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Abstract

The paper studies the effects of *Familias en Acción*, a conditional cash transfer programme implemented in rural areas in Colombia in 2002, on school enrolment and child labour. Using a quasi-experimental approach, our methodology makes use of an interesting feature of the data, which allows us to identify anticipation effects. Our results show that the programme increased school participation of 14 to 17 year old children quite substantially, by between 5 and 7 percentage points, and had lower, but non-negligible effects on enrolment of younger children of between around 1.5 and 2.5 percentage points. In terms of work, the effects are generally largest for younger children whose participation in domestic work decreased by around 10 to 12 percentage points after the programme but whose participation in income-generating work remained largely unaffected by the programme. We also find evidence of school and work time not being fully substitutable, suggesting that some, but not all, of the increased time at school may be drawn from children's leisure time.

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1. Introduction

Conditional Cash Transfer (CCT) programmes, aimed at fostering the accumulation of human capital among children living in indigent households, have attracted much attention in recent years. After the widely acclaimed success of the Mexican CCT programme PROGRESA (now known as *Oportunidades*), international financial institutions, along with governments in many developing countries, have adopted similar schemes. Different versions of CCT programmes now exist in a wide variety of countries, such as Nicaragua, Honduras, Brazil, Argentina, Ecuador, and Turkey. In this paper, we evaluate the effect on school enrolment and child labour of a CCT programme called *Familias en Acción (FA)*, which has been operating in parts of Colombia since 2002. In early evaluation reports of this programme (see Attanasio et al., 2004, 2005), we focussed on the effects of the programme on school enrolment. In this paper, we both expand those results, by analysing anticipation effects along with other methodological issues, and complement them with an analysis of child labour, both income-generating and domestic activities. This child labour analysis draws on the rich time use module of the surveys that has not hitherto been analysed.

Although conditional cash transfer programmes are by now routinely implemented in many developing countries, the first such programme that generated widespread evaluation - the Mexican Progresa programme - is still relatively new, having started in 1998 (Schultz, 2004). A number of similar programmes have since been evaluated and all paint the same picture of positive impacts on school enrolment (for a review see Rawlings and Rubio, 2005). Our paper corroborates this existing evidence for Colombia.

However, as noted above, we not only consider education choices, but also the effects of a CCT programme in Colombia on child labour. This is because the analysis of school enrolment says little about the effects of the programme on child labour: whilst time spent at work and school are likely to be substitutable, they are by no means perfectly so. So even if the programme has increased school enrolment, this may not have led to a reduction in child labour of the same magnitude. This is particularly relevant in countries where school classes do not necessarily extend over the full but rather over one half of the day, as in Colombia. The evidence on this issue is less

pervasive however, with the exceptions of Skoufias and Parker (2001) and Rubio (2002), who find positive impacts of the PROGRESA programme on time spent at school and negative impacts on child labour.[1]

Our methodology differs from these studies insofar as we use a quasi-experimental approach to identify the effects of the programme on school and labour outcomes. This is because unlike PROGRESA, *Familias en Acción* was not randomly assigned across localities, for political reasons. Our evaluation methodology involves comparing outcomes of interest in areas in which the programme was not implemented (control) to those in which it was (treated). We condition on a large range of household and municipality level characteristics, and also control for pre-programme differences in the particular outcome of interest using a difference-in-difference methodology. We are successful at finding suitable control individuals for the majority of treated individuals in this quasi-experimental set-up. This is in large part due to the considerable effort that was put into choosing the control areas in the design stage of the evaluation so as to ensure that they were as similar as possible to the treated ones.

Furthermore, in contrast to previous studies[2], we observe detailed data on the amount of time spent in various activities over *two* periods - before and after the programme starts. This offers us the scope to control for pre-programme differences between control and treated areas in our analysis of time uses and hence, to reduce possible biases that could affect such estimates.

We find that the programme increased the school participation rates of 14 to 17 year old children quite substantially, by between 5 and 7 percentage points. It had lower, but non-negligible effects on the enrolment of younger children of between around 1.5 and 2.5 percentage points. This is not surprising given the already high school enrolment rates of these groups. In terms of work, the effects are generally largest for younger children whose participation in domestic work decreased by around 10 to 12

[1]Skoufias and Parker (2001) show that for boys, the reductions in work participation are approximately equivalent to the increases in school participation, whereas for girls, the reductions in work participation tend to be lower than the increases in school participation.

[2] The following remark holds only for the analysis on time use as, for school and work participation, the survey for the evaluation of the Mexican programme PROGRESA contains several rounds of data that are used by Skoufias and Parker (2002) in a difference-in-difference strategy.

percentage points after the programme but whose participation in income-generating work remained largely unaffected by the programme. We also find evidence of school and work time being less than fully substitutable, suggesting that some, but not all, of the increased time at school may be drawn from children's leisure time. In this sense, our findings resemble those of Ravallion and Wodon (2000) in their analysis of the Food-for-Education programme in Bangladesh. However, our time use data allow us to analyse the impacts of the programme on schooling and child labour substitution at both the extensive and intensive margins, in contrast to their study that considers only binary indicators for school and work participation.

Furthermore, our estimates of the effects of the programme on school enrolment take into account possible anticipation effects, which is distinct from the existing literature on this. We are able to identify anticipation effects as we observe school enrolment rates for some treated areas for each of *two* years before the programme started. Whilst there is always the concern that enrolment just before a programme is implemented is already affected by individuals anticipating the subsidy, thus contaminating the counterfactual, this is less of a concern for enrolment two years before the programme starts.

It turns out that anticipation effects are significant but not very large. Whilst of course this cannot be generalised to similar programmes in other contexts, it is at least reassuring, as evidence of strong anticipation effects would not bode well for studies that do not have the data to identify them separately.

The paper proceeds as follows. In section 2, we describe the programme and the context of the rural Colombian communities in which it was implemented. Section 3 provides a discussion of the evaluation survey as well as some descriptive statistics relating to school enrolment and time use before the programme started. In section 4 we present our results: we first provide results for school enrolment, and then go on to show the results relating to time allocation between various work activities, and school. Section 5 concludes.

2. The Familias en Acción programme

The *Familias en Acción* welfare programme is aimed at alleviating poverty by fostering human capital accumulation among the poorest households in Colombia. Modelled on the Mexican PROGRESA (now called *Oportunidades*), it consists of conditional subsidies for investments into education, nutrition and health. Such interventions are typically justified either by positive externalities that human capital might confer, or by the existence of liquidity constraints. Whilst the former could justify making the transfer conditional, i.e. paid only if the household complies with certain conditions, the latter does not: if liquidity constraints are the reason for non-attendance to begin with, an unconditional transfer targeted to poor households will be sufficient to overcome this market failure. Other general reasons for conditionality include excessive discounting of the future utility of children on the part of the parents, myopia and the necessity or desire to increase the resources invested in children.

The largest component of the programme is the education one, which is targeted at families with children aged 7 to 17. Subsidies, paid to the mother of the child(ren), are granted conditional on the child(ren) attending at least 80% of school classes. The amounts of the subsidy vary by school level, being 14,000 pesos (US$6.15) and 28,000 pesos (US$12.30) for children attending primary and secondary school respectively.[3] Making the grant conditional on school attendance effectively decreases the relative price of education. Typically the level of the grant is chosen so as to substitute, at least in part, the income the household would forego if increased schooling came at the expense of reductions in income-generating activities. It should be noted however, that for households that would have sent their child(ren) to school on a regular basis anyway, the change in relative price will not matter and the grant is effectively an unconditional transfer that increases household income without altering any relative prices. However, it might still bring about changes in household

[3] These are the amounts in 2002, with an average annual exchange rate of $US1=2,275 Colombian pesos. In contrast to the PROGRESA programme, the education subsidy does not vary by gender. In contrast to Mexico however, if any gender differences do exist in school enrolment in Colombia, they in fact favour girls (see Table 3).

behaviour, not only due to the increase in income but also due to this additional income being managed by a female member of the household.[4]

The other component of the programme is the nutrition subsidy. A flat-rate monthly monetary supplement of 46,500 pesos (approximately US$20.45) is provided to mothers of all beneficiary families with children aged 0 through 6. Its receipt is conditional on fulfilling certain health care requirements including vaccinations and growth and development check-ups for children, and attendance at courses on nutrition, hygiene and contraception by the children's mothers.

The targeting of the programme took place in two stages. The first was geographic. A subset of 622 of the 1,060 Colombian municipalities was identified as qualifying for the programme. The conditions that had to be fulfilled for a municipality to qualify were: (i) that the town has less than 100,000 inhabitants and is not a departmental capital; (ii) that it has sufficient education and health infrastructure; (iii) that it has a bank and (iv) that the municipality administrative office has relatively up-to-date welfare lists and other official documents deemed to be important.

The second stage was to identify eligible households in qualifying towns. Eligibility was established on the basis of a six-level welfare indicator, SISBEN. The SISBEN rank is determined from the first principal component of a number of variables that are related to poverty. This indicator has been used in Colombia to target all previous welfare programmes as well as for the pricing of utilities. This indicator is, in theory, updated regularly, having been last updated at the end of December 1999. FA was targeted to households registered as SISBEN level 1, living in target municipalities, and with children less than age 17. Note that SISBEN 1 households essentially account for the lowest quintile of the household income distribution.

The programme was funded by a loan from the World Bank and Inter-American Development Bank (IADB) to the Colombian government in 2000, to cover the costs of running of the programme for three years. The programme started operating in 2001 or 2002, depending on the municipality. The sequential phasing in had important

[4] The perceived importance of the intra-household mechanism is implicit in the fact that most CCTs are paid to mothers.

implications for the evaluation methodology, which we discuss in section 4. In the first two years of the programme, 340,000 households were registered to participate. More recently the programme has been expanded to another 60,000 households and is currently being piloted in deprived urban areas.

3. Data

In this section we describe the survey and present some descriptive statistics relating to our sample. First, we provide evidence that treatment and control areas are similar along an extensive range of observed household and village characteristics.[5] Second, we show trends in enrolment for three years – two of which are pre-programme for one set of treatment areas, and one of which is pre-programme for the other set of treatment areas (we return to this below). This not only gives a flavour as to how enrolment rates vary across areas, but also alerts one to possible anticipation effects of the programme. We then move on to compare work participation and time allocation across treatment and control areas at the baseline (first survey) and follow-up (second survey). Finally, we take a look at socio-economic determinants of education and work choices in the absence of the programme.

3.1 Data collection

In December 2001, a consortium formed by the Institute for Fiscal Studies and partners in Colombia - a research institute (Econometria) and a data collection firm (SEI) - began to work on the evaluation of the programme. While it was hoped to randomly allocate the programme across a small set of municipalities during the first two years of its implementation, this strategy turned out to be politically infeasible. Instead, it was decided to construct a representative stratified sample of treatment municipalities and to choose control municipalities among those that were excluded from the programme but that belonged to the same strata. The 25 strata were determined by region and an index of infrastructure relating to health and education. The control towns were chosen, within the same stratum, to be as similar as possible to each of the treatment towns in terms of population, area and an index of quality of

[5] As mentioned already, considerable emphasis was placed on the choice of control groups in the design stage of the programme.

life. Most control areas satisfied most of the criteria for eligibility, with the exception of the presence of a bank. In the end, the evaluation sample was made up of 122 municipalities, 57 of which were treatment and 65 of which were controls.

In each village we randomly sampled approximately 100 eligible households for inclusion in the evaluation sample. We ended up with a sample of around 11,500 households who were interviewed between June and October 2002. Owing to a large effort in tracking the households in the second wave, between July and November 2003 (the second wave) we succeeded in re-contacting and obtaining complete interviews from 10,742 households, representing around 94% of the original sample. A third survey has just been completed in April 2006.

The original intention was for the first data collection to take place *before* the programme started in the treatment municipalities. The availability of a baseline survey was deemed important to control for any systematic pre-programme differences between treatment and control towns. Unfortunately, political pressure resulted in the programme starting in some municipalities before we were in a position to collect any data. Therefore in 2002, there exists a group of treatment municipalities in which the programme had already started and another group in which it had not. In particular, in 26 of the 57 treatment municipalities the programme had already started by the time of the first data collection. In what follows we label as TCP (*tratamiento con pago:* treatment with payment) the municipalities where the programme started early and the remainder as TSP (*tratamiento sin pago:* treatment without payment).[6] This means that both of the available surveys in TCP towns relate to a period during which the programme was underway; in TSP areas on the other hand, it was underway by the time of the second survey only. This sequential phasing in of the programme across pilot areas brings with it considerable identification merits, discussed more fully in section 4. For now it should be stressed that the sample of households in TSP towns was aware of the programme and moreover, some had even registered for it, but of course were not receiving any payments. As we discuss below, it is not unreasonable to expect them to have changed their behaviour in anticipation of receiving the subsidy. For this reason, a conscious effort was made to collect retrospective information, i.e. relating to the period before the first wave.

[6] Note that throughout the text, "treatment" is used to refer to both TSP and TCP areas taken together.

While this was relatively straightforward for some variables such as school enrolment, it was not feasible for others, such as detailed data on time use of each household member during the day before the survey.

The surveys contain detailed information on a wide range of individual and household characteristics, including the household socio-demographic structure, dwelling conditions, household assets, household member education levels, use of healthcare services, children's and mother's anthropometric indicators, household consumption, labour supply, income and transfers. In addition, information on the municipality infrastructure, wages and food prices was collected by administering questionnaires to well-informed town authorities and visits to local markets.

3.2 Characteristics across treatment and control areas at the baseline

The success of the evaluation and thus the credibility of the results (both of which we come back to in more detail in sections 4 and 5), ultimately rest on the choice of an appropriate comparison group on the basis of which to construct the counterfactual. This is because, to pre-empt somewhat, our evaluation methodology is based on a comparison of targeted outcomes before and after the programme in the set of towns that received it, with the same outcomes in a set of towns that did not receive it, but that was chosen to be similar along many observed dimensions. Conditional on this, the underlying assumption of our approach is that there are no unobserved differences between areas that affect outcomes.

Mindful of this, Table A1 in the appendix presents average values of an extensive range of household and village characteristics in TSP, TCP and control areas at the baseline. The table includes proxies for household wealth such as education levels of the head and the spouse, conditions of the household dwelling and type of ownership, access to various amenities, as well as the distance of the household to the nearest school. The variables relating to the village include the labour income of an average household in the village in 1999, the number of schools, as well as proxies for school resources such as the student-teacher ratio. It turns out that the three sets of towns are very close to each other on the basis of these dimensions. The fact that there are some statistically significant differences between treated and control areas, is in many ways

unavoidable given that the programme was not randomly assigned. However, the magnitude of the differences is in all cases very small, and moreover it is worth bearing in mind that we control for all of these pre-programme observed characteristics in the analysis.

3.3 School enrolment across treatment and control areas at the pre-baseline, baseline and follow-up

A key focus of the paper is to evaluate the effects of the programme on school choices. As this relies on comparing school outcomes between treatment and control areas, an important issue is the extent to which their school choices *before* the programme started are comparable. Table 1 provides a comparison of school enrolment rates across TSP, TCP and control areas one and two years before the programme, and one year after it, separately for urban and rural areas, and for children aged 8-13 and 14-17.[7] Note that these age cut-offs are guided by the sharp reduction in school enrolment in Colombia at age 14 observed in our data. School enrolment is defined on the basis of whether the child is registered at school in the relevant academic year. Pre-baseline data is collected retrospectively at the time of the baseline survey.

[7] All households in our sample live in rural municipalities: we use the term urban areas to denote the "cabecera municipal", i.e. the governing centre of the municipality, which is the most urbanised part of these rural municipalities.

Table 1: Enrolment rates in TSP, TCP and control areas in pre-baseline, baseline and follow-up periods

	TSP	TCP	Control
	%	%	%
Rural 14-17			
Pre-baseline	52.18	54.56	45.55
Baseline	54.51	57.22	43.33
Follow-up	59.04	65.24	48.58
Rural 8-13			
Pre-baseline	84.90	89.34	81.68
Baseline	90.93	94.83	86.83
Follow-up	92.42	94.12	88.35
Urban 14-17			
Pre-baseline	69.39	80.24	66.93
Baseline	71.61	79.70	64.89
Follow-up	76.69	82.46	69.88
Urban 8-13			
Pre-baseline	90.24	94.22	89.75
Baseline	95.63	96.48	92.50
Follow-up	96.52	96.64	93.46

There are a couple of points to take from the table, both of which highlight the importance of controlling for pre-programme enrolment in evaluating the effects of the programme on school choices, but for different reasons. The first is that there appear to be underlying pre-programme differences in enrolment in treatment and control areas before the programme: enrolment in control areas is generally lower than in treatment areas, particularly for older rural children. The second is that one cannot rule out the possibility of anticipation effects: we see that for older children there is a small increase in enrolment between pre-baseline and baseline in TSP areas, which is not observed in control areas. There is also an increase between pre-baseline and baseline for younger groups, but this is however observed in both treatment and control areas.

Another point worth noting from the table is that fact that even though enrolment in treatment areas is higher than in control areas after the programme, it is not clear that the increase in enrolment from before to after the programme is higher in treatment areas: for the TSP-control comparison, it depends on to the extent to which we believe

baseline TSP enrolment rates to reflect anticipation effects and to be thus contaminated by the programme. We return to an investigation of this in section 4.

3.4 Work activities in treatment and control areas at the baseline and follow-up

We carry out the same exercise for enrolment in different types of work, as well as for the amounts of time spent in work and school, although we do not observe these latter outcomes for the pre-baseline (not least of all owing to the difficulty in obtaining accurate retrospective information on these outcomes, a problem that is particularly acute for children). As this implies that we have no pre-programme data on these for TCP areas, we exclude them from our time use component of the analysis. In Table 2, we show participation in income-generating work (both on the labour market and on the family business[8]) and domestic work, at the extensive and intensive margins, for baseline and follow-up periods, and TSP and control areas. Time allocation is measured in hours and fractions thereof and relates to the day before the interview.[9] Note that we have no time use information for children below age 10.

In line with the descriptive statistics relating to school enrolment, children in control and TSP groups differ also in their time allocation at baseline, as shown in Table 2. In particular, children in TSP areas participate more in income-generating and domestic activities at baseline and go to school for fewer hours, compared to children in control areas – with the exception of 14-17 children in rural TSP areas who work less hours in income-generating activities compared to children in control municipalities. This is interesting, as even though school participation was seen to be higher in TSP than in control areas before the programme, the reverse is true for the amount of time spent in school in the same period, which is higher in control areas.

A number of points relating to the period after the programme are also worth noting from the table. First, work participation in TSP areas is in general lower than at

[8]We pool market and family work due to the very low employment rates of children in the labour market, particularly of those aged 10-13 whose participation in market work at the baseline is around 2.7%.
[9]We drop children interviewed on a Sunday or a Monday from the analysis, as their time use refers to a Saturday or Sunday respectively, which are not regular school days. This leads to the loss of 24.2% and 20.7% of 10-17 year old children, at baseline and follow-up respectively. This selection is based purely on the timing of interviews, which is independent of household characteristics and choices.

baseline, in contrast to what is observed in control areas apart from for children aged 14-17 in rural areas. Second, time spent at school appears to increase more in treated than in control areas. Third, time spent at domestic work decreases more in treated than in control areas for all groups, and the same is observed for hours in income-generating work – again, apart from for children aged 14-17 in rural areas. All of this evidence is consistent with there being desirable effects of the programme on child time allocation: in section 4 we go on to the causal analysis of the effects and to interpret our findings further.

Table 2: Participation in and time allocated to activities in TSP and control areas in baseline and follow-up periods

	Baseline		Follow-up	
	TSP	**Control**	**TSP**	**Control**
Rural 14-17				
Participation in income-generating activities	19.39%	21.32%	19.52%	19.90%
Participation in domestic work	67.62%	61.25%	65.12%	61.37%
Hours of income-generating work	1.45	1.57	1.52	1.50
Hours of domestic work	2.70	2.31	2.11	2.03
Hours of school	1.44	1.79	2.57	2.25
Rural 10-13				
Participation in income-generating activities	6.53%	3.97%	4.81%	3.79%
Participation in domestic work	69.20%	62.63%	65.67%	65.68%
Hours of income-generating work	0.35	0.21	0.24	0.24
Hours of domestic work	1.99	1.55	1.34	1.46
Hours of school	2.18	3.30	4.17	3.68
Urban 14-17				
Participation in income-generating activities	17.32%	11.76%	13.33%	12.22%
Participation in domestic work	65.99%	55.15%	60.71%	57.49%
Hours of income-generating work	1.22	0.79	0.78	0.86
Hours of domestic work	1.98	1.39	1.23	1.22
Hours of school	0.97	2.71	3.39	3.19
Urban 10-13				
Participation in income-generating activities	5.29%	2.95%	3.07%	2.29%
Participation in domestic work	69.86%	54.36%	61.38%	56.5%
Hours of income-generating work	0.25	0.17	0.15	0.11
Hours of domestic work	1.71	0.91	0.88	0.84
Hours of school	1.22	3.65	4.22	4.04

3.5. Determinants of school enrolment and work participation at baseline

As noted already, observing as much information as possible about the villages and households in our sample is crucial to the quasi-experimental evaluation set-up. It not only allows us to balance treatment and control areas, so as to ensure that we are comparing like with like, but also to improve the precision of the estimated effects. Moreover, our reason for controlling for these characteristics to begin with, stems from their perceived importance for education and work choices. Indeed, there is an

extensive literature documenting this across various developing countries (see, amongst others, Grootaert and Kanbur (1995), Jensen and Nielsen (1997), Patrinos and Psacharopoulos (1997), Ray (2000). Here we provide a summary of their relative importance in choices in the Colombian context that underlies out analysis.

We estimate a Probit model for school enrolment and work participation before the programme started, for individuals aged 7 (10 for work) through 17. We consider these choices at baseline only and omit TCP areas from the estimation, as the magnitudes and relative importance of determinants in this period may be contaminated by the existence of the programme in these areas. We control for all of the variables listed in table A1 in the appendix, and show the effects of a subset of them of particular interest in Table 3.

Turning to the effects for school enrolment, shown in column (1) of Table 3, we see that females are more likely to be enrolled in school than males, contrary to what is observed in Mexico (Skoufias and Parker, 2001) and other developing countries. Moreover, house ownership, which is a proxy for household wealth, is positively associated with enrolment at school and the effects of parental education are also in line with previous results in the literature on educational choices: higher education levels are associated with a higher probability of school enrolment, and this is particularly so for the education level of the spouse, who is most usually the child's mother. The effect of the child wage, which is the average of all observed child wages in the village, is negative as expected, but not statistically different from zero. As one would expect, the distance to the nearest school and the number of rural schools, which are proxies for the costs of going to school, decrease participation in school.

Table 3 Determinants of school enrolment and work participation at baseline, TSP and Control

Regressors	School enrolment	Income-generating work	Domestic work
	(1)	(2)	(3)
Female child	0.0466	-0.0659	0.2199
	(0.0081)**	(0.0061)**	(0.0140)**
Household owns house	0.0153	-0.0048	-0.0057
	(0.0061)*	(0.0083)	(0.0139)
Distance to nearest school	-0.0007	-0.0001	-0.0000
	(0.0003)*	(0.0002)	(0.0003)
Education level head			
Incomplete primary	0.0214	-0.0031	0.0120
	(0.0100)*	(0.0070)	(0.0143)
Complete primary	0.0390	-0.0071	-0.0254
	(0.0121)**	(0.0083)	(0.0241)
Incomplete secondary	0.0734	-0.0193	-0.0559
	(0.0133)**	(0.0129)	(0.0289)
Complete secondary +	0.0717	-0.0028	-0.0910
	(0.0179)**	(0.0227)	(0.0481)
Education level spouse			
Incomplete primary	0.0344	-0.0091	-0.0016
	(0.0097)**	(0.0068)	(0.0207)
Complete primary	0.0676	-0.0217	-0.0202
	(0.0082)**	(0.0096)*	(0.0248)
Incomplete secondary	0.0819	-0.0241	-0.0600
	(0.0010)**	(0.0106)*	(0.0334)
Complete secondary +	0.1044	-0.0386	0.0242
	(0.0087)**	(0.0146)**	(0.0432)
Municipality variables			
Number of urban schools	0.0005	-0.0020	-0.0105
	(0.0014)	(0.0017)	(0.0033)**
Number of rural schools	-0.0007	0.0004	-0.0009
	(0.0003)*	(0.0002)	(0.0007)
Average village monthly child wage	-0.0141	-0.0026	-0.0660
	(0.0110)	(0.0089)	(0.0240)**
Observations	15,245	7,885	7,883

Notes to table: we also control for variables listed in table A1 of the appendix. For school enrolment (work), sample comprises 7(10)-17 year olds at baseline in TSP and control areas. Note that average village monthly income is the average across the working children in the village.
The sample size for work participation is lower than that for school enrolment, due to the fact that only 11,117 children are aged 10 or above, and of these, one quarter are interviewed on Sunday or Monday and are therefore dropped from the sample. The few remaining ones are due to missing or inconsistent responses.

Turning to participation in work, columns (2) and (3) show that females are less frequently involved in income-generating activities compared to males but are more likely to undertake domestic work. In general, the effects of other variables are less noteworthy than for school enrolment decisions, though this may be partly due to the lower sample sizes and resulting decrease in precision. The effect of the spouse's

education is less strong, and even though it decreases the likelihood of participation in income-generating work, it has no significant effect on domestic work. Perhaps not surprisingly, the number of schools in the urban part of the municipality significantly decreases the incidence of domestic work, as do high child wages.

4. Evaluating the impact of *Familias en Acción* on school and work

We estimate the effect of the programme on school and work participation, and on the intensity of participation, using a difference-in-differences (DiD) methodology. This can be thought of as a before-after comparison of outcomes in treated areas, in which the difference is adjusted by the change experienced by the control group during the same period, in order to account for time trends that are unrelated to the programme. Identifying the programme effect using this approach of course assumes that time trends are the same in treated and control areas. Moreover, it assumes that conditional on observables, there are no unobserved factors affecting outcomes differentially in treated and control areas.[10]

Even though these assumptions are largely untestable, we here provide some support for their likely validity. First, it is important to bear in mind that we do in fact observe a huge amount about these villages, even including characteristics ranging from indicators of household and village poverty to school infrastructure including measures of quality, and we control for all of these in the analysis. This alleviates concerns about omitted variable bias. Second, the assumption that outcomes in treated and control areas would have trended in the same ways in treated and control areas had the programme *not* been implemented, can of course never be tested. However, we observe three years of per capita household labour income in treated and control areas, shown in Figure 1 in the Appendix. The evolution of this variable is very similar in treated and control areas prior to the programme, and there is no reason on the basis of this figure for one to expect them to subsequently diverge. This evidence, though descriptive and though relating to a non-outcome variable, is at least consistent with our common trends assumption. Third, note that the assumption of

[10] With a linear model, one can allow for different unobserved factors in treated and control areas, as long as these are fixed over time, as their effects would be purged in the linear difference-in-difference estimation. However, our estimation is non-linear and so this no longer holds.

common time effects is likely to be violated if individuals living in treatment areas change behaviour *in anticipation of* the programme. This would mean that outcomes in treatment areas in the period before the programme would not be representative of outcomes in treatment areas in the absence of the programme. This assumption can be tested in our data for school enrolment, allowing us to separate out anticipation effects and actual effects of the programme. We return to this issue in the following section.

Note that whilst the overall methodological framework is the same for both the school enrolment and time allocation analyses, a difference in implementation arises from the fact that we observe one extra period of data relating to school enrolment. For this reason we present each of the two analyses, both methodology and results, separately.

4.1 School Enrolment

In our evaluation of the effect of the programme on school enrolment, we are able to test whether individuals in treatment areas changed enrolment in anticipation of the programme. This is a useful and important feature of the evaluation, and is made possible due to both the way in which the programme was phased in across areas, and the collection of retrospective data. In particular, we exploit the fact that the programme was phased in sequentially across treatment areas, being implemented in TSP areas one year after TCP areas, as discussed in section 2. Moreover, as noted already, retrospective school enrolment data were collected at the baseline survey, so we have pre-programme enrolment rates for *two* years for TSP areas, and for *one* year for TCP areas. Whilst in the year immediately preceding the programme's implementation in TSP areas, the baseline period, a concern is that enrolment has already been affected in some way by the announcement of the programme (indeed individuals in these areas were already registered at this stage), this is much less of a concern two years before the programme started in these areas, in the pre-baseline period. Moreover, the programme is already underway in TCP areas at the baseline, making its existence more imminent for TSP areas.

The specification that we use to estimate the effects of the programme on school enrolment is

$$Y_{it} = \alpha_0 + \sum_{j=1}^{2} \alpha_{1j} 1.(t = j) + \alpha_2 P + \alpha_3 A + \alpha_4 T + \theta' Z_{it} + u_{it} \qquad (1.1)$$

for t = 0, 1 and 2, denoting pre-baseline, baseline and follow-up periods respectively. The 1.( ) notation denotes that the variable has a value of one if the condition in parentheses holds, and 0 otherwise. The rest of the notation is defined as follows

Y_{it}	= 1 if individual i is enrolled in school in period t = 0 otherwise
P	= 1 for TSP=1 or TCP=1 = 0 otherwise
A	= 1 for TSP=1 and t=1 = 0 otherwise
T	= 1 for (P=1 and t=2) or (TCP=1 and t=1) = 0 otherwise
Z_{it}	is a set of pre-programme individual, household and area characteristics

Note that in the above specification, which is estimated using three periods of data on school enrolment, the programme effect is identified from follow-up differences in enrolment between all treatment and control areas, and baseline differences between TCP and control areas, net of pre-baseline differences between treatment and control areas.[11] We assume throughout that $u_{it} \sim IN(0,\sigma^2)$, and estimate equation (1.1) using a probit model. The anticipation effect is identified by netting out differences in enrolment between TSP and control areas at pre-baseline from differences between them at baseline. Pre-baseline enrolment in treatment and control areas allows us to identify fundamental differences between the two areas.

Before proceeding, it is worth pointing out that one common criticism of the parametric specification is that extrapolation beyond the region of "common support", i.e. the region over which treated individuals have a counterpart in the group of controls, can lead to misleading inferences. Aware of this concern, we first estimate the effect of the programme on school enrolment using non-parametric kernel propensity score matching (see Attanasio et al (2004) for methodological details). This is both to investigate the region of common support, as well as to estimate effects

[11] Note that we also allowed for the treatment impact to differ depending on the treatment duration as TCP areas at follow-up have been receiving the programme for a longer period of time than TSP areas at follow-up or than TCP areas at baseline. However, we found no evidence of the programme impacts varying with length of exposure to the treatment.

non-parametrically so as to have a benchmark for comparison with the parametric specification.

In Table 4 we show the percentages of treated individuals that we succeed in finding matches for in the control areas. We consider separately urban and rural areas, and children aged 8-13 and 14-17. The region of common support is very large for urban areas: practically all treated individuals have a suitable match amongst the set of controls. Whilst the common support overlap is slightly lower in rural areas, it is still the case that around 91% of the older group and 92% of the younger group of treated individuals have at least one suitable match in the control group. Table 4 also contains the non-parametric estimates of the effect of the programme on individuals who fall within the common support. It provides evidence that the programme has increased school enrolment, particularly of older children. However, the effects are imprecisely estimated and so we impose a parametric specification to increase efficiency.

Table 4: Effect of programme on school enrolment, propensity score matching

	Rural 14-17	Rural 8-13	Urban 14-17	Urban 8-13
Propensity Score Matching				
P^o	91.4%	92.2%	99.0%	98.4%
Effect	0.0699*	0.0233	0.0347	0.0065
	(0.0291)	(0.0236)	(0.0252)	(0.0183)

Notes to table: Coefficients are estimated using propensity score matching. P^o denotes the percentage of treated individuals falling outside the common support, where common support is imposed by dropping treatment observations whose propensity score is higher than the maximum or less than the minimum propensity score of the controls. Bootstrapped standard errors, based on 200 replications, are in parentheses. We control for variables listed in Table A1 of the appendix.

We estimate the parametric specification of equation (1.1) using a Probit model, as the outcome variable is discrete in nature. To minimise any extrapolation bias within the parametric specification, we restrict the analysis to individuals who fall within the common support, as determined using the non-parametric methods described above.[12] Table 5, which presents the results of estimating equation (1.1), shows that the programme has had positive and significant impacts on school enrolment, especially for older age groups, of just under 7 percentage points for rural areas and 5 percentage points for urban areas. It has had a lower effect, of just over 2 percentage points, on the enrolment rates of young children in rural areas, and an effect of just over 1

[12]Note however that the estimates obtained when we do not restrict the sample to those within the common support, are very similar and are shown in Table A2 of the appendix.

percentage point for young children in urban areas. Comparing tables 4 and 5, we see that the estimates of the effects are robust across econometric specifications, but that the parametric results are more precisely estimated.

Table 5: Marginal effect of programme on school enrolment, anticipation effects and fundamental differences between treatment and control areas, Probit model

	Rural 14-17	Rural 8-13	Urban 14-17	Urban 8-13
Probit Model				
Treated (α_4)	0.0711 (0.0230)**	0.0270 (0.0104)*	0.0478 (0.0113)**	0.0138 (0.0046)**
Anticipation (α_3)	0.0390 (0.0287)	-0.0164 (0.0160)	0.0093 (0.0170)	0.0082 (0.0058)
TSP-TCP area (α_2)	0.0800 (0.0545)	0.0267 (0.0093)**	0.0400 (0.0233)	0.0167 (0.0072)*
N	1,900	3,735	1,583	2,818

Notes to table: Marginal effects in the upper panel are estimated from a Probit model using equation (1.1). N is the number of treated individuals falling within the common support in the follow-up period. * denotes statistical significance at the 1 to 5 per cent level; ** denotes statistical significance at the 1 per cent level or less. Standard errors, clustered at the municipality level, are in parentheses. Control for variables listed in Table A1 of the appendix.

Table 5 also shows the anticipation effects, identified from differences between TSP and control areas at baseline. The indication from Table 1 that older children in rural areas are the ones most likely to have changed enrolment in anticipation of the programme, is borne out but not statistically significantly so. The rest of the anticipation effects are very low and not statistically different from zero at conventional levels.

Moreover, there is evidence in Table 5 of significant pre-baseline differences in enrolment between treatment and control areas for young children, with enrolment being significantly higher for young children in treatment areas; the differences between older children, though sizeable, are not statistically different from zero. We also tested, in a more flexible specification, whether these fundamental differences hold for each of the two types of pilot areas, relative to the controls, by splitting the treatment dummy variable into TSP and TCP: the estimated coefficients were not statistically different from each other, hence they are not reported.

4.2 Time Allocation

We have seen in the previous section that the programme has been effective in its main objective: that of contributing to human capital accumulation via increasing enrolment in school. This increased time at school must come at the expense of some other activities that the child was formerly engaged in, whether work or leisure related. Indeed, the short-term effects on children welfare of increased school enrolment depend on whether the CCT programme reduced time spent by children in work-related activities, vis-à-vis affecting leisure time. Moreover, the effect of the programme on the child's contribution to household labour income, and thus on the immediate welfare of the household, can be gauged somewhat by considering the extent to which involvement in income-generating work was affected by the programme.

In this section, we assess how the programme has affected the amounts of time spent by children in work activities, as well as at school. To this end, we use detailed time use data from both the baseline and follow-up. As discussed in section 3, there are no pre-baseline data on this outcome. This means that we have no pre-programme information on time use for TCP areas, given that they were already receiving the programme at the baseline. We thus have no way of controlling for fundamental differences in time use between TCP and control areas. We therefore choose to exclude TCP areas from all of the analysis that follows.

There is still the concern that we cannot estimate separately how much of the baseline difference in time allocation between TSP and control areas is due to fundamental differences in time uses between the two areas, and how much is due to TSP individuals changing behaviour in anticipation of the programme. However, if anticipation effects in work choices exist, our estimates of the effect of the programme on child time allocations would represent lower bounds on the actual effects, assuming that individuals reduce participation in work in anticipation of the programme, and never increase it, an assumption that is consistent with the overall treatment effects we go on estimate. Moreover, the school enrolment analysis of the previous section provides little evidence of anticipation effects. Therefore we are

fairly confident that, even though we do not have pre-baseline data, the baseline data is sufficient to capture fundamental differences in time uses given that school enrolment decisions and time allocation are highly correlated. To further alleviate our concerns, we control for pre-baseline school enrolment. However, our results are not sensitive to omitting it from the set of regressors.

In the analysis that follows, we consider income-generating activities (i.e. labour market and family business activities) both separately from and together with domestic activities. The groups that we consider are the same as in section 4.2, apart from a higher cut-off of age 10 for the younger groups, as time use information is not collected from children any younger than this.

To ascertain whether *participation* in various activities has changed due to the programme, we first use the time allocation data to construct binary indicators of participation in different activities, denoted *j*, which may be income-generating activities, domestic work, total work (which pools the two previous activities), or school. For each group, we use data from the baseline and follow-up periods, across TSP and control areas, to estimate the following equation

$$P_{it}^{j} = \beta_{0j} + \beta_{1j} 1.(t = 2) + \beta_{2j} TSP + \beta_{3j} T + \psi' Z_{it} + u_{it} \qquad (1.2)$$

where P_{it}^{j} = 1 if individual *i* spends a positive amount of time in activity *j* on the day before the interview in period *t*
= 0 otherwise
T = 1 for TSP=1 and t=2
= 0 otherwise.

All other variables are as previously defined. As our outcome variable is discrete in nature, we estimate equation (1.2) using a Probit model, for each of the activities listed above. The results are shown in Table 6.

We see from column (1) that the programme has had very little effect on participation in income-generating activities, decreasing participation of young children in urban areas only, and by just 1 percentage point. In other words, the counterfactual participation rate in income-generating work for this group is around 4.1% (estimated using their actual post-programme participation rate work of 3.1% - see Table 6).

The effects of the programme on participation in domestic work are much larger, as can be seen from column (2). The programme has decreased participation in domestic work of young children in both rural and urban areas, by 9.5 and 12.3 percentage points respectively. The corresponding counterfactual (actual) participation rates are 74.2% (65.7%) and 73.7% (61.4%) in rural and urban areas respectively. There has also been a decrease in participation of older children in urban areas, by just over 8.5 percentage points, to give a corresponding counterfactual participation rate of 69.21%, compared to the observed post-programme rate of 60.7%.[13]

To sum up, we see in column (3) that the programme has significantly reduced participation in work for all groups apart from for older children living in rural areas.[14] This suggests that participation of older children in income-generating activities or domestic work responds less to the programme in rural than in urban areas, which is perhaps not surprising if children are important labour inputs in agriculture and labour markets are more imperfect in rural than in urban areas.

[13]This effect is only significant at 10% level or less.
[14]Note that participation in income-generating activities and domestic work are not mutually exclusive, so the participation rate in work (either domestic work or income-generating activity) is lower than the sum of the two.

Table 6: Impact of the programme on participation in different activities

Participation in:	Income-generating work (1)	Domestic work (2)	All work (3)=(1)&(2)
Rural 14-17			
Marginal effect	-0.0035	-0.0242	0.0001
	(0.0217)	(0.0372)	(0.0338)
N	789	791	789
Participation w subsidy	19.5%	65.1%	80.8%
Rural 10-13			
Marginal effect	-0.0099	-0.0948	-0.1026
	(0.0081)	(0.0491)*	(0.0493)*
N	1,034	1,057	1,057
Participation w subsidy	4.8%	65.7%	69.1%
Urban 14-17			
Marginal effect	-0.0339	-0.0846	-0.1408
	(0.0221)	(0.0459)	(0.0407)**
N	566	567	567
Participation w subsidy	13.3%	60.7%	69.3%
Urban 10-13			
Marginal effect	-0.0105	-0.1232	-0.1378
	(0.0050)*	(0.0407)**	(0.0417)**
N	720	734	734
Participation w subsidy	3.1%	61.4%	62.8%

Notes to table: Marginal effects are estimated using equation (1.2). N is the number of treated individuals in the follow-up period. * denotes statistical significance at the 1 to 5 per cent level; ** denotes statistical significance at the 1 per cent level or less. Standard errors, clustered at the municipality level, are in parentheses. Control for variables listed in Table A1 of the appendix. "Participation w subsidy" is the average participation of each group in treated area at follow-up.

However, this analysis ignores intensity of work activity, which is the more important margin from both welfare and income-generating viewpoints. Moreover, if the FA subsidy is not sufficient to replace fully foregone child income, we may expect to observe larger impacts at the intensive rather than at the extensive margin. We estimate the impact of the programme on the amount of time allocated to each activity using the following specification

$$h_{it}^{j} = \gamma_{0j} + \gamma_{1j} 1.(t=2) + \gamma_{2j} TSP + \gamma_{3j} T + \theta' Z_{it} + u_{it} \qquad (1.3)$$

where h_{it}^{j} denotes the amount of time (in hours and fractions thereof) spent by individual *i* in activity *j* in period *t* and all other variables are as previously defined. We estimate equation (1.3) for each activity using a Tobit model, to account for the

fact that the dependent variable is censored at zero for individuals who report that they do not spend any time in activity *j*.

The results are shown in Table 7. For each activity, we report both the estimated coefficient γ_{3j}, which is the discrete change in the latent dependent variable as a result of the programme, and the marginal effect, which represents the average increase in time allocated to a particular activity if a household receives the programme.[15] To assess the magnitude of these effects, below the marginal effects we report the average number of hours supplied at follow-up by children in treated areas.

The main message to emerge from Table 7 is that the programme increases significantly the amount of time spent in school for all children, and decreases time at work for almost all groups.

The magnitudes of the impacts are however, very different across groups: the estimated impact is largest for young children, who spend around 4.2 hours and 2.3 hours more per day in school after the programme compared to their counterparts in control areas, in urban and rural areas respectively, to attend school for 4.2 hours per day on average following the programme. Time at school also increases substantially after the programme for urban children aged 14-17, by 3.5 hours as shown in Table 7. For children aged 14-17 in rural areas however, the effect of the programme on the number of hours at school is low, around 0.8 hours, but again statistically different from zero at conventional levels. Their time spent at work is not significantly reduced by the programme, which as noted already may be indicative of inelastic child labour supply in rural areas.

[15]In contrast to the estimate γ_{3j}, this effect takes into account the non-linearity of the dependent variable.

Table 7: Impact of the programme on hours of child time uses

Hours spent at:	Income-generating work (1)	Domestic work (2)	All work (3)	School (4)
Rural 14-17				
Coefficient	-0.17	-0.41	-0.34	1.97
	(0.89)	(0.26)	(0.26)	(0.84)**
Marginal effect	-0.02	-0.25	-0.26	0.82
	(0.12)	(0.16)	(0.19)	(0.37)**
No. hours with subsidy	1.5	2.1	3.6	2.6
Rural 10-13				
Coefficient	-2.22	-0.89	-1.01	2.96
	(-)	(0.32)**	(0.32)**	(0.78)**
Marginal effect	-0.04	-0.53	-0.62	2.31
	(-)	(0.18)**	(0.19)**	0.55**
No. hours with subsidy	0.2	1.3	1.6	4.2
Urban 14-17				
Coefficient	-3.03	-0.95	-1.54	4.98
	(1.64)	(0.28)**	(0.30)**	(0.78)**
Marginal effect	-0.21	-0.50	-0.90	3.52
	(0.09)*	(0.14)**	(0.16)**	(0.64)**
No. hours with subsidy	0.8	1.2	2	3.4
Urban 10-13				
Coefficient	-2.80	-1.02	-1.13	4.84
	(-)	(0.21)**	(0.23)**	(0.83)**
Marginal effect	-0.04	-0.50	-0.56	4.23
	(-)	(0.09)**	(0.10)**	(0.74)**
No. hours with subsidy	0.1	0.9	1	4.2

Notes to table:

- The coefficients and marginal effects are estimated parametrically using equation (1.3), controlling for the variables in the Table A1 of the appendix, as well as for an indicator of pre-baseline school enrolment. Treatment areas include TSP only. Bootstrapped standard errors based on 200 replications, adjusted for clustering at the municipality level, are in parentheses.* denotes statistical significance at the 5 per cent level or less. ** denotes statistical significance at the 1 per cent level or less. "No. hours with subsidy" is the average number of hours provided by each group in treated areas at follow-up.
- For children aged 10-13 years, it was not possible to bootstrap the standard errors of the impacts of the programme on hours spent in income-generating activities due to the very low number of positive outcomes. For this reason we do not report standard errors for these groups. However, on the basis of very large non-clustered standard errors (likely to be inflated even more after adjusting for clustering), we can say that the effects are not statistically different from zero.

Another important point to take from this table is that when the programme has significant impacts on times at school and at work, the increased time at school is not wholly substituted by reduced time at work. For children aged 14-17 living in urban areas and for children aged 10-13 in rural areas, more than one quarter of the increase in time spent at school comes out of time that would otherwise have been spent on work activities. However, in urban areas, substitution effects are much smaller for younger children, as less than one seventh of the increase in their time spent at school comes out of time at work.

Moreover, most of the substitution relates to domestic work. Time spent at income-generating activities does not change significantly after the programme, except for children aged 14-17 in urban areas. However, the magnitude of the impact is small, as the programme decreases their time spent at income-generating activities by around 0.2 hours. This suggests that the leisure time of children decreases slightly after the programme, although we have no direct information on this to substantiate this claim. It also suggests that the contribution of children to total household labour income may not have decreased much due to the low impacts of the programme on child labour supply.

5. Conclusion

In this paper, we have evaluated the effects of an ongoing large-scale welfare programme in Colombia, *Familias en Acción*, on school participation and work activities of children.

In terms of school enrolment, our main finding is that the programme has increased the school participation rates of 14 to 17 year old children quite substantially, by between 5 and 7 percentage points, to observed enrolment rates of 64% and 81% in rural and urban areas respectively. It has also had non-negligible effects on the enrolment of younger children, of between around 1.5 and 2.5 percentage points, despite their already high attendance rates in the absence of the programme, at between 91% and 96%. Whilst the effects on attendance are largest for older groups, in our analysis of the effects at the intensive margin, we have found that the effects are most pronounced for younger children, whose school attendance goes up by between three and four hours per day compared to just over one hour for older urban children, and no significant effects for older rural children.

The effects on domestic work participation are largest for younger children whose participation is around 10 and 12 percentage points lower after the programme, at 66% and 61% in rural and urban areas respectively. We also find that time spent at work was only partially substituted by increased time spent at school, and that in fact most of this substitution comes from decreased time spent at domestic work. These results suggest that parents are substituting other uses of their children's time and are

not using the conditional subsidy to replace fully the earnings from their children's work, but are choosing to combine the two sources of income. The largest substitution effects are observed for children aged 14-17 living in urban areas and for children aged 10-13 in rural areas for whom more than one quarter of the increase in time spent at school comes out of time that would otherwise have been spent on work activities. As the programme does not decrease significantly the time spent by children in income-generating activities, we can also infer from this that household income has not been negatively affected through this channel.

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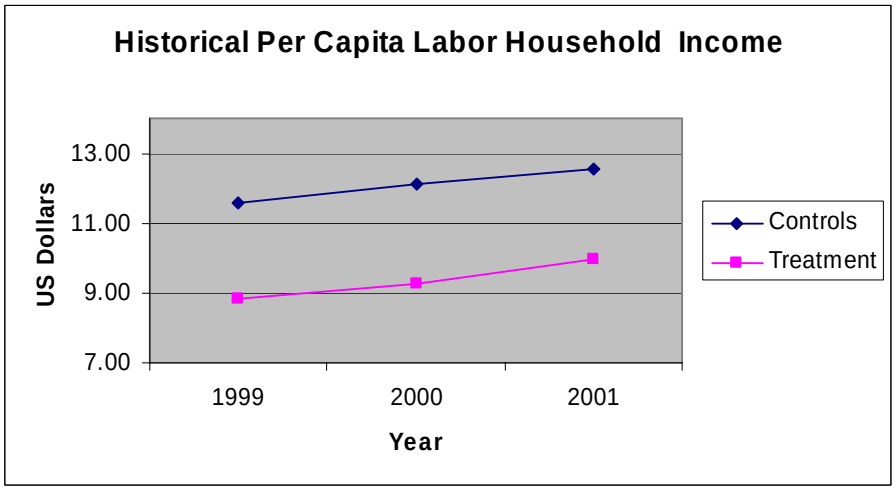
Appendix

Figure 1



Note: this figure is taken from Attanasio et al (2005b).

Table A1: Summary of mean characteristics across TSP, TCP and control areas at the baseline

	TSP	TCP	Control
Age of child	10.89 (0.04)	11.03 (0.05)	11.02 (0.03)
Child is female	0.472 (0.005)	0.472 (0.005)	0.470 (0.005)
Health insurance of head			
Unsubsidised	0.029 (0.008)	0.037 (0.008)	0.052 (0.008)
Subsidised	0.706 (0.033)	0.636 (0.040)	0.707 (0.026)
Informally subsidised	0.184 (0.033)	0.214 (0.039)	0.138 (0.021)
Age of head	44.45 (0.32)	45.59 (0.36)	45.38 (0.30)
Age of spouse	40.39 (0.32)	41.33 (0.32)	41.34 (0.31)
Single parent	0.188 (0.012)	0.206 (0.013)	0.183 (0.008)
Education level head			
None	0.287 (0.029)	0.252 (0.020)	0.283 (0.023)
Incomplete primary	0.459 (0.027)	0.499 (0.024)	0.455 (0.026)
Complete primary	0.145 (0.013)	0.140 (0.012)	0.136 (0.010)
Incomplete secondary	0.075 (0.008)	0.085 (0.011)	0.090 (0.013)
Complete secondary +	0.033 (0.005)	0.023 (0.004)	0.037 (0.006)
Education level spouse			
None	0.234 (0.022)	0.226 (0.023)	0.241 (0.021)
Incomplete primary	0.496 (0.016)	0.498 (0.026)	0.465 (0.026)
Complete primary	0.157 (0.016)	0.159 (0.011)	0.158 (0.011)
Incomplete secondary	0.080 (0.010)	0.083 (0.011)	0.097 (0.012)
Complete secondary +	0.032 (0.005)	0.033 (0.005)	0.038 (0.005)
House walls			
Brick	0.429 (0.038)	0.401 (0.030)	0.449 (0.032)
Mud	0.407 (0.052)	0.377 (0.047)	0.331 (0.044)
Good quality wood	0.121 (0.041)	0.159 (0.041)	0.177 (0.039)
Poor quality wood	0.032 (0.009)	0.046 (0.012)	0.025 (0.007)
Cardboard/none	0.011 (0.004)	0.017 (0.005)	0.018 (0.003)
Has piped gas	0.052 (0.023)	0.092 (0.036)	0.069 (0.027)
Has piped water	0.643 (0.042)	0.512 (0.037)	0.630 (0.039)
Has sewage system	0.279 (0.044)	0.192 (0.032)	0.246 (0.033)
Has rubbish collection	0.300 (0.036)	0.245 (0.033)	0.339 (0.049)
No telephone	0.915 (0.015)	0.918 (0.016)	0.904 (0.020)
Communal telephone	0.020 (0.007)	0.020 (0.012)	0.011 (0.003)
Private telephone	0.064 (0.012)	0.062 (0.013)	0.085 (0.018)
Toilet connected to sewage	0.501 (0.035)	0.500 (0.029)	0.520 (0.032)
Own house	0.683 (0.025)	0.651 (0.021)	0.651 (0.021)
Rented house or in mortgage	0.093 (0.013)	0.085 (0.011)	0.079 (0.010)
Occupied house without legal agreement	0.039 (0.010)	0.031 (0.008)	0.066 (0.015)
House in usufruct	0.184 (0.016)	0.233 (0.020)	0.204 (0.014)
Householder suffered from violence 2000-2002	0.034 (0.006)	0.027 (0.006)	0.041 (0.008)
Mins to nearest school	13.19 (1.13)	14.88 (1.00)	13.15 (0.84)
Municipality variables			
Altitude	600.85 (141.87)	659.91 (158.74)	517.06 (113.65)
# urban public schools	6.867 (1.235)	10.349 (1.731)	7.782 (1.640)
# rural public schools	36.568 (5.488)	48.394 (4.807)	29.643 (3.913)
# students per teacher	22.44 (1.24)	22.68 (0.76)	22.58 (0.88)
Class m^2 per student	2.93 (0.44)	2.84 (0.41)	2.50 (0.26)
Urban population 2002	13744.1(2928.3)	16313.8 (3401.9)	14760.40 (3331.9)
Rural population 2002	12715.5 (1700.0)	18275.1 (1958.2)	12176.0 (1885.0)
Average labour income 1999	127511.9 (8071.0)	136385.2 (8387.9)	163112.05 (7485.1)
Region of residence			
Atlantic	0.377 (0.104)	0.432 (0.107)	0.431 (0.109)
Oriental	0.206 (0.081)	0.203 (0.080)	0.221 (0.084)
Central	0.264 (0.092)	0.253 (0.089)	0.214 (0.081)
Pacific	0.153 (0.084)	0.112 (0.065)	0.133 (0.076)
Sample size	7,077	7,580	10,330

Notes to table: Sample of households with at least one child aged 8 to 17 at the follow-up. Standard errors in parentheses.

Table A2: Marginal effect of programme on school enrolment, anticipation effects and fundamental differences between treatment and control areas, Probit model, whole sample

	Rural 14-17	Rural 8-13	Urban 14-17	Urban 8-13
Probit Model				
Treated (α_4)	0.0696 (0.0221)**	0.0238 (0.0113)*	0.0486 (0.0114)**	0.0136 (0.0045)**
Anticipation (α_3)	0.0381 (0.0270)	-0.0168 (0.0156)	0.0102 (0.0167)	0.0085 (0.0058)
TSP-TCP area (α_2)	0.0772 (0.0553)	0.0293 (0.0099)**	0.0400 (0.0234)	0.0170 (0.0072)*
N	2,080	4,050	1,598	2,865

Notes to table: Marginal effects in the upper panel are estimated using equation (1.1). N is the number of treated individuals in the follow-up period. * denotes statistical significance at the 1 to 5 per cent level; ** denotes statistical significance at the 1 per cent level or less. Standard errors, clustered at the municipality level, are in parentheses. Control for variables listed in Table A1 of the appendix.