CHILD EDUCATION AND WORK CHOICES IN THE PRESENCE OF A CONDITIONAL CASH TRANSFER PROGRAMME IN RURAL COLOMBIA

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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. This research is part of a large evaluation effort, undertaken by a consortium formed by IFS, Econometria and SEI, which has considered the effects of *Familias en Acción* on a variety of outcomes one year after its implementation. In early reports (see Attanasio et al. (2005)), we focussed on the effects of the programme on school enrolment. In this paper, we both expand those results, by carefully analysing anticipation effects along with other issues, and complement them with an analysis of child labour - both paid and unpaid (including domestic) work. The child labour analysis is made possible due to a rich time use module of the surveys that has not previously been analysed.

Our analysis of the effect of the programme on school enrolment differs from most existing evaluations, in that we can disentangle anticipation effects from "true" effects of the programme. This is because we observe school enrolment rates in around half of the 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, this is much 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 evaluations, it is nonetheless the case that evidence of strong anticipation effects would not bode well for studies that do not have the data to separately identify them.

We also investigate the effect of the programme on child income-generating and domestic work activities. This is because the analysis of school enrolment says little about the effects of the programme on child labour: whilst child labour and schooling are substitutable, they are by no means mutually exclusive. Indeed there can be no supposition that if the programme has increased school enrolment, this has led to a reduction in child labour.

For political reasons, the programme was not randomly assigned across localities. To evaluate its effect, we compare outcomes of interest in areas in which the programme was not implemented (control) to those in which it was (treated). Whilst the control areas were carefully chosen to be as similar as possible to the treated ones, we nonetheless condition the comparison on a large range of household and municipality level characteristics as well as controlling fro pre-programme differences in the outcome of interest. We will argue that we are successful at finding suitable control individuals for the majority of treated individuals in this experimental set-up.

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, and had lower, but non-negligible effects on the enrolment of younger children of between 1.4 and 2.4 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.

The paper proceeds as follows. In section 2, we describe the programme and set it in 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 on school enrolment and time use before the programme started. In section 4 we present our results, in two parts: the first provides results for school enrolment, and the second reports results on 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 change intra-household decisions.[1]

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 the school attended, being 14,000 pesos (US$6) and 28,000 pesos (US$12) for children attending primary and secondary school respectively.[2] Making the grant conditional on school attendance, effectively decreases the relative price of education, making it more attractive. 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 behaviour, not only due to the increase in income but also due to this additional income being controlled by a female member of the household.

[1] The perceived importance of the intra-household mechanism is implicit in the fact that most CCTs are paid to mothers. The conditionality might be a further and more direct way to ensure that resources are invested in particular household members, namely the children.

[2] In contrast to the PROGRESA programme, the education subsidy does not vary by gender.

The other component of the programme is the nutrition subsidy. A flat-rate monthly monetary supplement of 46,500 pesos (approximately US$20) 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 for a municipality to qualify were: (i) that the town have 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 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 using a score that is 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. FA was targeted to households registered, as at the end of December 1999, as SISBEN level 1 with children less than 17 and living in target municipalities. SISBEN 1 households account for more or less 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, for the purpose of covering the costs of running of the programme for three years. The programme started operating between the latter half of 2002 and the beginning of 2003.[3] The sequential phasing in discussed below had important implications for the evaluation methodology that we discuss in section 4. In the first two years of the programme, a total of 340,000 households were registered to participate. More recently the

[3] In a few localities the programme started as early as the end of 2001.

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 provide some descriptive statistics relating to our sample. First, we show evidence that treatment and control areas are very similar along a wide range of observable household and village characteristics. This balancing of our samples along observable dimensions at least is reassuring given that the programme was not randomly assigned. Second, we show trends in enrolment for three years – two (one) of which are (is) pre-programme for TSP (TCP) areas. This not only gives a flavour as to how enrolment rates vary across areas, but also provides weak evidence of possible anticipation effects for some individuals. We then move on to compare work participation and time allocation across TSP and control areas at the baseline and follow-up. Finally, we look at a set of key determinants of education and work choices before the programme was implemented.

3.1 Data collection

In December 2001, a consortium formed by IFS 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 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 (randomly selected) treatment towns, in terms of population, area and an index of quality of life. In the end, the evaluation sample was made up of 122 municipalities, 57 of which were treatment and 65 of which were controls. Of the four criteria established for eligibility, most controls turned out to fail criteria (iii) (bank presence) and (iv) (availability of up-to-date welfare lists etc.). As a consequence, control towns are slightly poorer than treatment towns, but broadly comparable to treatment towns.

In each of the villages 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, i.e. 94% of the original sample. A third survey is in the field at the time of writing.

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 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 is 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*) the municipalities where the programme started early and the remainder as TSP (*tratamiento sin pago*).[4] 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 below. On the negative side, it should be stressed that the sample of households in TSP towns was aware of the programme and, indeed, had already registered for it, even though they were not yet receiving 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 to collect retrospective information - i.e. relating to the period before the first wave - was made. While this was relatively straightforward for some variables, particularly for discrete ones such as school enrolment, it was not possible for others, such as detailed data on time uses.

[4] Throughout the text, "treatment" is used to refer to both TSP and TCP areas taken together.

The surveys contain information on a wide range of variables, 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. Additionally, information on the municipality infrastructure, wages and food prices was collected by administering questionnaires to knowledgeable town authorities and through visits to local markets.

3.2 Characteristics across treatment and control areas at the baseline

Table A1 in the appendix shows mean values of a 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 including the type of ownership, whether there is access to various amenities, and the distance to the nearest school. The variables relating to the village include number of schools, as well as proxies for school resources such as the student-teacher ratio, and the income of an average household in the village in 1999. Reassuringly, we see that the treatment and control areas are very similar along most of these dimensions, with none of the variables statistically significantly different from each other across areas.

3.3 School enrolment

Table 1 compares 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.[5] These cut-offs are chosen because at age 14 there is a sharp reduction in school enrolment in Colombia. School enrolment is defined on the basis of whether the child is registered at school in the relevant academic year.[6] Pre-baseline data is collected retrospectively at the time of the baseline survey.

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[5] By urban areas we mean urban parts of the rural municipalities in which all households in our sample are living.

[6] There are two academic years or "calendars" in Colombia: August through June, and January through December. Of individuals who report being registered at school, most are in the January-December calendar. For the pre-baseline, the (retrospective) school enrolment relates to calendars Aug00-June01 and Jan01-Dec01. For the baseline, it relates to Aug01-June02, Jan02-Dec02 and Aug02-June03; Note that the Aug02-June03 outcome is

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 worth noting from the table. First, enrolment in control areas is generally lower than in treatment areas, particularly for older rural children. Second, whilst it is difficult to assess the extent of anticipation effects from these raw means, we can see that enrolment increases slightly for older children between pre-baseline and baseline in TSP areas but not in control areas, whereas for younger groups the increase in enrolment between pre-baseline and baseline is observed in both treatment and control areas. Of course, part of these differences could be simply due to underlying differences between the groups. Therefore, in the analysis in section 4, we control for a range of individual, household and village variables that may underlie some of these observed differences.

measured at the time of the follow-up rather than the baseline survey; for the follow-up, it relates to Jan03-Dec03 and Aug03-June04.

3.4 Work Activities

In **Error! Reference source not found.**, we show participation in income-generating work (both on the labour market and on the family business[7]) and domestic work, at the extensive and intensive margins, for baseline and follow-up periods, and TSP and control areas. We exclude TCP areas because we have no pre-programme data for them, as we have no retrospective information on time use at baseline (not least of all owing to the difficulty in obtaining accurate retrospective information on this outcome, particularly for children). Time allocation is self-reported, is measured in hours and fractions thereof and relates to the day before the interview.[8] Note that we have no time use information for children below age 10.

Error! Reference source not found. points to the fact that children in control and TSP groups differ in their time allocation at baseline. Children in TSP areas participate more in income-generating and domestic activities at baseline and less in school activities, compared to children in control areas. But after the programme their participation in work is, in general, lower than at baseline, which is in contrast to what is observed in control areas. Time spent at school after the programme increases more in treated than in control areas. 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 apart from for children aged 14-17 in rural areas.

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 work	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 work	6.53%	3.97%	4.81%	3.79%

[7]Our reason for pooling market and family work is because of the very low employment rates of children in the labour market, particularly for those aged 10-13 whose baseline participation in market work is only around 2.7%.
[8]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. Note that this selection is based on the timing of interviews, which is independent of household characteristics.

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 work	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 work	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

To get a flavour as to what household- and village-level factors are important for school enrolment and work decisions, 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 choose the baseline period for all decisions, and for this reason omit TCP areas from the estimation. We control for all variables listed in table A1 in the appendix, but just show the effects of a select set of them, such as gender of the child, distance to school, house ownership, distance to nearest school, education of the household head and spouse, number of schools and average monthly income of children in the village, in Table 3 below.

For school enrolment, shown in column (1) of Table 3, we see that females are more likely to be enrolled in school than males; house ownership, which may be considered a proxy for household wealth, is also positively associated with enrolment at school. As one would expect, the farther away from the nearest school one lives, the lower is participation in school. The effects of parental education are in line with expectations: 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 negative as expected is, however, not statistically different from zero for school enrolment decisions.

Somewhat surprisingly, the number of rural school has a negative effect. It should be remembered, however, that we are controlling for the distance to the nearest school.

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

Regressors	School enrolment (1)	Income-generating work (2)	Domestic work (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 regressors 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.

For participation in work, columns (2) and (3) show that females are less frequently involved in income-generating activities than males but are more likely to undertake domestic work. In general, the effects of other variables are less striking than for school enrolment decisions due to the lower sample smaller size and decreased

precision. The effect 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. Moreover, 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

In this section, we provide estimates of the effect of the FA conditional transfer on education and work choices, at both extensive and intensive margins.

4.1 Methodology

We estimate the effect of the programme on various school and work related outcomes using a difference-in-differences (DiD) methodology, by comparing relevant outcomes in treatment and control municipalities before and after the programme. Under the assumptions that, conditional on observed individual, household and area characteristics, (a) unobserved area characteristics do not change before and after the programme, and (b) there are common time effects across treatment and control areas, the difference in the outcome before and after the programme for treatment areas minus the difference in the outcome before and after the programme for control areas yields the DiD estimate of the effect of the programme.

The assumption of 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 that below.

4.2 School Enrolment

One novel aspect of the evaluation of the effect of the programme on enrolment, is that we can test whether individuals in treatment areas changed enrolment in

anticipation of the programme. To do this, 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 mentioned 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.[9]

In our specification below, which uses three periods of data on school enrolment, the programme effect is identified from follow-up differences in enrolment between treatment and control areas, and baseline differences between TCP and control areas, net of pre-baseline differences between treatment and control areas. 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. The specification that we estimate 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. Also,

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

[9] Moreover, the programme is already underway in TCP areas at the baseline, making its existence more imminent for TSP areas.

Before proceeding, it is worth pointing out that one potential 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. To allay 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). In Table 4 we show the percentages of treated individuals that we succeed in finding matches for in the control areas. Note that we separately consider 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: 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 (note to e: redo bootstrapping with 200 reps). We control for variables listed in Table A1 of the appendix.

Table 4 also shows 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.

We estimate the parametric specification of equation (1.1) using a Probit model, due to the discrete nature of the outcome variable. To minimise any bias due to extrapolation within the parametric specification, we restrict the analysis to individuals in the common support. However, the results for all individuals are extremely similar and are presented in Table A2 in the appendix. **Error! Reference source not found.**Table 5, which shows the results of estimating equation (1.1),

shows that the programme has had significant effects 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 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 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, estimated in TSP areas. 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.

However, there is evidence in Table 5**Error! Reference source not found.** of significant pre-baseline differences in enrolment between treatment and control areas for young children, with it being higher in treatment areas; the differences between older children, though sizeable, are not statistically different from zero. We test for fundamental differences between the two types of pilot areas separately, and control areas (not reported), by splitting the treatment dummy variable into TSP and TCP: the coefficients are not statistically different from each other however.

We conclude our analysis of school enrolment by presenting, in Table 6, our best estimate of the percentage of children attending school had the programme not been in operation. These counterfactual enrolment rates (in italics) are estimated by subtracting the programme impact from the observed follow-up enrolment rates in treatment areas.

Table 6: Impact of the programme on school enrolment

	Rural 14-17	**Rural 8-13**	**Urban 14-17**	**Urban 8-13**
Impact	0.0711 (0.0230)**	0.0270 (0.0104)*	0.0478 (0.0113)**	0.0138 (0.0046)**
Enrolment w/o subsidy	*56.4%*	*90.8%*	*75.9%*	*95.6%*
Enrolment with subsidy	63.5%	93.5%	80.7%	97.0%

Notes to table: Counterfactual enrolment rates, in italics, are estimated from subtracting the impact from the observed enrolment rates.

4.3 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 is clearly at the expense of some other activities, whether work or leisure related, that the child was formerly engaged in. 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 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, we do not have any pre-baseline data for this outcome. This of course means that we have no pre-programme information on time use for TCP areas, given that they had already been exposed to the programme at baseline. We thus have no way of controlling for fundamental differences in time use between TCP and control areas, which we have

reason to believe exist given the evidence in section 4.2. We therefore exclude TCP areas from the analysis that follows.

There is still the concern that we cannot separately estimate 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. In any case, 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.

However, 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 if - as we know - school enrolment decisions and time allocation are highly correlated. Moreover, to alleviate the concern that any differences in time use between areas remain, we control for pre-baseline school enrolment. Our main results however, are not sensitive to excluding pre-baseline school enrolment from the regressions.

In the analysis that follows, we consider income-generating activities (taking labour market and family business activities together[10]) both separately from and together with domestic activities. The groups that we consider are the same as in section 5.2, apart from a lower cut-off of age 10 for the younger groups, as time use information is not collected from children younger than this.

To get a flavour as to 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

[10]Our reason for pooling market and family work is because of the very low employment rates of children in the labour market, particularly for those aged 10-13 whose baseline participation in market work is only around 2.7%.

school. For each group, we use data from the baseline and follow-up periods, across TSP and control areas, to estimate the following equation

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

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, we estimate equation (1.2) using a Probit model, for each of the activities we consider. The results are shown in Table 7.

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, by just 1 percentage point. As the observed post-programme participation rate in income-generating work for this group is 3.1%, this estimate translates into a counterfactual (i.e. in the absence of the programme) participation rate in income-generating work of 4.1% for this group.

The effects of the programme on participation in domestic work are much larger, as can be seen from column (2). The programme decreases 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 is also 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%.[11]

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

[11]This effect is of borderline significance at conventional levels so we report the counterfactual for this group as well.

areas.[12] 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 easy to understand if children are important labour inputs in agriculture and labour markets are more imperfect.

[12]Note that participation in income-generating activities and domestic work are not mutually exclusive, so the rate of participation in work (either domestic work or income-generating activity) is lower than the sum of the two.

Table 7: 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/o subsidy	-	-	-
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/o subsidy	-	*74.2%*	*79.4%*
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/o subsidy	-	*69.2%*	*83.4%*
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/o subsidy	*4.1%*	*73.7%*	*76.6%*
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.

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

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

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 8. For each activity, we report the estimated coefficient γ_{3j}, which is the discrete change in the latent dependent variable that is brought about by the programme. We also show the marginal effect when it is statistically different from zero at the 5% level or less. This is the average increase in time allocated to a particular activity if a household receives the programme.[13] Using this, we estimate the average time children in the treated areas would have spent at a particular activity had they not received the programme. This is shown in italics in the row beneath the marginal effects.

The main message to emerge from Table 8 is in line with what one would expect: the programme increases the amount of time spent in school for all children, and decreases time at work for all groups apart from older children in rural areas.

The magnitudes of the impacts are, however, very different across groups: the estimated impact is largest for young treated children, who spend 3.7 hours and 2.7 hours more per day in school after the programme than controls, 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 for urban children aged 14-17 after the programme, to 3.4 hours from an estimated counterfactual of 1.3 hours. For children aged 14-17 in rural areas however, the effect of the programme on the number of hours at school is low and not statistically different from zero at conventional levels. It is also noticeable that their time spent at work is not reduced by the programme either, which confirms our interpretation on their inelastic participation in work activities.

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

Table 8: 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	-0.236	-0.392	-0.357	2.092
	(0.969)	(0.268)	(0.278)	(0.855)**
Marginal effect	-	-	-	0.202
				(0.198)
No. hours w/o subsidy	-	-	-	*2.4*
No. hours with subsidy	1.5	2.1	3.6	2.6
Rural 10-13				
Coefficient	-2.328	-0.930	-1.057	2.873
	(2.291)	(0.328)**	(0.355)**	(0.793)**
Marginal effect	-	-0.798	-0.968	2.737
		(0.303)**	(0.342)**	0.517**
No. hours w/o subsidy	-	*2.1*	*2.6*	*1.5*
No. hours with subsidy	0.2	1.3	1.6	4.2
Urban 14-17				
Coefficient	-3.123	-1.042	-1.613	5.072
	(1.756)	(0.284)**	(0.313)**	(1.037)**
Marginal effect	-	-0.837	-1.559	2.128
		(0.243)**	(0.298)**	(0.380)**
No. hours w/o subsidy	-	*2*	*3.6*	*1.3*
No. hours with subsidy	0.8	1.2	2	3.4
Urban 10-13				
Coefficient	-3.450	-1.008	-1.164	4.859
	(2.479)	(0.195)**	(0.214)**	(0.773)**
Marginal effect	-	-0.767	-0.939	3.718
		(0.159)**	(0.178)**	(0.368)**
No. hours w/o subsidy	-	*1.6*	*1.9*	*0.5*
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 listed 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, 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.

Another important point to take from this table is that when the programme has a significant impact on time at school, the increased time at school is not wholly substituted by reduced time at work. However, substitution effects are larger for older children in urban areas than for younger children: for the latter, less than one third of the increase in time spent at school comes from reduced 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, for any of the groups. This suggests that 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 a welfare programme that is ongoing in Colombia, *Familias en Acción*, on school participation and work activities of children.

In terms of school choices, 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 between 61% and 66%. We also find that time spent at work was only partially displaced by increased time spent at school, and that in fact most of this substitution comes from decreased time spent at domestic work. The largest substitution effects are observed for children aged 14-17 living in urban areas, for whom less than one third 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 conclude that household income has not been negatively affected through this channel.

References

Attanasio, O., E. Fitzsimons and A. Gomez (2005), "The Impact of a Conditional Education Subsidy on School Enrolment in Rural Colombia", *IFS Report Summary 1.*

Attanasio, O., et al (2004), "Baseline Report on the Evaluation of Familias en Acción" *IFS Report April 2004.*

Appendix

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

	TSP		TCP		Control	
Age of child	11.08	(3.31)	11.20	(3.31)	11.20	(3.29)
Child is female	0.47	(0.50)	0.47	(0.50)	0.47	(0.50)
Health insurance of head						
Unsubsidised	0.02	(0.15)	0.03	(0.17)	0.04	(0.19)
Subsidised	0.70	(0.46)	0.61	(0.49)	0.69	(0.46)
Informally subsidised	0.20	(0.40)	0.24	(0.43)	0.14	(0.35)
Age of head	44.45	(11.65)	45.59	(12.17)	45.38	(12.04)
Age of spouse	40.39	(10.94)	41.33	(11.51)	41.34	(11.55)
Single parent	0.19	(0.39)	0.21	(0.40)	0.18	(0.39)
Education level head						
None	0.29	(0.45)	0.25	(0.43)	0.28	(0.45)
Incomplete primary	0.46	(0.50)	0.50	(0.50)	0.45	(0.50)
Complete primary	0.14	(0.35)	0.14	(0.35)	0.14	(0.34)
Incomplete secondary	0.08	(0.26)	0.09	(0.28)	0.09	(0.29)
Complete secondary +	0.03	(0.18)	0.02	(0.15)	0.04	(0.19)
Education level spouse						
None	0.23	(0.42)	0.23	(0.42)	0.24	(0.43)
Incomplete primary	0.50	(0.50)	0.50	(0.50)	0.46	(0.50)
Complete primary	0.16	(0.36)	0.16	(0.37)	0.16	(0.36)
Incomplete secondary	0.08	(0.27)	0.08	(0.28)	0.10	(0.30)
Complete secondary +	0.03	(0.18)	0.03	(0.18)	0.04	(0.19)
House						
Family lives in house	0.97	(0.16)	0.98	(0.15)	0.97	(0.17)
House walls						
Brick	0.43	(0.50)	0.40	(0.49)	0.45	(0.50)
Mud	0.41	(0.49)	0.38	(0.48)	0.33	(0.47)
Good quality wood	0.12	(0.33)	0.16	(0.37)	0.18	(0.38)
Poor quality wood	0.03	(0.18)	0.05	(0.21)	0.03	(0.16)
Cardboard/none	0.01	(0.10)	0.02	(0.13)	0.02	(0.13)
Has piped gas	0.05	(0.22)	0.09	(0.29)	0.07	(0.25)
Has piped water	0.64	(0.48)	0.51	(0.50)	0.63	(0.48)
Has sewage system	0.28	(0.45)	0.19	(0.39)	0.25	(0.43)
Has rubbish collection	0.30	(0.46)	0.24	(0.43)	0.34	(0.47)
No telephone	0.92	(0.28)	0.92	(0.27)	0.90	(0.29)
Communal telephone	0.02	(0.14)	0.02	(0.14)	0.01	(0.10)
Private telephone	0.06	(0.25)	0.06	(0.24)	0.08	(0.28)
Toilet connected to sewage	0.50	(0.50)	0.50	(0.50)	0.52	(0.50)
Own house	0.68	(0.47)	0.65	(0.48)	0.65	(0.48)
Rented house or in mortgage	0.09	(0.29)	0.09	(0.28)	0.08	(0.27)
Occupied house without legal agreement	0.04	(0.19)	0.03	(0.17)	0.07	(0.25)
House in usufruct	0.18	(0.39)	0.23	(0.42)	0.20	(0.40)
Householder suffered from violence 2000-2002	0.03	(0.18)	0.03	(0.16)	0.04	(0.20)
Mins to nearest school	13.19	(17.67)	14.88	(18.98)	13.15	(17.90)
Municipality variables						
Altitude	600.85	(682.77)	659.91	(736.50)	517.06	(729.52)
# urban public schools	6.87	(6.19)	10.35	(8.75)	7.78	(9.52)
# rural public schools	36.57	(29.39)	48.39	(24.37)	29.64	(26.00)
# students per teacher	22.44	(5.99)	22.68	(3.73)	22.58	(5.62)
Class m^2 per student	2.93	(2.12)	2.84	(2.47)	2.50	(1.89)
Urban population 2002	13744.1	(15824.1)	16313.8	(17700.3)	14760.40	(18064.26)
Rural population 2002	12715.5	(7995.53)	18275.07	(9515.4)	12176.01	(12521.43)
Average income 1999	127511.9	(36749.7)	136385.2	(39356.3)	163112.05	(46802.31)

Region of residence			
Atlantic	0.38 (0.48)	0.43 (0.50)	0.43 (0.50)
Oriental	0.21 (0.40)	0.20 (0.40)	0.22 (0.41)
Central	0.26 (0.44)	0.25 (0.44)	0.21 (0.41)
Pacific	0.15 (0.36)	0.11 (0.31)	0.13 (0.34)
N	7,077	7,580	10,330

Notes: Sample includes households with at least one child aged 8-17 inclusive at the follow-up survey. Standard deviations in parentheses. N refers to the number of individuals.

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.