more recently affiliated producers were significantly more educated and had larger land size. Their empirical findings are consistent with historical evidence on the progressively tighter affiliation standards of that cooperative. This is not the case, however, of our two projects, where schooling years were not significantly different across producers in different affiliation cohorts. This is consistent with non-rejection of the null of independence of the two equations in the treatment regression model in our study and with the rejection of the same hypothesis in Becchetti et al.'s (2008a) Chilean study.

A well-known problem which may not have been fully overcome in our estimates is the omission of current income. If it is true that such an omission may generate an upward bias on the affiliation coefficient, it is also true that, in so far as affiliation has positive effects on income, as we showed in our previous estimates, part of the positive effect of income on schooling should be attributed to fair trade. Consider as well that factors incorporated into the fixed effects, such as schooling years and age cohort, are generally accepted as proxies for this unobserved variable.

Our estimates show that, in the Chulucanas case, FT affiliation significantly contributes to the schooling decision from 18 to 14 years (see Figure 1 and Table 6.1). More specifically, in the Chulucanas project each affiliation year increased the household schooling rate by 3 percent when our upper bound was 18 years. The effect declined as we lowered the upper bound but remained significant up to the 14 year threshold. The Chulucanas effect is clearly visible also from descriptive evidence:[33] the schooling rate for the 15-18 year cohort rises from 58 to 72 percent after FT affiliation. In the final row of the Table we document (F-test) that the null of a common effect in the two projects in a joint estimate is clearly rejected.

[32] We also estimated a specification in which we included producer's age and the total number of children in the school age cohort (denominator of our index of human capital investment at (1)). The results were substantially the same; they are omitted for reasons of space and available upon request.

[33] Omitted and available upon request.

In the Juliaca case the affiliation effect on the schooling index vanishes below the 16 threshold and its magnitude is smaller. Furthermore, for both areas we test the nested hypothesis of nonlinearity in the effect of schooling years by adding the square of affiliation years on equation [3]. The null hypothesis is not rejected. [34]

By considering the differences in standard of living between the two groups, the different schooling effects in the two projects seem consistent with the *luxury axiom hypothesis* stating that parents start sending children to school when they overcome a given threshold of household income (Basu, 1999; Basu and Van, 1998).[35] Our finding is also consistent with the difference between treatment and control samples in terms of standard of living: while farmers close to the subsistence level use large part of the premium to increase food expenditure,[36] better-off farmers can choose other destinations including human capital investment. This choice is also rational and consistent with returns on education in the two different areas, since the effect of (parents) schooling years on their income is much higher in the Chulucanas than in the Juliaca area (see Table 4.1, columns 2 and 3). To sum up, it is highly likely that relatively higher-income parents who experience higher returns on education are more willing to translate income effects into decisions to send their children to school.

4.3 Endogeneity and robustness check

We wondered whether the relationship between FT affiliation and child education may be affected by endogeneity. The absence of explicit child-labour ban rules in FT eases one part of the problem by reducing the possibility of reverse causality (i.e. a greater propensity to child education may cause affiliation). Furthermore, the finding of a progressive effect of affiliation over time seems to show that any additional year significantly reduces child labor. This progressive effect is less likely

[34] We estimated the specification with OLS and found that the main results were substantially unchanged. The only slight difference was that the positive effect of affiliation years in the Juliaca project was weaker. This might imply that such effect was detectable only once we had corrected for family fixed effects.

[35] More specifically, our results are compatible with a situation in which the FT effect shifts a significant part of the distribution of producers' income above the threshold in the Chulucanas project, while it is not enough to move large part of the same distribution above it in the Juliaca project.

to be interpreted in the reverse way and is consistent with the observed findings on the progressive effects of FT affiliation on producers' income.

As a further robustness check we verify whether our result is robust in a a dynamic system GMM estimate[37] in which the current value of the household schooling index (*HSI*) indicator was regressed on its one-period-lagged level, affiliation years and time dummies.[38]

We used two-step estimates with the Windmeijer (2005) correction, which enables two-step estimates to be better than one-step ones in estimating coefficients, with lower bias and standard errors. As well known, the GMM approach allows the introduction of endogenous or predetermined instruments together with strictly exogenous ones. We selected the subset of strictly exogenous instruments using the Davidson-McKinnon (1993) exogeneity test. We found that two- and three-period lagged affiliation values did not reject the null of strict exogeneity and therefore used these variables as exogenous regressors (Table 6.2). We also introduced the education years of the producer, of his father and mother as predetermined instruments.

The results presented in Table 6.2 show that the standard assumption needed to estimate a GMM equation (absence of second order autocorrelation of residuals) is supported by AR(2) diagnostics. The Sargan test indicates that the null of the joint validity of our instruments is not rejected.

The coefficient of the affiliation year is smaller in magnitude but strongly significant in determining changes in our dependent variable with respect to the previous period level. Again, the effect in the Chulucanas project persists across all the schooling age cohorts considered, while that on the Juliaca project is weaker and soon disappears.

To control for the possibility that the superior performance of affiliated producers depended on their ex ante superior skills, we tested whether in the preaffiliation period such producers performed better in terms of schooling decisions than the control sample. We did so by introducing a trend

[36] See on this point the narrow gap in the food expenditure share between treatment and control samples in the Juliaca vis-à-vis the much wider gap in the Chulucanas area in Table 3.
[37] For details on the GMM approach see Arellano-Bond (1991), Arellano-Bover (1995) and Blundell-Bond (1998).
[38] The selected specification is in Table 6.2.

variable multiplied by a (0/1) dummy for the future affiliated producers. The null of the lack of significance of this variable was not rejected in our estimates.

As a final robustness check we considered that the graduation of our treatment gave us an additional degree of freedom with respect to other conventional impact studies. We could therefore solve the problem of presumed heterogeneity between treatment and control sample by estimating the model for affiliated producers only. The results from this check substantially confirmed our findings (Table 6.3).

Since it might be objected that the HSI indicator has the limitations of a composite variable whose change does not depend only on schooling decisions but also on variations in the number of family children in the school age cohort, we checked whether our findings were robust when using as dependent variables:

i) the schooling/no schooling decision for each child-year observation;

ii) the education gap (a dynamic version of the Maldonado and Gonzalez-Vega, 2008 indicator): that is, the difference between the child' years of education achieved in a given year t and his/her expected level of education (according to age) in the same year. More formally, the expected level of child education (*Expected Education*$_t$) is then equal to *ChildAge*$_t$*-6* and, consequently, the education gap is defined as: *Education gap*$_t$ *= max{0, Expected Education*$_t$ *– Achieved Education*$_t$*}*.

We report in Table 7 the main findings from two estimates for each of the two indicators (the first on Chulucanas FT producers only and the second on Juliaca FT producers only).[39] Both estimates included the pre-affiliation trend variable. In this way we eliminated heterogeneity between treatment and control samples and checked whether the post-affiliation schooling performance was a follow-up of what had happened before affiliation. The schooling/no schooling decisions were

[39] Full estimate findings from these specifications (and from the specification which included control producers and did not contradict our main results) are omitted for reasons of space and are available upon request.

estimated with panel probit random effects, while the education gap index was estimated with panel fixed effects (at child level) and variance clustered for family and year.[40]

In all of the four estimates run on the overall schooling age period of the observed children we found that affiliation years significantly affected the schooling decision (significantly reduced the education gap) in the Chulucanas project, while they were not significant in the Juliaca project.

5. Conclusions

The empirical investigation on the impact of FT on apparel and pottery producers in two different areas of Peru involved in projects markedly different in terms of length of FT relationship and producers' standard of living in the area provides a rich set of empirical findings.

First, we observe a clear positive impact of FT affiliation years on producers' income, plus an additional effect determined by trade channel diversification which is mostly attributable to the availability of the new FT channel, and is much stronger in the poorer area, showing that trade diversification benefits may be very important for poorer producer groups.

We also note that comparative evidence on the two projects (very different in terms of average local standards of living) show surprisingly homogeneous returns on fair trade affiliation years amidst marked heterogeneity in terms of impact of education and age on producers' income.

Second, two kinds of external effects have been tentatively measured, providing empirical evidence for an important part of the debate on FT effects. On the one hand, we find that FT helps producers to improve their bargaining power with local intermediaries. Evidence on this point, even though only descriptive, is supported by two independent sources (comparison of prices with non-FT intermediaries for the treatment and control samples and producers' answers to a direct question on

[40] Note that the specification selected in the education gap estimate included child years as an additional regressor with respect to the estimate in Table 6.1. This variable is an important control since the education gap is expected to grow in

this issue). On the other hand, we find that, in one project but not in the other, the conditions of control producers in the area were improved by FT presence. This implies that LeClair's (2002) hypothesis is not rejected for the majority of control sample producers in only one of the two cases. An original aspect of the paper is that our test on the impact of FT affiliation controlled for the two externalities which may alter such comparison.

Third, we used a retrospective panel approach to develop a method which enabled evaluation of the effect of affiliation on schooling decisions. In this regard we found that, in the Chulucanas project, FT affiliation years had a significant and positive effect on producers' decisions to send their children to school. The effect was much weaker for Juliaca producers. This suggests that, consistently with the luxury axiom, the impact of fair trade has stronger effects on schooling decisions when producers are relatively better off. An interesting point is that producers' choices were consistent with the luxury axiom and the different returns on education in the two areas: where the latter were higher, the impact of FT affiliation years on schooling decisions was stronger.

In these concluding remarks we mention some policy considerations which stem from our results. First, descriptive evidence on the price premium, and FT externalities and the robustness of the impact on income and child schooling, document that FT may be a market-driven instrument with which to improve the economic well-being of producers by easing market access and enhancing capacity building, and because of its antitrust effects on the market power of local intermediaries.

Second, the weak Juliaca child-labour finding suggests that, in poorer socioeconomic environments close to the absolute poverty threshold, further progress is probably needed from the "scaling up" perspective by strengthening the link among fair trade, schooling decisions and wealth accumulation. To this end, it may be useful in the future to link FT intervention in poorer socioeconomic environments with complementary actions by international and domestic institutions on infrastructure (not easily affected by fair trade projects), and with schooling policies such as

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child years by construction.

conditional transfer programs which, jointly with fair trade, may help families to overcome the luxury axiom threshold that triggers schooling decisions.

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Table 1 Variable legend

Guantintcost	Price paid for a pair of standard wool gloves by local intermediaries in Juliaca
Guantminkacost	Price paid for a pair of standard wool gloves by Fair Trade importers in Juliaca
Guantturcost	Price paid for a pair of standard wool gloves by "responsible travelers" in Juliaca
Ftage	Number of affiliation years to Fair Trade (FT)
Ftdummy	Dummy variable taking value one if the producers is affiliated to FT and zero otherwise
Age	Producer's age
Female	Dummy variable taking value one if the producer is a woman and zero otherwise
Landsize	Land size in hectares
Selfprod	Dummy variable taking value one if the producer has self production activities
Numtradechan	Number of trading channels
Othincome	Dummy variable taking value one if the producer has more than one source of income and zero otherwise
Secactno	Dummy variable taking value one if the producer has not other productive activities besides the main one
Married	Dummy variable taking value one if the producer is married and zero otherwise
Divorced	Dummy variable taking value one if the producer is divorced and zero otherwise
Separated	Dummy variable taking value one if the producer is separated and zero otherwise
Schoolyears	Number of producer's schooling years
Numson	Number of producer's children
Wagefirstact	Producer's monthly income from the main activity
Houseprop	Dummy taking value one if the responder owns his house and zero otherwise
Lastsonvac	Dummy taking valueone if the last child has been vaccinated and zero otherwise
Total income	Producer's overall monthly income
Foodcons	Weekly food consumption expenditure
Compstandliv	Perceived standard of living with respect to the average standard of living in the area
Pricediftur	$[(P_T-P_{FT})/P_{FT}]$ Price differential on a standard pair of wool gloves between the price paid by socially responsible tourists (P_T) and that paid by fair trade importers (P_{FT}) in Juliaca.
Pricedifint	$[(P_{FT}-P_{LI})/P_{LI}]$ Price differential on a standard pair of wool gloves between the price paid by fair trade importers (P_{FT}) and that paid by local intermediaries (P_{LI}) in Juliaca.
Sinceminkaext	Dummy taking value one if the (FT affiliated) respondent declared that FT affiliation had improved his/her sale conditions also with local intermediaries
Improveminkarea	Dummy taking value one if the control sample respondent declared that FT affiliation had improved his/her well-being
Consshare	Share of food consumption in producer's total income
HSI (10-18)	Household schooling index (HSI) for the age cohort (10-18) given by the following expression $$HSI_{it} = \frac{\sum_{j=1}^{n_i} TOTSCH_{ijt} \left| Entryage_{ijt} \le Age_{ijt} \le Endage_{ijt} \right.}{\sum_{j=1}^{n_i} TOTPOTH_{ijt} \left| Entryage_{ijt} \le Age_{ijt} \le Endage_{ijt} \right.}$$ The HSI_{it} index is the number of the j children of the i-th producer in the school age cohort who actually went to school in a given period t ($TOTSCH_{ijt}$), divided by the number of children of the i-th producer in the school age cohort in the same period ($TOTPOTH_{ijt}$). The school age cohort has a lower bound in *Entryage* (5 or 6 years according to the questionnaire declaration). The upper bound (*Endage*) varies according to our investigation goals (from 10 to 18 years).
Extbargtreat	Product of the (0/1) affiliation dummy with a unit dummy for affiliated producers declaring that FT increased their bargaining power with local intermediaries.
Extcontr	Product of the (0/1) dummy of participation to the control group with a unit dummy for those control producers who declared improved conditions in the area after FT entry
juliaca	Dummy taking value one if the producer is in the Juliaca treatment or control sample
chulucanas	Dummy taking the value one if the producer is in the Chulucanas treatment or control sample

Table 2. Economic and socio-demographic characteristics

Variable	Obs.	Mean	Std. Dev.	Min	Max
Guantintcost	77	3.168	3.029	0.5	18
Female	241	0.352	0.354	0	1
Age	241	35.066	12.021	25	82
Schoolyears	241	7.780	3.628	0	20
Landsize	241	1.586	3.918	0	40
NumSon	241	2.473	2.309	0	10
Numtradechan	241	1.116	1.050	1	3
Foodcons	240	81.916	43.224	10	300
Wagefirstact	241	396.688	481.790	40	1400
Total income	240	436.875	499.593	53	2000
Consshare	240	0.732	2.143	.13	100
Houseprop	241	0.639	0.481	0	1
Trusttot	241	1.477	0.553	0	3
Lastysavtot	241	0.539	0.682	0	3
Lastsonvac	187	0.951	0.214	0	1
Standlivcomptot	241	1.854	0.826	0	4
Sinceminkaext	150	0.625	.4918	0	1
Improveminkarea	77	0.513	0.506	0	1
Pricediftur	77	0.498	0.3414	0.166	1.5
Pricedifint	23	4.261	3.352	-0.2	12

Variable legend: see Table 1

Table 3. Summary characteristics of the four farmer groups

Variable	Juliaca treatm.	Juliaca control	Chulucanas treatm.	Chulucanas control
Guantintcost	3.110 [2.09, 4.12]	2.068 [1.73, 2.40]		
Guantturcost	17.561 [16.62, 18.51]			
Guantminkacost	11.938 [11.25, 12.62]			
Age	40.26 [37.73, 42.80]	38.70 [34.14, 43.26]	28.59 [26.65, 30.52]	34.02 [30.70, 37.35]
Schooling years	6.48 [5.69, 7.26]	6.33 [5.03, 7.62]	9.46 [8.92, 10.00]	8.46 [7.23, 9.70]
FT affiliation years	14.94 [12.64, 17.23]		3.01 [2.24, 3.79]	
FT affiliation years (median)	13.12		3.53	
FT affiliation years (25th percentile)	4.21		1.52	
FT affiliation years (75th percentile)	22.63		4.21	
Land size	3.09 [2.08, 4.10]	1.16 [0.77, 1.54]	0.94 [0.11, 1.94]	0.12 [-0.02, 0.27]
N. of children	3.53 [2.98, 4.07]	3.22 [2.36, 4.09]	1.23 [0.89, 1.56]	2.12 [1.61, 2.63]
N. of trading channels	2.19 [2.07, 2.30]	1.02 [.97, 1.07]	2.37 [2.75, 2.99]	1.62 [1.25, 1.98]
Wage first activity	199.00 [178.21, 219.79]	50.00 [41.27, 58.73]	663.78 [519.26, 808.29]	599.51 [73.76, 725.27]
Food consumption	81.78 [76.30, 87.26]	50.37 [42.57, 58.18]	91.42 [81.56, 101.31]	95.62 [83.88, 107.34]
Home owner	0.74 [0.64, 0 .84]	0.50 [0.34, 0.66]	0.54 [0.42, 0.65]	0.78 [0.65, 0.91]
Consumption share	96.12 [94.02, 98.22]	99.45 [97.85, 101.05]	49.63 [45.61, 53.65]	61.36 [55.53, 67.20]
Savings last year	0.45 [0.31, 0.56]	0.30 [0.15, 0.45]	0.78 [0.62, 0.96]	.46. [0.24, 0.69]
HSI (6-18)	0.7650711 [0.740, 0.790]	0.7514489 [0.710, 0.793]	0.8872035 [0.852, 0.922]	0.8299883 [0.794, 0.866]
HSI (6-16)	0.792945 [0.770, 0.815]	0.8019406 [0.762, 0.841]	0.9176136 [0.886, 0.949]	0.8813549 [0.850, 0.913]
HSI (6-14)	0.8275862 [0.806, 0.849]	0.8483333 [0.813, 0.884]	0.9248366 [0.894, 0.966]	0.9173482 [0.890, 0.945]

Variable legend: see Table 1. * Percent values. 95 percent intervals based on standard errors of reported values are in square brackets.

Table 4.1 The impact of Fair Trade on producers' income (Variable legend: see Table 1)

	All sample	Juliaca	Chulucanas	All sample incl. externality effects
Ftage*chulucanas	.1049539		.0866008	.0969828
	(2.75)		(2.58)	(3.08)
Ftage*juliaca	.0876644	.0846428		.0830874
	(4.17)	(3.59)		(3.27)
Ftagesq*chulucanas	-.0052868		-.0044302	-.0049076
	(-1.78)		(-1.69)	(-2.71)
Ftagesq*juliaca	-.0022686	-.0021859		-.0021279
	(-3.23)	(-2.79)		(-2.56)
Age*chulucanas	.0316105*		.0319426	.032009
	(4.71)		(3.19)	(4.89)
Age*juliaca	-.0160549*	-.0078994		-.0152642
	(-2.89)	(-1.12)		(-2.37)
Landsize*chulucanas	-.0153184*		-.0177303	-.0141784
	(-1.03)		(-1.35)	(-2.32)
Landsize*juliaca	.0428828*	.0442772		.0407758
	(2.16)	(1.97)		(1.65)
Female	-.3035431	.12831	-.2449404	-.2977792
	(-2.34)	(0.47)	(-1.67)	(-2.26)
Selfprod	-.0586239	-.0409245	-.0512626	-.0615388
	(-2.77)	(-1.46)	(-1.45)	(-2.77)
Numtradechan*chulucanas	.0338808*		.0956906	-.0343358
	(0.46)		(1.42)	(-0.34)
Numtradechan*juliaca	.3204534*	.4060103		.3079457
	(3.00)	(3.27)		(2.17)
Othincome	-.1647883	-.3014154	-.0957288	-.191843
	(-1.11)	(-1.02)	(-0.61)	(-1.24)
Secactno	-.1865286	-.0449558	-.3616575	-.1977873
	(-1.81)	(-0.28)	(-2.81)	(-2.18)
Married	.222184	.210517	.2965529	.2035918
	(1.73)	(1.30)	(1.68)	(1.65)
Divorced	.5332345	.5138608	.8710459	.5749839
	(1.21)	(0.74)	(1.63)	(2.13)
Separated	.0451268	.3162442	-.1099242	.0387997
	(0.16)	(0.79)	(-0.29)	(0.18)
Schoolyears*chulucanas	.0649894*		.0337743	.0611229
	(3.71)		(1.84)	(0.11)
Schoolyears*juliaca	.0146627*	.0514608		.0160108
	(0.87)	(2.30)		(0.80)
Numson	-.0102875	.0296511	-.1089388	-.0115135
	(-0.36)	(0.81)	(-1.91)	(-0.36)
Extcontr*chulucanas				.3179139
				(1.54)
Excontr*juliaca				-.1522251
				(-0.64)
Extbargtreat*chulucanas				.4347917
				(1.31)
Extbargtreat*juliaca				.0029489
				(0.93)
Constant	4.883873	3.487274	5.417817	4.934907
	(17.26)	(6.06)	(15.10)	(17.20)
Number of obs.	238	119	119	238
R^2	0.7208	0.5013	0.4212	0.7251
F	28.01	7.47	5.41	30.59

* The null of the equality of coefficients in the two project is rejected

Table 4.2 Robustness check on the effects of the main variables on producer's income

	Specification column 1, Table 4.1			Specification column 4, Table 4.1		
	Affiliation years and number of trading channels in logs	Introduction of agesq	Number of trading channels in logs and Introduction of agesq	Affiliation years and number of trading channels in logs	Introduction of agesq	Number of trading channels in logs and Introduction of agesq
Ftage*chulucanas	0.10667	0.1042449	0.107322	0.0965094	0.0950217	0.0964272
	(3.76)	(3.58)	(3.68)	(3.03)	(2.84)	(2.84)
Ftage*juliaca	0.0889331	0.0839256	0.0781116	0.0860762	0.0770115	0.0727639
	(3.56)	(3.61)	(3.21)	(3.25)	(3.04)	(2.78)
Ftagesq*chulucanas	-0.0053971	-0.0054641	-0.0056791	-0.0048729	-0.0050653	-0.0051794
	(-3.14)	(-3.10)	(-3.24)	(-2.66)	(-2.70)	(-2.73)
Ftagesq*juliaca	-0.002297	-0.0022151	-0.0020717	-0.0022062	-0.0020086	-0.0019069
	(-2.85)	(-2.87)	(-2.62)	(-2.60)	(-2.44)	(-2.28)
Age*chulucanas	0.0343661	0.1170672	0.1292195	0.0345856	0.1169871	0.1290419
	(5.00)	(4.93)	(5.28)	(5.08)	(4.77)	(5.03)
Age*juliaca	-0.0171027	0.0079172	0.0021837	-0.0159469	0.0071563	0.001602
	(-2.70)	(0.43)	(0.12)	(-2.41)	(0.38)	(0.09)
Agesq*chulucanas		-0.0013944	-0.0015438		-0.0013878	-0.0015318
		(-4.03)	(-4.29)		(-3.85)	(-4.04)
Agesq*juliaca		-0.0002023	-0.0001442		-0.0001901	-0.0001346
		(-0.98)	(-0.70)		(-0.91)	(-0.64)
Numtradechan*chulucanas		0.0826615			0.0247262	
		(1.11)			(0.24)	
Numtradechan*juliaca		0.3640448			0.3882935	
		(3.02)			(2.72)	
Log(Numtradechan*chulucanas)	0.0256178		0.1139701	-0.1267746		-0.0142868
	(0.17)		(-0.80)	(-0.64)		(-0.07)
Log(Numtradechan*juliaca)	0.7703688		1.091909	0.6830511		1.116648
	(2.23)		(3.15)	(1.84)		(2.82)

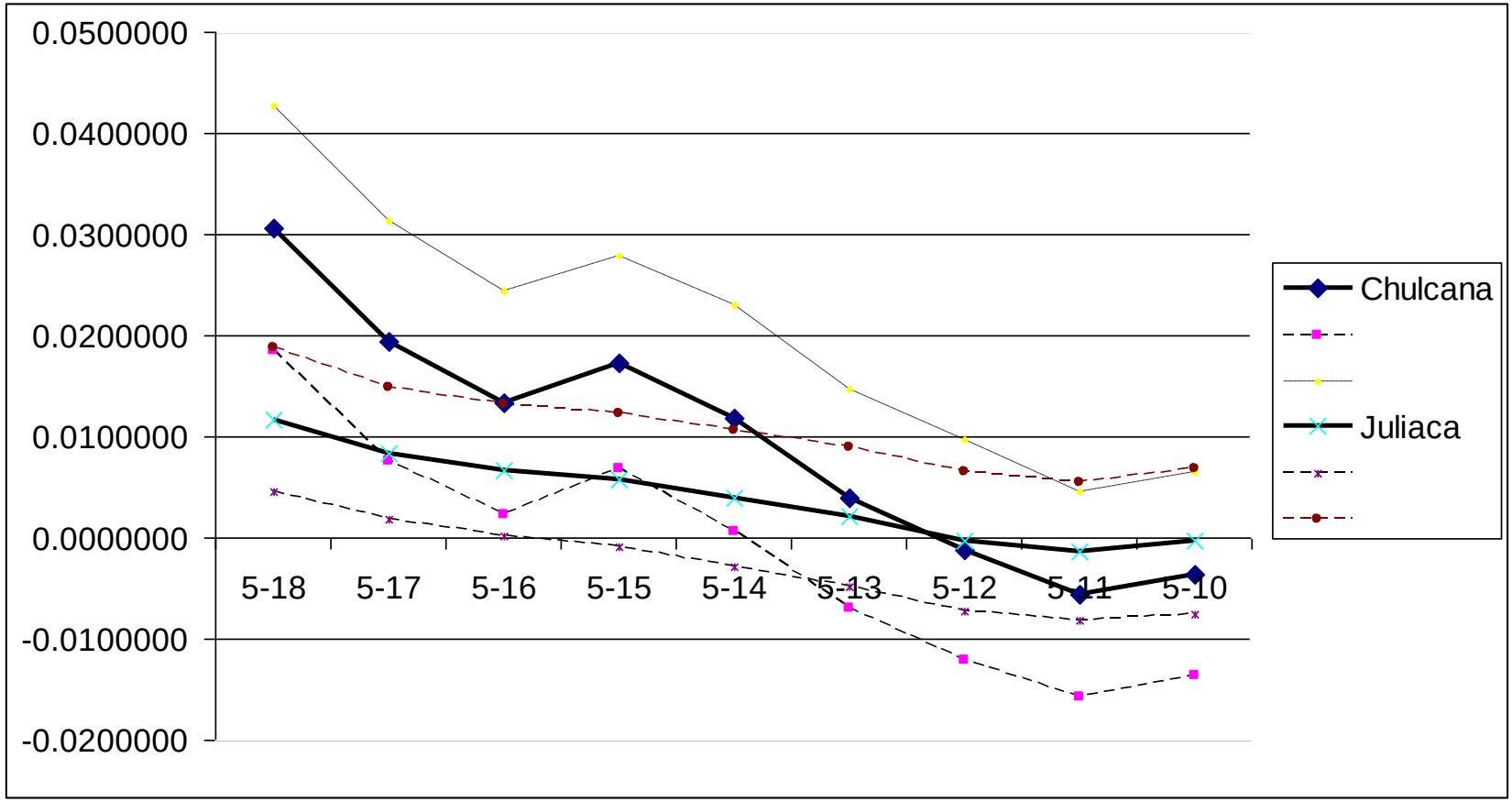
Variable legend: see Table 1.

Table 5. The impact of FT affiliation after controlling for selection bias

	SINGLE EQUATION MODEL	TREATMENT REGRESSION MODEL		
	BASE EQUATION	BASE EQUATION	SELECTION EQUATION	
Dep. Var.	*Wage from the first activity*	*Wage from the first activity*	*Ftdummy*	
Age*chulucanas	0.0397038	0.0395402	**Othincome**	0.2074397
	(7.33)	(7.10)		(0.21)
Age*juliaca	-0.0123016	-0.0124286	**Married**	0.2651101
	(-2.42)	(-2.40)		(0.46)
Landsize*chulucanas	-0.0116502	-0.0117116	**Numson**	-0.0604774
	(-0.85)	(-0.87)		(-0.58)
Landsize*juliaca	0.0313778	0.0316279	**Constant**	0.4326107
	(2.07)	(2.11)		(2.88)
Selfprod	-0.0525334	-0.0523343		
	(-2.71)	(-2.75)		
Secactno	-0.2311133	-0.2321158		
	(-2.48)	(-2.51)		
Schoolyears*chulucanas	0.0577156	0.058594		
	(3.69)	(3.46)		
Schoolyears*juliaca	0.017833	0.0180901		
	(1.20)	(1.25)		
Ft*juliaca	0.9588636	0.9639816		
	(5.10)	(4.96)		
Ftage*chulucanas	0.0341308	0.0338659		
	(1.87)	(1.87)		
Ftage*juliaca	0.0027948	0.0026021		
	(0.38)	(0.35)		
Ftdummy	0.2460886	0.4772368		
	(2.00)	(0.29)		
Constant	4.60387	4.441554		
	(19.53)	(3.83)		
LR* :		$\chi2(1)$ = 0.01 Prob > $\chi2$ = 0.9404		
Number of obs.		238		
Log likelihood		-344.73131 (0.00)		

*Test of the independence of the two equations (H_0: ρ=0, or no correlation between the residuals of the two equations).

Figure 1. The net effect of one year of FT affiliation on the household human capital investment rate



Legend: effect of FT affiliation years (ftage) on the Household Schooling Index in a fixed effect estimate. The net effect is on the vertical axis, the age cohort considered on the horizontal axis. The Household schooling index (HSI) is given by the following expression

$$HSI_{it} = \frac{\sum_{j=1}^{n_i} TOTSCH_{ijt} \left| Entryage_{ijt} \le Age_{ijt} \le Endage_{ijt} \right.}{\sum_{j=1}^{n_i} TOTPOTH_{ijt} \left| Entryage_{ijt} \le Age_{ijt} \le Endage_{ijt} \right.}$$

where the HSI_{it} index is the number of the j children of the i-th producer in the school age cohort ($6 \le Age_{ijt} \le 18$) who actually went to school in a given period t ($TOTSCH_{ijt}$), divided by the number of children of the i-th producer in the school age cohort in the same period ($TOTPOTH_{ijt}$). The school age cohort has a lower bound in *Entryage* (5 or 6 years according to the questionnaire declaration). The upper bound (*Endage*) varies according to our investigation goals (we move it from 10 to 16 years according to different estimates). The sample period is from 1987 to 2007.

Table 6.1 The effect of Fair Trade on the household schooling index (children from the 5-18 to the 5-10 age cohort)

	5-18 SCHOOL AGE COHORT		5-17 SCHOOL AGE COHORT		5-16 SCHOOL AGE COHORT	
	JULIACA	CHULUCANAS	JULIACA	CHULUCANAS	JULIACA	CHULUCANAS
Ftage	0.0116695	0.0305899	0.0083405	0.0194684	0.0067047	0.0133189
	(3.22)	(4.96)	(2.50)	(3.20)	(2.03)	(2.36)
Number of obs.	1100	551	1085	547	1069	542
F	27.69	21.19	31.78	17.21	30.50	14.44
R^2 within	0.3590	0.4746	0.3949	0.4254	0.3891	0.3857
R^2 between	0.1034	0.0110	0.0962	0.0127	0.0493	0.0040
R^2 overall	0.0637	0.1625	0.0892	0.1431	0.1092	0.1590
F(2)*	12.27		11.33		14.85	
Prob>F	(0.00)		(0.00)		(0.00)	

	5-15 SCHOOL AGE COHORT		5-14 SCHOOL AGE COHORT		5-13 SCHOOL AGE COHORT	
	JULIACA	CHULUCANAS	JULIACA	CHULUCANAS	JULIACA	CHULUCANAS
Ftage	0.0056836	0.017305	0.0039123	0.0118497	0.0020684	0.0039205
	(1.70)	(3.22)	(1.14)	(2.08)	(0.59)	(0.71)
Number of obs.	1053	534	1031	524	1004	510
F	29.46	12.84	25.70	10.06	21.92	9.35
R^2 within	0.3843	0.3622	0.3579	0.3129	0.3287	0.3041
R^2 between	0.0618	0.0000	0.0447	0.0001	0.0277	0.0015
R^2 overall	0.1338	0.1680	0.1555	0.1608	0.1834	0.1565
F(2)*	21.67		14.71		9.09	
Prob>F	(0.00)		(0.00)		(0.00)	

	5-12 SCHOOL AGE COHORT		5-11 SCHOOL AGE COHORT		5-10 SCHOOL AGE COHORT	
	JULIACA	CHULUCANAS	JULIACA	CHULUCANAS	JULIACA	CHULUCANAS
Ftage	-0.0003217	-0.0011801	-0.0014203	-0.0055577	-0.0003434	-0.0035976
	(-0.09)	(-0.21)	(-0.41)	(-1.08)	(-0.09)	(-0.70)
Number of obs.	973	490	932	468	870	440
F	21.06	9.98	18.87	9.08	15.16	6.66
R^2 within	0.3277	0.3285	0.3141	0.3200	0.2847	0.2713
R^2 between	0.0122	0.0029	0.0030	0.0078	0.0031	0.0044
R^2 overall	0.2464	0.1631	0.2618	0.1218	0.2262	0.0895
F(2)*	3.37		2.45		3	
Prob>F	(0.06)		(0.11)		(0.08)	

Legend: effect of FT affiliation years (ftage) on the Household Schooling index (HSI) in a fixed effect estimate (for the definition of the dependent variable see Table 1 legend and Figure 1). 5-X school age cohort: the school age cohort considered to build the Household Schooling Index is from year 5 to year X.

F(2)*: H_0= equality of separate *Ftage* coefficients for the two projects in a joint estimate including observations from both.

Table 6.2 Robustness check: GMM estimates on the effects of FT affiliation on the Household Schooling Index

	5-18 SCHOOL AGE COHORT	5-17 SCHOOL AGE COHORT	5-16 SCHOOL AGE COHORT	5-15 SCHOOL AGE COHORT	5-14 SCHOOL AGE COHORT
$HSI_{i,t-1}$	.6011263	.3960709	.161936	.1516641	.1501028
	(3.36)	(2.26)	(0.80)	(0.59)	(0.80)
$Ftage_{i,t-1} * JULI$	.0041497	-.0019912	-.0047242	-.0040212	-.0035324
	(2.73)	(-0.98)	(-0.39)	(-1.06)	(-1.26)
$Ftage_{i,t-1} * CHULC$	.0112327	.0148574	.0125056	.0133918	.0109378
	(1.99)	(2.90)	(3.49)	(2.64)	(3.49)
Number of obs.	1527	1506	1486	1463	1427
AR(1) test	-3.70	-3.48	-2.20	-1.79	-2.47
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
AR(2) test	1.49	1.52	0.53	0.78	1.18
	(0.32)	(0.30)	(0.59)	(0.43)	(0.24)
Sargan test	50.64	43.40	0.53	37.08	63.18
	(0.48)	(0.54)	(0.59)	(0.32)	(0.12)
Davidson-McKinnon	1.601271	1.855	1.92	1.74	1.65
exogeneity test	(0.216)	(0.149)	(0.09)	(0.122)	(0.192)
Test on common preaffiliation trends – Juliaca*	0.01	0.03	0.02	0.08	1.05
	(0.58)	(0.24)	(0.94)	(1.03)	(124)
Test on common preaffiliation trends – Chulucanas*	0.002	-0.001	0.002	-0.002	-0.003
	(0.31)	(0.30)	(0.82)	(0.73)	(0.84)
F	23.98	19.74	14.30	13.09	9.30
Prob>F	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)

The base specification is: $HSI_{it} = \alpha_0 + \alpha_1 HSI_{i,t-1} + \alpha_2 Age_{i,t-1} + \alpha_3 Ftage_{i,t-1} * JULI + \alpha_4 Ftage_{i,t-1} * CHULC + \sum_{k=t_0}^{T} \beta_k Dtime_k + u_i$

where HSI_{it} is the household schooling index of the i-th producer in the year t (for the definition of the index see Table 1 legend), *Age* is producer's age, *Ftage* is the number of affiliation years, *Dtime* are year dummies, *CHULC* and *JULI* are (0\1) dummies for producer's location in the Chulucanas and in the Juliaca project respectively. 5-X school age cohort: the school age cohort considered to build the Household Schooling Index is from year 5 to year X. The equation is estimated with a system GMM model with two-step coefficients and Windmejier (2005) correction to obtain unbiased standard errors. Variables used to build endogenous or predetermined (GMM) instruments were schoolyears of the producer and his/her mother and father. Variables used to build strictly exogenous instruments were two- and three-period lagged affiliation years. Time dummies coefficients are omitted and available upon request. The Sargan statistic is distributed as a χ^2 under the null of instrument validity. AR(1) and AR(2) are tests for first- and second-order serial correlation in the residuals, asymptotically distributed as a N(0,1) under the null of instrument validity. The Davidson-McKinnon statistic is distributed as an F under the null of orthogonality of the set of strictly exogenous instruments to the error term of the base estimate.* We estimated the model in the subsample of the control group and the treatment group before affiliation. We introduced a variable in which a linear trend was multiplied for the treatment group dummy and tested whether the latter was significantly different from zero. The table reports the coefficients and the t-statistics. The test was performed on separate estimates for the two projects.

Table 6.3 Robustness check: GMM estimates on the effects of FT affiliation on the Household Schooling Index (treatment sample only)

	5-18 SCHOOL AGE COHORT	5-17 SCHOOL AGE COHORT	5-16 SCHOOL AGE COHORT	5-15 SCHOOL AGE COHORT	5-14 SCHOOL AGE COHORT
$HSI_{i,t-1}$	.3549	.1422	.1899	.2655	.1602
	(2.10)	(1.00)	(1.14)	(1.40)	(2.03)
$Ftage_{i,t-1} * JULI$	.0023	.0019912	-.0047242	-.0025	-.0035324
	(2.03)	(0.98)	(-0.39)	(-0.66)	(-1.26)
$Ftage_{i,t-1} * CHULC$	.0125	.0136	.0156	.0124	.0998
	(1.69)	(2.32)	(2.32)	(2.14)	(2.15)
Number of obs.	769	758	748	736	714
AR(1) test	-3.20	-2.96	-2.48	-2.59	-3.21
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
AR(2) test	2.09	-1.24	0.16	1.13	1.18
	(0.04)	(0.21)	(0.87)	(0.25)	(0.23)
Sargan test	59.14	43.40	40.53	37.08	63.18
	(0.24)	(0.44)	(0.49)	(0.62)	(0.12)
Davidson-McKinnon	1.0324	1.523	1.71	1.94	1.60
exogeneity test	(0.313)	(0.349)	(0.128)	(0.092)	(0.199)
F	30.37	12.74	11.28	16.27	13.38
Prob>F	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)

The base specification is: $HSI_{it} = \alpha_0 + \alpha_1 HSI_{i,t-1} + \alpha_2 Age_{i,t-1} + \alpha_3 Ftage_{i,t-1} * JULI + \alpha_4 Ftage_{i,t-1} * CHULC + \sum_{k=t_0}^{T} \beta_k Dtime_k + u_i$

where HSI_{it} is the household schooling index of the i-th producer in the year t (for the definition of the index see Table 1 legend), *Age* is producer's age, *Ftage* is the number of affiliation years, *Dtime* are year dummies, *CHULC* and *JULI* are (0\1) dummies for producer's location in the Chulucanas and in the Juliaca project respectively. 5-X school age cohort: the school age cohort considered to build the Household Schooling Index is from year 5 to year X. The equation was estimated with a system GMM model with two-step coefficients and Windmejier (2005) correction to obtain unbiased standard errors. Variables used to build endogenous or predetermined (GMM) instruments were schoolyears of the producer and his/her mother and father. Variables used to build strictly exogenous instruments were two- and three-period lagged affiliation years. Time dummies coefficients are omitted and available upon request. The Sargan statistic is distributed as a χ^2 under the null of instrument validity. AR(1) and AR(2) are tests for first- and second-order serial correlation in the residuals, asymptotically distributed as a N(0,1) under the null of instrument validity. The Davidson-McKinnon statistic is distributed as an F under the null of orthogonality of the set of strictly exogenous instruments to the error term of the base estimate.

Table 7. Robustness check on the effect of FT affiliation on schooling decisions in Chulucanas and Juliaca

	(0/1) SCHOOLING DECISION		EDUCATION GAP	
	JULIACA FT PRODUCERS ONLY	CHULUCANAS FT PRODUCERS ONLY	JULIACA FT PRODUCERS ONLY	CHULUCANAS FT PRODUCERS ONLY
$Ftage_{i,t}$	-.003	.1126	1.11	-.0591
	(0.61)	(2.27)	(0.41)	(-2.73)
YearsbeforeFTaff	0.38	-0.0014	.061	.0185
	(1.00)	(-0.01)	(0.84)	(0.74)
Fixed effects			YES	YES
Year dummies	YES	YES	YES	YES
Number of obs.	1,992	758	2,169	524
F (Prob>F)			69.49 (0.00)	17.77 (0.00)
Wald χ^2(p. value)	260.32(0.00)	260.32(0.00)		
R^2 within	0.48	0.47		

Columns 1 and 2 report effects of FT affiliation (*Ftage*) on the schooling/no schooling decision for each child-year observation. The selected random effect probit specification is

$$School_{ijt} = \alpha_0 + \alpha_1\, Childage_{ijt} + \alpha_2\, FTage_{it} + \alpha_3\, YearsbeforeFTaff_{ijt} + \alpha_4\, Numson_{it} + \alpha_5\, Fatherbirthyear_i + \alpha_6\, Fathereducation_{it} + \sum b_t Year_t + u_i$$

where *Childage* is the child's age, *YearsbeforeFTaff* is the number of years which precede producer affiliation, *Fatherbirthyear* is the year of birth of the producer and *Fathereducation* his years of school. In column one the model is estimated on Juliaca FT producers only, and in column 2 on Chulucanas FT producers only.

Columns 3 and 4 report effects of FT affiliation on the education gap, the difference between the child' years of education achieved in a given year t and his/her expected level of education (according to age) in the same year. More formally, the expected level of child education (*Expected Education*$_t$) is then equal to *ChildAge*$_t$*-6* and, consequently, the education gap is defined as: *Education gap*$_t$ *= max{0, Expected Education*$_t$ *– Achieved Education*$_t$*}*.

The selected panel fixed effect specification is

$$Educationgap_{ijt} = \alpha_0 + \alpha_1\, Childage_{ijt} + \alpha_2\, FTage_{it} + \alpha_r\, YearsbeforeFTaff_{ijt} + \sum b_t Year_t + u_j + v_{ijt}$$

standard errors ar clustered for family and years.

Appendix. Survey design

The first step in the research was to create two lists of affiliated (treatment sample) and non-affiliated (control sample) producers with the same work activity. The list included 30 percent more individuals than our target of participants in each of the two groups in order to allow for random selection and to compensate for non-responses. As usually happens, the construction of the control sample turned out to be more difficult, while the list for the treatment sample could be drawn automatically from the list of members usually available from the associations affiliated to Fair Trade (in our case Minka and Allpa producer groups in Juliaca and Chulucanas respectively).
More specifically, we interviewed in Juliaca artisans from the 14 comunidades campesinas closest to Juliaca (Unocolla, Ccota, Cochaquinray, Pucachupa I, Pucachupa II, Rancho Sollata, Tacamani, El Inti, Huayna Roque, Corisuyo, San Pablo, Antipampilla, Cochapata, Ccorpa). As control producers we randomly chose artisans living in the same comunidades but who did not have fair trade relationships. With regard to the Chulucanas (ALLPA) project, all the artisans interviewed (control and target group) worked and lived in the small village of Chulucanas.
Additional information was taken from the cooperative on survivorship in the years of interest and selection criteria (presence of explicit and implicit membership selection rules) before we created our lists. The negligible amount of exits due to misperformance made us confident about survivorship bias. Apart from the work activity there were no other entry selection standards besides reference to maximum quantitative limits of production imposed by market demand and by the two organisations. We controlled that the control group producers were also eligible (had the minimum quantitative of production required to enter the FT affiliated groups) in order to avoid heterogeneity and selection bias.

Implicit selection bias was partially taken into account with econometric techniques, as shown in section 4.2. Since we did not have observations repeated in time on the same individuals, we could not use techniques such as propensity score matching or tests on significant breaks between preformation and postformation performance trends. We therefore used the treatment regression approach, which can also be applied to cross-sectional data.
The survey questions were defined on the basis of the World Bank *Living Standard Measurement Studies* (LSMS) (accessed on 1 September 2009 at http://econ.worldbank.org/WBSITE/EXTERNAL/EXTDEC/EXTRESEARCH/EXTLSMS/0,,contentMDK:21610833~pagePK:64168427~piPK:64168435~theSitePK:3358997,00.html.) and adapted to the circumstances of our research with a specific set of questions on affiliation characteristics, price conditions on different sale channels, and relationship with the affiliated organisations.