Children's Schooling and Work in the Presence of a Conditional Cash Transfer Program in Rural Colombia

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I. Introduction

Conditional cash transfer (CCT) programs, aimed at alleviating long-run poverty by fostering the accumulation of human capital among children living in indigent households and at reducing current poverty through the transfer payment, have attracted much attention in recent years. Such welfare programs have long been established in developed countries, with widespread targeting of tuition subsidies to less well-off families, but are relatively new to less developed economies. A conditional food transfer program, the Food for Education program, was one of the first of its kind to be implemented in the

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early 1990s in Bangladesh, although the first large-scale conditional cash transfer program was PROGRESA (now known as Oportunidades) launched in Mexico in 1997.[1] More recently, CCT programs have been implemented extensively in developing countries, such as Colombia, Nicaragua, Honduras, Brazil, Argentina, Ecuador, and Turkey, with the main aim of inducing parents to send their children to school. In this study, we evaluate the effect of the CCT program Familias en Acción (FA) on children's school and work participation. This program has been operating in rural parts of Colombia since 2002.

The upsurge in conditional cash transfer programs in developing countries has been matched by widespread evaluation of such programs, with the evidence all pointing to positive impacts on school enrollment (for a review, see Rawlings and Rubio 2005; and Handa and Davis 2006). The first contribution of this study is to add to and corroborate this existing body of evidence using the Familias en Acción welfare program (in so doing, it also extends the results contained in Attanasio et al. [2005, 2006]). Evidence that the program has increased school enrollment does not imply a reduction in child labor of the same magnitude, however, as time spent at work and school may not be perfectly substitutable. The second contribution of the study is to investigate the effects of the program on the time spent by children in school and work activities. This, of course, has some bearing on current household poverty, but also the extent to which the intensive margin of schooling responds to the program may be of greater policy relevance than the extensive margin. The evidence on this is, however, less pervasive, with some exceptions such as Skoufias and Parker (2001) and Rubio-Codina (2002), who find positive impacts of PROGRESA on time at school and negative impacts on time at work.

The program Familias en Acción was not randomly assigned across localities. However, considerable effort 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 areas, and we will provide evidence in Section III that this effort was rewarded. Nonetheless, we cannot rule out the fact that the areas may differ in unobserved dimensions relevant to the outcomes of interest. We thus estimate the effects of the program within a difference-in-differences framework, using pre- and postprogram data on outcomes, and conditioning on a large range of household- and municipality-level characteristics.

We find that the program increased school enrollment rates of 14–17-year-old children quite substantially, by between 5 and 7 percentage points. It

[1] See Schultz (2004) on the PROGRESA program, and Ahmed and Del Ninno (2002) on the Food for Education program.

increased the already high enrollment of 8–13-year-old children by between around 1 and 3 percentage points.

For work-related outcomes, we find that the effects of the program are generally largest for younger children, whose participation in domestic work decreased by around 10–13 percentage points after the program but whose participation in income-generating work remained largely unaffected. 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. This finding reinforces the extensive-margin finding of Ravallion and Wodon (2000) that children's school and work participation are not perfectly substitutable.

The paper proceeds as follows. In Section II, we describe the program and the context of the rural Colombian communities in which it was implemented. Section III provides a discussion of the surveys as well as some descriptive statistics relating to school enrollment and time use before the program started. In Section IV, we present our results, first for school enrollment and then for time spent in various work activities and school. Section V concludes.

II. The Familias en Acción Program

The Familias en Acción program is aimed at alleviating poverty by fostering human capital accumulation among the poorest households in Colombia. Modeled on the Mexican PROGRESA (now Oportunidades), it consists of conditional subsidies for investments into education, nutrition, and health. Such interventions are typically justified by positive externalities that human capital might confer, by the existence of liquidity constraints, and/or by other reasons such as excessive discounting of the future utility of children by parents or myopia (see Das, Do, and Ozler [2005] for a synthesis of the theoretical arguments underlying conditional cash transfers). While positive externalities could justify making the transfer conditional, that is, paid only if the household complies with certain conditions, an unconditional transfer should be sufficient to overcome liquidity constraints.

The largest component of the program is the education one, targeted at families with children aged 7 through 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 are 14,000 pesos (US$6.15) and 28,000 pesos (US$12.30) for children attending primary and secondary school, respectively.[2] Making the grant conditional on school attendance ef-

[2] These are the amounts in 2002, with an average annual exchange rate of US$1 = 2.275 Colombian pesos.

fectively decreases the relative price of education (Ravallion and Wodon 2000). The level of the grant was chosen so as to substitute, at least in part, the income the household would forgo if increased schooling involved 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 affect their decision to send their child(ren) to school, so the grant is effectively an unconditional transfer that increases household income. However, it might still bring about changes in household behavior, due not only to the increase in income but also to the additional income being managed by a female member of the household.[3]

A second subsidy is available for improving nutrition. A flat-rate monthly monetary supplement of 46,500 pesos (approximately US$20.45) is provided to mothers of 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 program took place in two stages. First, 622 out of the 1,098 municipalities in Colombia were deemed eligible to qualify for the program, on the basis of fulfilling the following criteria: (i) it has fewer than 100,000 inhabitants and is not a departmental capital, (ii) it has basic education and health infrastructure, (iii) it has a bank, and (iv) the municipality administrative office has relatively up-to-date welfare lists and other important official documents. Next, eligible households were identified in qualifying towns. Eligibility was established on the basis of a six-level welfare indicator, SISBEN, which is determined from the first principal component of a number of variables related to poverty. SISBEN has been used in Colombia to target most preceding welfare programs, as well as for the pricing of utilities (see Vélez, Castaño, and Deutsch 1998). This indicator is updated regularly, and at the time of the survey was last updated in December 1999. FA was targeted to households registered as SISBEN level 1 (in extreme poverty), living in target municipalities, and with children younger than 18.

The program was funded by a loan from the World Bank and the Inter-American Development Bank (IADB) to the Colombian government in 2000, to cover the costs of running the program for 3 years. It started operating in 2001 or 2002, depending on the municipality. The sequential phasing in had important implications for the evaluation methodology, discussed in Section

[3] The perceived importance of the intrahousehold mechanism is implicit in the fact that most CCTs are paid to mothers.

IV. In the first 2 years of the program, 340,000 households were registered to participate. The program has recently been expanded to an additional 60,000 households and is currently being piloted in deprived urban areas.

III. 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 municipality characteristics. Second, we show trends in school enrollment for 3 years—two of which are preprogram for one set of treatment areas, and one of which is preprogram for the other set of treatment areas (we return to this below). This not only gives a flavor as to how school enrollment rates vary across areas but also alerts one to possible anticipation effects of the program, in other words, individuals changing current behavior in the knowledge (or anticipation) that they were to receive the subsidy in the future. We then move on to compare work participation and time allocation across treatment and control areas before and after the program. Finally, we take a look at socioeconomic determinants of education and work choices in the absence of the program.

A. 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 FA program. For political reasons, random allocation of the program was not feasible, so instead it was decided to construct a representative stratified sample of treatment municipalities and to choose control municipalities among those that belonged to the same strata but that were excluded from the program.[4] 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 life. Control areas satisfied most of the criteria for eligibility, with the exception of the presence of a bank. The final evaluation

[4] Randomization was not feasible because the program was intended as one way of alleviating the effects of the deep recession that affected Colombia in 2000–2001, and it was therefore deemed important to deliver the program in the largest number of municipalities possible in the shortest possible time frame. Moreover, the government was keen to develop the program quickly as the presidential elections were approaching.

sample is made up of 122 municipalities, 57 of which are treatment and 65 of which are controls.[5]

In each municipality we randomly sampled approximately 100 eligible households for inclusion in the evaluation sample. We ended up with a sample of around 11,500 households that were interviewed between June and October 2002. Between July and November 2003 (the second wave) we succeeded in recontacting and obtaining complete interviews from 10,742 households, representing around 94% of the original sample.[6] The first data collection was scheduled to take place before the program started in the treatment municipalities, to provide a baseline survey to control for any systematic preprogram differences between treatment and control towns. Unfortunately, political pressure resulted in the program starting in 26 out of 57 treatment municipalities before the fieldwork commenced.[7] In what follows we refer to the municipalities in which the program started early as "early-treat" areas and the remainder as "late-treat" areas.[8] This means that both surveys in early-treat areas took place when the program was already under way; in late-treat areas it was under way at the time of the second survey, although knowledge of it was widespread at the time of the first survey and registration had even begun. Thus school enrollment at the first survey is directly affected either by the program (early treat) or by the knowledge that it was to be received in the near future (late treat). For these reasons, retrospective information on school enrollment was collected in the first survey, so as to provide a baseline free of contamination for the evaluation. It was not feasible to collect data on time use retrospectively, however, although we are less concerned about anticipation effects along this dimension, as unlike school enrollment, child labor was not subject to any conditionality. Thus, our baselines for the school enrollment and time use analyses refer to different periods: for the school enrollment analysis, we use retrospective data collected at the first survey, and for the time use analysis we use data collected at the first survey.

The surveys contain detailed information on a wide range of individual and household characteristics, including the household sociodemographic structure,

[5] Budgetary considerations prevented there from being more than 122 municipalities in the evaluation sample. To take into account possible correlations within these clusters, standard errors are clustered at the municipality level throughout the analysis.

[6] Note that a third survey was completed in April 2006, although it is not used in this study.

[7] Program officials stated repeatedly to the evaluation team that which municipalities started first was a random process determined by the order in which the paperwork had been administered in the central office.

[8] Throughout the paper, "treatment" is used to refer to both early-treat and late-treat areas taken together.

dwelling conditions, household assets, household member education levels, use of health care services, children's and mother's anthropometric indicators, household consumption, labor 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 through visits to local markets.

B. Characteristics across Treatment and Control Areas in the First Survey

The evaluation methodology and thus the credibility of the results (both of which we come back to in more detail in Secs. IV and V), ultimately rest on the choice of an appropriate comparison group on the basis of which to construct the counterfactual. This is because our evaluation methodology is based on a comparison of outcomes before and after the program in the 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.

Table A1 in the appendix presents average values of observed municipality characteristics in treatment and control areas at the time of the first survey. Most characteristics are not statistically different from each other between the two areas; while some variables relating to service delivery differ, such as a lower number of hospitals and small health care centers in control areas, it should be noted that this is due to the smaller populations on average in control municipalities. We should point out, in any case, that later on we correct for the imbalance using propensity score matching. We will provide more information on the balancing between control and treatment areas of the whole set of household and individual characteristics in Section IV.

C. School Enrollment across Treatment and Control Areas

A key aim of the study is to evaluate the effects of the program 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 program started are comparable. As discussed in Section III.A, school enrollment data were collected retrospectively at the time of the first survey, and these data constitute our baseline data. School enrollment is defined on the basis of whether the child is registered at school in the academic year corresponding to the survey. Table 1 provides a comparison of school enrollment rates across late-treat, early-treat, and control areas, for three periods: baseline, first survey, and second survey, separately for relatively more urbanized and

TABLE 1
ENROLLMENT RATES IN TREATMENT AND CONTROL AREAS IN BASELINE, FIRST AND SECOND SURVEYS (%)

	Late Treat	Early Treat	Control
Rural 14–16:			
Baseline	52.74	55.13	45.96
Survey 1	58.48	58.91	42.36
Survey 2	65.67	71.23	53.16
Rural 8–13:			
Baseline	85.80	89.74	82.41
Survey 1	91.68	92.92	83.20
Survey 2	92.92	94.04	87.91
Urban 14–16:			
Baseline	70.11	80.56	67.74
Survey 1	75.26	82.94	67.61
Survey 2	82.18	88.50	75.88
Urban 8–13:			
Baseline	90.28	94.93	90.36
Survey 1	95.23	95.15	90.31
Survey 2	96.82	97.26	93.32

Note. Baseline data refer to a preprogram period; survey 1 data relate to a period during which the program was in place for early-treat but not late-treat areas; survey 2 data relate to a postprogram period for all treatment areas.

relatively more rural areas,[9] and for 8–13- and 14–16-year-olds.[10] These groups are chosen on the basis of the sharp reduction in school enrollment in Colombia at age 14 observed in our data.

There are a couple of points to take from the table, both of which highlight the importance of controlling for preprogram enrollment in evaluating the effects of the program on school choices, but for different reasons. The first is that there appear to be preprogram differences in school enrollment in treatment and control areas before the program: school enrollment in control areas is generally lower than in treatment areas. The second is that one cannot rule out the possibility of anticipation effects: increases in school enrollment

[9] From here on, we use the terms "urban" and "rural" for simplicity. "Urban" refers to the *cabecera municipal* of the rural municipality. This is the center of government in each particular municipality, and it is expected to have at least 3,000 inhabitants and to have various public facilities, including a city hall, a school, and a health center, among other public buildings. "Rural" refers to the more remote parts of the municipality.

[10] As the baseline data are collected retrospectively from individuals aged 7 through 17 at the first survey, we do not observe the enrollment rates of individuals who were 17 years old at the baseline (because they were 18 at the first survey), so the upper age cut-off in table 1 is 16. We also omit 7-year-olds because in practice many children do not start school until age 7: remember we are considering as an outcome variable school enrollment in the academic years corresponding to the survey, and a child who is 7 years old at the time of a survey may have been 6 at the start of the academic year corresponding to that survey and, hence, may not yet have actually started school.

between the baseline and the first survey are observed in treatment areas but not in control areas.

Another point worth noting from the table is that even though school enrollment in treatment areas is higher than in control areas after the program, it is not clear that the increase in school enrollment from before to after the program is higher in treatment areas: for the late-treat/control comparison, it depends on to the extent to which we believe that late-treat enrollment rates in the first survey (i.e., before the program started in these areas) reflect anticipation effects, that is, are contaminated by the program. We return to this in Section IV.

D. Work Participation and Time Allocation in Treatment and Control Areas

We carry out the same exercise for participation 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 retrospectively (owing to the difficulty in obtaining accurate retrospective information on these outcomes). As this means that we have no preprogram data on these for early-treat areas, we exclude them from the time use analysis. In table 2, we show participation in income-generating work[11] and domestic work, at the extensive and intensive margins, before and after the program, in late-treat and control areas. Time allocation is measured in hours and fractions thereof and relates to the day before the interview.[12] Note that we have no information on time use for children younger than 10.

In line with the descriptive statistics relating to school enrollment, children in control and late-treat areas differ also in their time allocation before the program, as shown in table 2. In particular, most children in late-treat areas participate more in income-generating and domestic activities before the program (with the exception of rural 14–17-year-olds) and go to school for fewer hours, compared to children in control areas. This is in contrast to what we observed for school enrollment, which is higher in late-treat than in control areas before the program.

Relating to the period after the program, we see that first, work participation in late-treat areas is generally lower than before the program (with the ex-

[11] This pools together work in the labor market and the family business, due to the very low employment rates of children in the labor market, particularly of those aged 10–13, whose participation in labor market at the baseline is around 2.7%.

[12] We drop children interviewed on a Sunday or a Monday, 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 the first and second survey, respectively. This selection is based on the timing of interviews, which is independent of household characteristics and choices.

TABLE 2
PARTICIPATION IN AND TIME ALLOCATED (IN HOURS PER DAY) TO ACTIVITIES IN LATE-TREAT AND CONTROL AREAS BEFORE AND AFTER THE PROGRAM

	Before		After	
	Late Treat	Control	Late Treat	Control
Rural 14–17:				
Participation in income-generating activities (%)	14.85	14.91	18.71	19.22
Participation in domestic work (%)	70.58	64.28	65.49	61.31
Hours of income-generating work	1.04	1.03	1.45	1.45
Hours of domestic work	2.59	2.23	2.12	2.04
Hours of school	1.77	2.31	2.61	2.28
Rural 10–13:				
Participation in income-generating activities (%)	4.79	2.97	4.73	3.59
Participation in domestic work	69.76	61.93	65.66	66.01
Hours of income-generating work	.24	.15	.22	.23
Hours of domestic work	1.93	1.41	1.34	1.46
Hours of school	2.27	3.49	4.20	3.69
Urban 14–17:				
Participation in income-generating activities (%)	13.16	7.94	12.94	11.89
Participation in domestic work	69.08	57.40	60.91	56.83
Hours of income-generating work	.83	.49	.75	.84
Hours of domestic work	2.08	1.23	1.20	1.18
Hours of school	1.13	3.18	3.46	3.25
Urban 10–13:				
Participation in income-generating activities (%)	4.20	1.59	3.08	2.23
Participation in domestic work (%)	69.29	51.91	61.53	56.40
Hours of income-generating work	.20	.05	.14	.11
Hours of domestic work	1.59	.84	.88	.85
Hours of school	1.20	3.76	4.23	4.06

Note. The period before (after) the program refers to the first (second) survey. Note that statistics relating to hours are not conditional on participation in that activity.

ception of rural 14–17-year-olds), in contrast to control areas in which it is generally higher. Second, time spent at school tends to increase more in late-treat than in control areas. Third, time spent at domestic work generally decreases by more in late-treat than in control areas for all groups. Finally, time spent at income-generating work generally decreases in late-treat areas, whereas it increases in control areas. All of this evidence is consistent with there being desirable effects of the program on child time allocation: in Section IV we go on to the causal analysis of the effects.

E. Determinants of School Enrollment and Work Participation

As noted already, observing as much information as possible about the municipalities and households in our sample is important to the quasi-experimental evaluation set-up. It allows us not only 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. The importance of such characteristics for education and work choices in various developing countries has been well established (e.g., 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 our analysis.

We estimate a probit model for school enrollment and work participation across individuals aged 8 (10 for work) through 17. We use the data from the first survey only, and for this reason we omit early-treat areas, as the relative importance of determinants in this period may be contaminated by the existence of the program in these areas. We control for all of the variables listed in tables A1 and A2 in the appendix, and show the effects of those of particular interest in table 3.

Turning to the effects for school enrollment, 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). The effects of parental education are in line with previous results in the literature on educational choices: higher education levels are associated with a higher probability of school enrollment, 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 municipality, is negative, as expected, but not statistically different from zero. The distance to the nearest school, which is a proxy for the cost of going to school, decreases participation in school.

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 enrollment decisions, although 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.

IV. Evaluating the Impact of Familias en Acción on School and Work

We estimate the effect of the program on school and work participation at both the extensive and intensive margins, using a difference-in-differences methodology combined with matching. After controlling for observables, this

TABLE 3

DETERMINANTS OF SCHOOL ENROLLMENT AND WORK PARTICIPATION AT THE FIRST SURVEY, LATE TREAT, AND CONTROL

Regressors	School Enrollment (1)	Income-Generating Work (2)	Domestic Work (3)
Female child	.0503	−.0658	.2197
	(.0068)**	(.0061)**	(.0140)**
Household owns house	.0145	−.0048	−.0055
	(.0243)	(.0083)	(.0139)
Distance to nearest school	−.0006	−.0001	−.0001
	(.0003)*	(.0002)	(.0003)
Education level head:			
Incomplete primary	.0206	−.0031	.0120
	(.0091)*	(.0070)	(.0143)
Complete primary	.0345	−.0071	−.0257
	(.0116)**	(.0084)	(.0241)
Incomplete secondary	.0675	−.0192	−.0563
	(.0126)**	(.0129)	(.0289)
Complete secondary +	.0653	−.0030	−.0898
	(.0159)**	(.0227)	(.0480)
Education level spouse:			
Incomplete primary	.0296	−.0090	−.0020
	(.0093)**	(.0068)	(.0207)
Complete primary	.0642	−.0216	−.0208
	(.0096)**	(.0096)*	(.0248)
Incomplete secondary	.0766	−.0241	−.0595
	(.0097)**	(.0106)*	(.0335)
Complete secondary +	.0940	−.0385	.0234
	(.0085)**	(.0146)**	(.0432)
Municipality variables:			
Number of urban schools	−.0001	−.0020	−.0104
	(.0014)	(.0017)	(.0033)**
Number of rural schools	−.0006	.0004	−.0009
	(.0003)*	(.0002)	(.0007)
Average municipality monthly child wage	−.0041	−.0027	−.0658
	(.0105)	(.0089)	(.0240)**
Observations	12,691	7,885	7,883

Note. We also control for variables listed in tables A1 and A2 of the appendix. The sample size for work participation is lower than that for school enrollment, 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 school enrollment (work), sample comprises 8(10)–17-year-olds at the first survey in late-treat and control areas. Note that average municipality monthly income is the average across the working children in the municipality.

* Denotes statistical significance at the 1%–5% level.

** Denotes statistical significance at the 1% level or less.

is essentially the difference between outcomes before and after the program in treated areas, adjusted by the change experienced by the control group over the same period, to account for time trends that are unrelated to the program. Identifying the program effect using this approach assumes that there are no unobserved factors affecting outcomes differentially in treated and control areas.[13] As we observe and control for detailed household- and municipality-level information, much of our concern about omitted variable bias is alleviated.

However, the assumption that time trends are the same in treated and control areas needs to be further examined. While this assumption cannot be tested, it is useful to compare trends in school enrollment between treatment and control areas before the program started. If it is the case that they are similar, it is likely that they would have been the same in the posttreatment period in the absence of the program. We test this using data from the Colombian Demographic Health Surveys (DHS) of 1990, 1995, and 2000, all of which are preprogram periods. One problem, however, is that we observe very few of our control municipalities in the DHS, so the test is likely to be sensitive to this. For this reason, we also present the trends using as controls all municipalities that did not go on to receive the treatment.[14] This increases our sample size, although at the cost of introducing a relatively more heterogeneous set of control municipalities.[15] Given this, it is likely to represent the worst case scenario. We condition on SISBEN level 1 households (i.e., households that would be eligible for the program), and we control for the same set of household-level regressors as in the evaluation. Results in table 4 show that in neither case can we statistically reject the hypothesis that the preprogram year dummies (and hence time trends) are the same for treatment and nontreatment areas at the 5% level of statistical significance. This evidence is reassuring, although we should acknowledge that some doubt remains regarding preprogram trends in the actual treatment and control areas.

Further reassuring evidence that control and treatment areas displayed similar trends in the preprogram period is the fact that the evolution of per capita household labor income (observed retrospectively in our survey) in treatment and control areas in the 3 years 1999, 2000, and 2001 is very similar in

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

[14] The DHS does not contain all of the municipalities that are in the Familias survey we use. When we restrict the sample to SISBEN level 1 (would-be eligible) households, between 5% and 8% of surveyed households in the DHS, we observe 13 treatment municipalities in 1990, 21 in 1995, and 19 in 2000. We observe 8 control municipalities in 1990, 2 in 1995, and 2 in 2000.

[15] We observe 28 nontreatment municipalities in 1990, 23 in 1995, and 22 in 2000.

TABLE 4
PREPROGRAM TIME TRENDS IN SCHOOL ENROLLMENT IN TREATMENT AND NONTREATMENT AREAS

	(1)	(2)
Treatment area	.0117	.0865
	(.0756)	(.0624)
Year = 1995	−.1770	−.0002
	(.1697)	(.0822)
Year = 2000	.0646	.0742
	(.0940)	(.0928)
Treatment area × Year = 1995	.3237	.1481
	(.1764)	(.0952)
Treatment area × Year = 2000	.0149	−.0007
	(.1016)	(.0995)
Number of observations	1,441	1,876

Note. 1990 is the reference year. Column 1 includes as control municipalities a subset of those used in the evaluation; col. 2 includes as control municipalities all municipalities sampled in DHS that did not go on to become treated. Standard errors, clustered at municipality level, in parentheses. We control for a similar set of regressors as in table A2 of the appendix.

treatment and control areas prior to the program (table A3 in the appendix). Although this relates to a nonoutcome variable, it is not inconsistent with our common trends assumption, which is encouraging.

Moreover, the assumption of common time effects is likely to be violated if individuals living in treatment areas change behavior in anticipation of the program. This would mean that outcomes in treatment areas in the period before the program would not be representative of outcomes in treatment areas in the absence of the program. To minimize this possibility, we use school enrollment data from two years before the program as our baseline.

A. School Enrollment

To evaluate the effect of the program on school enrollment, we use retrospective data on enrollment collected at the time of the first survey as our measure of preprogram, or baseline, enrollment. This is because, as discussed in Section III.A, the program had already started in early-treat areas at the time of the first survey; in late-treat areas, even though the program had not started, knowledge of it was widespread and registration had begun for some individuals. It turns out that such anticipation effects are important.

The specification that we use to estimate the effects of the program on enrollment is

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

for $t = 0, 1$, and 2. The baseline is $t = 0$, $t = 1$ is the first survey (preprogram for late treat and postprogram for early treat), and $t = 2$ is the second survey (postprogram for all treated areas). The 1.( ) notation denotes that the variable has a value of one if the condition in parentheses holds and zero otherwise. The rest of the notation is defined as follows: $Y_{it} = 1$ if individual i is enrolled in school in period t and 0 otherwise; $P = 1$ for late treat $= 1$ or early treat $= 1$ and 0 otherwise; $A = 1$ for late treat $= 1$ and $t = 1$ and 0 otherwise; $T = 1$ for ($P = 1$ and $t = 2$) or (early treat $= 1$ and $t = 1$) and 0 otherwise; and Z_{it} is a set of preprogram individual, household, and area characteristics.

The above specification is estimated using individuals who are aged 8–17 at the time of the second survey and who are observed in both the first and second surveys. The effect of the program, estimated separately for 8–13- and 14–17-year-olds in urban and rural areas, is given by α_4.[16] Note that α_3 estimates the anticipation effect in late-treat areas at the first survey. We assume throughout that $u_{it} \sim \mathrm{IN}(0, \alpha^2)$ and estimate equation (1) using a probit model.

One criticism of the parametric specification is that extrapolation beyond the region of "common support," that is, the region over which treated individuals have a counterpart in the group of controls, can lead to misleading inferences. To address this concern, we first match treatment and control observations using kernel-weighted propensity score matching, and impose common support by dropping 10% of the treatment observations at which the propensity score density of the control observations is the lowest.

Tables A5–A8 in the appendix show that characteristics of the matched treatment and control samples are more similar than those of the unmatched samples, across all four groups. While a few significant differences between the treated and controls remain in the matched sample, this should be weighed up against the fact that we consider a very detailed set of variables. Note, moreover, that in the small number of cases in which there are significant differences, it is generally with respect to variables that are insignificant in the propensity score estimation. We also see from the tables that matching improves substantially the overall quality of the comparison, as shown by both the reduction in the mean and median absolute standardized biases by around 50% for each of the groups, and the decrease in the Pseudo R^2 of the probit model for the selection of treated households.

[16] Note that in a different specification we allowed for the treatment impact to differ depending on the treatment duration, as early-treat areas at the time of the second survey have been receiving the program for a longer period of time than late-treat areas at the second survey or than early-treat areas at baseline. However, we found no evidence of the program impacts varying with length of exposure to the treatment.

TABLE 5
EFFECT OF PROGRAM ON SCHOOL ENROLLMENT, PROPENSITY SCORE MATCHING

	Rural 14–17	Rural 8–13	Urban 14–17	Urban 8–13
Effect	.0693*	.0223	.0331	.0056
	(.0311)	(.0241)	(.0257)	(.0181)
N	1,873	3,648	1,439	2,579

Note. Coefficients are estimated using propensity score matching using a difference-in-differences approach. Age denotes age at the second survey. Common support is imposed by dropping 10% of 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 250 replications, are in parentheses. We control for variables listed in tables A1 and A2 of the appendix.
* Denotes statistical significance at the 1%–5% level.

TABLE 6
MARGINAL EFFECT OF PROGRAM ON SCHOOL ENROLLMENT AND ANTICIPATION EFFECTS, PROBIT MODEL

Probit Model	Rural 14–17	Rural 8–13	Urban 14–17	Urban 8–13
Treated (α_4)	.0662	.0282	.0470	.0140
	(.0232)**	(.0111)**	(.0123)**	(.0066)*
Anticipation (α_3)	.0631	.0149	.0300	.0242
	(.0291)**	(.0144)	(.0193)	(.0057)**
N	1,873	3,648	1,439	2,579

Note. Marginal effects are estimated from a probit model using eq. (1). *N* is the number of treated individuals falling within the common support in the postprogram period. Standard errors, clustered at the municipality level, are in parentheses. Control for variables listed in tables A1 and A2 of the appendix.
* Denotes statistical significance at the 1%–5% level.
** Denotes statistical significance at the 1% level or less.

We next estimate the effects of the program on individuals who fall within the common support using difference-in-differences propensity score matching. This exercise is carried out in order to provide a benchmark for comparison with the parametric specification. We see from this, in table 5, that the program increased school enrollment, particularly of older children, although the effects are imprecisely estimated.

To increase efficiency, we estimate equation (1) parametrically, again using a difference-in-difference approach. To minimize any extrapolation bias within the parametric specification, we restrict the analysis to individuals who lie within the common support, as determined using the methods described above. One caveat is that this means that we cannot infer anything about the impact of the program on individuals who fall outside the common support, who are often the ones who benefit most from it. Note, however, that the estimates obtained when we do not restrict the sample to those within the common support are in fact very similar (shown in table A4 of the appendix). Table 6 presents the results from estimating equation (1). Similar to table 5, it shows that the program had positive and significant impacts on school enrollment, especially for older age groups, of just under 7 percentage points in rural areas and around 5 percentage points in urban areas. It had a lower effect, of just under 3 percentage points, on the enrollment 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 fairly similar across econometric specifications, although they are more precisely estimated in the parametric one. Table 6 also shows that late-treat areas enrollment rates were already contaminated by the program even before it was implemented (marginal impact α_3). This underlines the importance of collecting data well in advance of when programs start, so as to have a clean baseline for evaluation.

B. Time Allocation

We have seen in the previous section that the program has been effective in its main objective: contributing to human capital accumulation via increasing enrollment in school. This increased participation in 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's welfare of increased school enrollment depend on whether the CCT program reduced time spent by children in work-related activities, vis-à-vis affecting leisure time. Moreover, the effect of the program on the child's contribution to household labor 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 program. However, it is worth bearing in mind that children attending school spend, on average, 5.5 hours in school per day[17] and that it takes just under 15 minutes on average for children to reach school (table A2). With school days being relatively short, increased school participation need not imply reduced work participation.

We use detailed time use data from before and after the program to assess how the program has affected the amounts of time spent by children in work activities as well as at school. As discussed in Section III, there are no retrospective data on this outcome. This means that we have no preprogram information on time use for early-treat areas, given that they were already receiving the program by the time the first survey was collected. We thus have no way of controlling for fundamental differences in time use between early-treat and control areas, and for this reason we chose to exclude early-treat areas from all of the analysis that follows.

[17] The school day in Colombia is relatively short and does not incorporate breaks. Moreover, it is not uncommon for schools to offer two short schedules to different pupils on the same day to facilitate demand.

There is still the concern that we cannot estimate separately how much of the difference in time allocation between late-treat and control areas at the time of the first survey is due to fundamental differences in time uses between the two areas and how much is due to late-treat individuals changing behavior in anticipation of the program. However, if anticipation effects in work choices exist, our estimates of the effect of the program on child time allocations would represent lower bounds on the actual effects, assuming that individuals reduce participation in work in anticipation of the program, an assumption that is consistent with the overall treatment effects we go on estimate. To further alleviate our concerns, we control for retrospective school enrollment (the baseline for the school enrollment analysis), although the results are not sensitive to omitting it from the set of regressors. Therefore, we are fairly confident that the data collected in the first survey are sufficient to capture fundamental differences in time uses.

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

To ascertain whether participation in various activities changed due to the program, 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 first and second surveys, across late-treat and control areas, to estimate the following equation:

$$\begin{aligned} P_{it}^{j} = {} & \beta_{0j} + \beta_{1j}1.(t = 2) + \beta_{2j}(\text{late treat}) \\ & + \beta_{3j}T + \psi' Z_{it} + u_{it}, \end{aligned} \tag{2}$$

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 and 0 otherwise; and $T = 1$ for late treat $= 1$ and $t = 2$ and 0 otherwise.

All other variables are as defined in Section IV.A. As our outcome variable is discrete, we estimate equation (2) using a probit model, for each of the activities listed above. The results are shown in table 7.

We see from column 1 that the program had no significant impact on participation in income-generating activities. The effects of the program on participation in domestic work are much larger, as can be seen from column 2. The program decreased participation in domestic work of both old and young children in urban areas by just under 10 and 13 percentage points,

TABLE 7

IMPACT OF THE PROGRAM ON PARTICIPATION IN DIFFERENT ACTIVITIES

	Participation In:		
	Income-Generating Work (1)	Domestic Work (2)	All Work (3) = (1) & (2)
Rural 14–17:			
Marginal effect	.0005	−.0312	.0040
	(.0259)	(.0421)	(.0402)
N	789	791	789
Participation w subsidy (%)	18.71	65.49	80.66
Rural 10–13:			
Marginal effect	−.0093	−.0638	−.0744
	(.0095)	(.0496)	(.0484)
N	1,034	1,057	1,057
Participation w subsidy (%)	4.73	65.66	68.97
Urban 14–17:			
Marginal effect	−.0362	−.0967	−.1499
	(.0198)	(.0436)*	(.0443)**
N	570	571	571
Participation w subsidy (%)	12.94	60.91	69.28
Urban 10–13:			
Marginal effect	−.0091	−.1290	−.1417
	(.0063)	(.0483)**	(.0489)**
N	723	745	745
Participation w subsidy (%)	3.08	61.53	62.87

Note. Marginal effects are estimated using eq. (2). *N* is the number of treated individuals in the second period. Standard errors, clustered at the municipality level, are in parentheses. Control for variables listed in tables A1 and A2 of the appendix. "Participation w subsidy" is the average participation of each group in treated area after the program.

* Denotes statistical significance at the 1%–5% level.

** Denotes statistical significance at the 1% level or less.

respectively. These imply corresponding counterfactual participation rates of just under 71% and above 74% compared to the observed postprogram rates of just under 61% and 62% for old and young children, respectively.

To sum up, we see in column 3 that the program significantly reduced participation in work in urban areas only.[18] This suggests that participation of children in income-generating activities or domestic work responds less to the program in rural than in urban areas, which is perhaps not surprising if children are important labor inputs in agriculture and there is greater flexibility in hours worked for children in this sector.

However, this analysis ignores intensity of work activity, which is the more important margin from both welfare and income-generating viewpoints. More-

[18] Note that participation in income-generating activities and participation in 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.

over, if the FA subsidy is not sufficient to replace fully forgone child income, we may expect to observe larger impacts at the intensive rather than at the extensive margin. We estimate the impact of the program on the amount of time allocated to each activity using the following specification:

$$h_{it}^{j} = \gamma_{0j} + \gamma_{1j}1.(t = 2) + \gamma_{2j}(\text{late treat}) + \gamma_{3j}T + \theta' Z_{it} + u_{it}, \tag{3}$$

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 (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 both the estimated coefficient, which is the discrete change in the latent dependent variable as a result of the program, and the marginal effect, which represents the average increase in time allocated to a particular activity if a household receives the program.[19] To assess the magnitude of these effects, we also report the average number of hours supplied after the program by children in treated areas. The main message to emerge from table 8 is that the program increased significantly the amount of time spent in school for all children and decreased time at work for almost all groups.

The magnitudes of the impacts, however, are very different across groups: the estimated impact is largest for young children in urban areas, who spend around 4.5 hours more per day in school after the program compared to their counterparts in control areas. Time at school also increases substantially after the program for urban children aged 14–17, by 3.8 hours as shown in table 8, as well as for rural children aged 10–13, by 2.5 hours. For children aged 14–17 in rural areas, however, the effect of the program on the number of hours at school, although low, at around 1 hour, is statistically different from zero at conventional levels. Their time spent at work is not significantly reduced by the program, which as noted already may be indicative of inelastic child labor supply in rural areas.

Another important point to take from this table is that when the program 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

[19] In contrast to the estimate γ_{3j}, this effect takes into account the nonlinearity of the dependent variable.

TABLE 8
IMPACT OF THE PROGRAM 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	.06	−.52	−.39	2.22
	(1.09)	(.30)	(.33)	(.93)*
Marginal effect	.01	−.33	−.31	.96
	(.14)	(.18)	(.26)	(.46)*
No. hours with subsidy	1.5	2.1	3.6	2.6
Rural 10–13:				
Coefficient	−2.18	−.90	−1.04	3.13
	—	(.31)**	(.30)**	(.84)**
Marginal effect	−.04	−.54	−.64	2.48
	—	(.18)**	(.18)**	(.61)**
No. hours with subsidy	.2	1.3	1.6	4.2
Urban 14–17:				
Coefficient	−3.34	−1.19	−1.78	5.21
	(1.66)*	(.27)**	(.33)**	(.87)**
Marginal effect	−.22	−.61	−1.03	3.79
	(.09)*	(.13)**	(.18)**	(.72)**
No. hours with subsidy	.8	1.2	2.0	3.5
Urban 10–13:				
Coefficient	−3.07	−1.11	−1.29	5.09
	—	(.25)**	(.25)**	(.88)**
Marginal effect	−.03	−.54	−.64	4.49
	—	(.10)**	(.10)**	(.78)**
No. hours with subsidy	.1	.9	1.0	4.2

Note. (1) The coefficients and marginal effects are estimated parametrically using eq. (3), controlling for the variables in tables A1 and A2 of the appendix, as well as for an indicator of retrospective school enrollment from the first survey. Treatment areas include late-treat only. Bootstrapped standard errors based on 200 replications, adjusted for clustering at the municipality level, are in parentheses. "No. hours with subsidy" is the average number of hours provided by each group in treated areas in the period after the program. (2) For children aged 10–13 years, it was not possible to bootstrap the standard errors of the impacts of the program 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 nonclustered standard errors (likely to be inflated even more after adjusting for clustering), we can say that the effects are not statistically different from zero.
* Denotes statistical significance at the 5% level or less.
** Denotes statistical significance at the 1% level or less.

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. These effects are quite different from the results found in Mexico where, for boys in particular, the reductions in work participation are approximately equivalent to the increases in school participation (Skoufias and Parker 2001).[20]

[20] For girls, however, the reductions in work participation tend to be lower than the increases in school participation.

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

V. Conclusion

In this study we have evaluated the effects of an ongoing large-scale welfare program in Colombia, Familias en Acción, on school and work participation of children. We find that the program increased the school participation rates of 14–17-year-old children quite substantially, by between around 5 and 7 percentage points, to reach enrollment of 64% and 82% in rural and urban areas, respectively. It also had nonnegligible effects on the enrollment of younger children, of between 1.3 and 2.8 percentage points, despite their already high participation rates in the absence of the program, at between 91% and 96%. In our analysis of the effects at the intensive margin, we found that the effects are larger in urban areas, where school attendance goes up by between 3.8 hours per day for older children and 4.5 hours per day for younger children, compared to 1 hour for older rural children and 2.5 hours for younger rural children.

The effects on domestic work participation are largest in urban areas, where participation is around 10 and 13 percentage points lower after the program, at 61% and 62% for older and younger children, respectively. Time spent at work (mainly domestic work) was reduced by less than the increase in time spent at school. These results suggest that parents are substituting other uses of their children's time, such as leisure, and are not using the conditional subsidy to replace fully the earnings from their children's work. The largest substitution effects are observed for children aged 14–17 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 there is very little evidence that the program in Colombia decreased significantly the time spent by children in income-generating activities, it seems unlikely that household income has been negatively affected through this channel.

Appendix

TABLE A1
CHARACTERISTICS OF TREATMENT AND CONTROL MUNICIPALITIES

	Treatment		Control	
Proportion of households with piped water	.88	(.14)	.88	(.13)
Proportion of households with sewage facilities	.56	(.36)	.62	(.36)
Urban population in 2001	15,935	(2,373)	13,218	(2,110)
Rural population in 2001	14,630	(1,185)	10,254	(1,389)*
Altitude	729.57	(773)	810.83	(885)
No. of urban public schools	8.70	(8.18)	6.69	(8.33)
No. of rural public schools	42.89	(29.82)	25.55	(23.44)*
No. of students per teacher	22.15	(5.04)	21.83	(5.72)
Class m^2 per student	2.97	(2.58)	2.75	(1.92)
Number of hospitals in 2002	.82	(.38)	.64	(.48)*
Number of health care centers in 2002	.89	(1.13)	.81	(1.14)
Number of small health care centers in 2002	5.15	(4.26)	3.29	(4.99)*
Number of pharmacies in 2002	9.77	(7.43)	6.53	(6.14)*
Proportion of municipalities where a health care provider employee deserted in 2001	.12	(.33)	.06	(.24)
Proportion of municipalities with a strike in health care providers in 2001	.32	(.47)	.16	(.37)*
Region of residence:				
Atlantic	.33	(.48)	.29	(.46)
Oriental	.25	(.43)	.31	(.47)
Central	.30	(.46)	.29	(.46)
Pacific	.12	(.33)	.11	(.31)

Note. Standard deviations in parentheses. A * indicates that variable is statistically different across treatment and control areas (based on *t*-tests at the 5% level of significance).

TABLE A2

SUMMARY OF MEAN CHARACTERISTICS ACROSS LATE-TREAT, EARLY-TREAT, AND CONTROL AREAS AT THE FIRST SURVEY

	Late Treat	SD	Early Treat	SD	Control	SD
Age of child	11.084	2.908	11.22	2.91	11.17	2.9
Child is female	.476	.499	.475	.499	.466	.499
Health insurance of head:						
Unsubsidized	.027	.163	.038	.191	.053	.223
Subsidized	.691	.462	.633	.482	.699	.459
Informally subsidized	.177	.382	.206	.404	.135	.342
Age of head	44.438	11.424	45.228	11.84	45.247	11.751
Age of spouse	40.338	10.62	41.065	11.142	41.118	11.147
Single parent	.179	.383	.199	.4	.17	.375
Education level of head:						
None	.272	.445	.234	.423	.265	.442
Incomplete primary	.437	.496	.469	.499	.431	.495
Complete primary	.145	.352	.137	.344	.133	.34
Incomplete secondary	.074	.262	.083	.277	.083	.275
Complete secondary +	.03	.172	.022	.146	.037	.19
Education level of spouse:						
None	.22	.414	.209	.407	.227	.419
Incomplete primary	.479	.5	.472	.499	.438	.496
Complete primary	.157	.364	.153	.36	.151	.358
Incomplete secondary	.076	.264	.08	.272	.092	.289
Complete secondary +	.027	.162	.032	.176	.037	.189
House walls:						
Brick	.434	.496	.405	.491	.456	.498
Mud	.421	.494	.383	.486	.334	.471
Good quality wood	.104	.306	.15	.357	.171	.377
Poor quality wood	.031	.173	.045	.207	.024	.152
Cardboard/none	.01	.099	.016	.127	.015	.123
Has piped gas	.052	.222	.094	.292	.073	.261
Has piped water	.649	.477	.518	.5	.636	.481
Has sewage system	.277	.447	.193	.395	.247	.431
Has rubbish collection	.296	.456	.245	.43	.336	.472
No telephone	.918	.275	.919	.273	.906	.292
Communal telephone	.019	.137	.02	.141	.011	.103
Private telephone	.063	.243	.061	.239	.083	.276
Toilet connected to sewage	.498	.5	.506	.5	.524	.499
Own house	.694	.461	.652	.476	.655	.475
Rented house or in mortgage	.086	.281	.085	.279	.075	.263
Occupied house without legal agreement	.04	.195	.032	.175	.068	.252
House in usufruct	.18	.384	.231	.422	.202	.402
Householder suffered from violence 2000–2002	.029	.169	.026	.16	.04	.197
Minutes to nearest school	13.953	17.373	15.707	19.634	13.663	17.254
Sample size	7,077		7,580		10,330	

Note. Sample of households with at least one child aged 8–17 in the second survey.

TABLE A3

PREPROGRAM TIME TRENDS IN PER CAPITA HOUSEHOLD LABOR INCOME IN TREATMENT AND CONTROL AREAS

Late-treat area	−2.2708 (.5573)**
Year 2000	.6158 (.2442)*
Year 2001	1.0513 (.2849)**
Late-treat area × Year = 2000	−.1294 (.2878)
Late-treat area × Year = 2001	.0836 (.3576)
N	8,003

Note. Dependent variable is per capita household labor income. 1999 is the reference year. Standard errors, clustered at municipality level, in parentheses. We control for a similar set of regressors as in table A2 of the appendix.
* Denotes statistical significance at the 5% level or less.
** Denotes statistical significance at the 1% level or less.

TABLE A4

MARGINAL EFFECT OF PROGRAM ON SCHOOL ENROLLMENT AND ANTICIPATION EFFECTS, PROBIT MODEL, WHOLE SAMPLE

Probit Model	Rural 14–17	Rural 8–13	Urban 14–17	Urban 8–13
Treated (α_4)	.0659	.0249	.0566	.0126
	(.0223)**	(.0117)*	(.0125)**	(.0057)**
Anticipation (α_3)	.0633	.0146	.0299	.023
	(.0280)**	(.0139)	(.0189)	(.0057)
N	2,081	4,053	1,598	2,865

Note. Marginal effects are estimated using eq. (1). N is the number of treated individuals in the post-program period. Standard errors, clustered at the municipality level, are in parentheses. Control for variables listed in table A1 and A2 of the appendix.
* Denotes statistical significance at the 1%–5% level.
** Denotes statistical significance at the 1% level or less.

TABLE A5

COMPARISON OF CHARACTERISTICS ACROSS MATCHED AND UNMATCHED SAMPLES, GROUP 1

	Unmatched Sample			Matched Sample		
	Treated	Control	*p*-Value Difference	Treated	Control	*p*-Value Difference
Female child	.445	.422	.186	.444	.441	.821
Health insurance of head:						
Unsubsidized	.020	.034	.013	.021	.026	.256
Subsidized	.681	.744	.000	.692	.713	.150
Informally subsidized	.194	.104	.000	.174	.163	.404
Age of head	47.410	47.683	.478	47.377	47.449	.835
Age of spouse	42.830	43.293	.187	42.752	42.837	.789
Single parent	.147	.141	.626	.141	.144	.823
Education level head:						
Incomplete primary	.544	.516	.121	.555	.545	.557
Complete primary	.103	.101	.838	.100	.101	.956
Incomplete secondary	.032	.034	.681	.032	.026	.238
Complete secondary +	.013	.020	.142	.014	.014	.939
Education level spouse:						
Incomplete primary	.557	.531	.151	.561	.562	.944
Complete primary	.121	.119	.893	.122	.101	**.043**
Incomplete secondary	.032	.039	.300	.032	.026	.238
Complete secondary +	.014	.009	.198	.014	.012	.533
House walls:						
Mud	.508	.461	.010	.505	.541	**.024**
Good quality wood	.156	.221	.000	.161	.144	.142
Poor quality wood	.040	.022	.005	.042	.035	.319
Cardboard/none	.012	.013	.984	.012	.009	.265
Has piped gas	.009	.008	.826	.010	.006	.189
Has piped water	.388	.407	.282	.401	.388	.433
Has sewage system	.061	.059	.828	.060	.074	.085
Has rubbish collection	.063	.062	.874	.064	.072	.318
No telephone	.969	.964	.413	.967	.953	**.027**
Communal telephone	.016	.018	.787	.018	.025	.140
Toilet connected to sewage	.363	.415	.003	.375	.387	.455
Own house	.990	.985	.205	.990	.980	**.017**
Household suffered from violence 2000–2002	.028	.053	.000	.030	.046	**.011**
Mean absolute bias		7.542			3.739	
Median absolute bias		4.784			3.389	
Pseudo R^2		.157			.026	

Note. The absolute standardized bias is taken over all regressors. Pseudo R^2 of probit model for the selection of treated households. Bolded *p*-values indicate that differences are significant at less than the 5% level.

TABLE A6

COMPARISON OF CHARACTERISTICS ACROSS MATCHED AND UNMATCHED SAMPLES, GROUP 2

	Unmatched Sample			Matched Sample		
	Treated	Control	*p*-Value Difference	Treated	Control	*p*-Value Difference
Female child	.485	.463	.089	.485	.486	.944
Health insurance of head:						
Unsubsidized	.019	.037	.000	.021	.019	.627
Subsidized	.660	.730	.000	.672	.709	**.001**
Informally subsidized	.207	.125	.000	.187	.175	.162
Age of head	43.954	44.275	.290	43.995	44.047	.848
Age of spouse	39.126	39.749	.029	39.138	39.077	.811
Single parent	.124	.138	.123	.122	.131	.224
Education level head:						
Incomplete primary	.516	.496	.120	.516	.517	.964
Complete primary	.132	.113	.030	.134	.133	.968
Incomplete secondary	.041	.049	.130	.042	.038	.388
Complete secondary +	.012	.015	.466	.013	.011	.303
Education level spouse:						
Incomplete primary	.528	.513	.258	.531	.534	.841
Complete primary	.144	.145	.890	.145	.138	.347
Incomplete secondary	.042	.048	.266	.043	.046	.550
Complete secondary +	.014	.014	.998	.015	.011	.173
House walls:						
Mud	.496	.428	.000	.490	.529	**.001**
Good quality wood	.181	.243	.000	.186	.174	.189
Poor quality wood	.041	.034	.146	.040	.032	.063
Cardboard/none	.011	.015	.219	.011	.008	.184
Has piped gas	.007	.007	.885	.008	.010	.290
Has piped water	.398	.404	.684	.408	.405	.789
Has sewage system	.057	.054	.630	.059	.074	**.008**
Has rubbish collection	.057	.064	.213	.059	.071	**.037**
No telephone	.964	.967	.561	.964	.954	**.044**
Communal telephone	.023	.019	.288	.022	.024	.519
Toilet connected to sewage	.336	.379	.001	.347	.367	.079
Own house	.990	.987	.190	.990	.986	.074
Household suffered from violence 2000–2002	.031	.050	.000	.033	.052	**.000**
Mean absolute standardized bias		6.876			2.848	
Median absolute standardized bias		3.677			1.582	
Pseudo R^2		.142			.019	

Note. The absolute standardized bias is taken over all regressors. Pseudo R^2 of probit model for the selection of treated households. Bolded *p*-values indicate that differences are significant at less than the 5% level.

TABLE A7

COMPARISON OF CHARACTERISTICS ACROSS MATCHED AND UNMATCHED SAMPLES, GROUP 3

	Unmatched Sample			Matched Sample		
	Treated	Control	*p*-Value Difference	Treated	Control	*p*-Value Difference
Female child	.464	.467	.898	.461	.459	.907
Health insurance of head:						
Unsubsidized	.048	.061	.089	.049	.055	.518
Subsidized	.653	.670	.340	.662	.671	.600
Informally subsidized	.175	.161	.287	.170	.164	.658
Age of head	46.556	47.063	.219	46.674	46.703	.946
Age of spouse	43.340	43.603	.490	43.363	43.300	.872
Single parent	.292	.219	.000	.264	.264	.985
Education level head:						
Incomplete primary	.354	.377	.185	.366	.376	.583
Complete primary	.171	.153	.161	.158	.160	.859
Incomplete secondary	.113	.095	.106	.110	.103	.560
Complete secondary +	.034	.040	.355	.037	.032	.521
Education level spouse:						
Incomplete primary	.406	.386	.249	.409	.420	.528
Complete primary	.180	.159	.125	.167	.165	.896
Incomplete secondary	.108	.119	.354	.108	.112	.704
Complete secondary +	.043	.051	.280	.046	.038	.305
House walls:						
Mud	.265	.224	.008	.265	.275	.529
Good quality wood	.064	.112	.000	.068	.056	.161
Poor quality wood	.031	.019	.045	.028	.024	.440
Cardboard/none	.013	.014	.833	.013	.012	.806
Has piped gas	.185	.138	.000	.185	.178	.611
Has piped water	.828	.849	.109	.824	.857	**.017**
Has sewage system	.474	.404	.000	.459	.509	**.008**
Has rubbish collection	.563	.559	.830	.546	.566	.272
No telephone	.847	.850	.809	.856	.858	.913
Communal telephone	.018	.004	.000	.012	.012	.968
Toilet connected to sewage	.730	.628	.000	.716	.731	.354
Own house	.969	.962	.303	.968	.971	.665
Household suffered from violence 2000–2002	.028	.029	.927	.027	.029	.722
Mean absolute standardized bias		7.227			3.134	
Median absolute standardized bias		4.172			1.992	
Pseudo R^2		.108			.027	

Note. The absolute standardized bias is taken over all regressors. Pseudo R^2 of probit model for the selection of treated households. Bolded *p*-values indicate that differences are significant at less than the 5% level.

TABLE A8

COMPARISON OF CHARACTERISTICS ACROSS MATCHED AND UNMATCHED SAMPLES, GROUP 4

	Unmatched Sample			Matched Sample		
	Treated	Control	*p*-Value Difference	Treated	Control	*p*-Value Difference
Female child	.485	.489	.737	.485	.487	.871
Health insurance of head:						
Unsubsidized	.053	.067	.030	.056	.060	.535
Subsidized	.649	.670	.097	.664	.673	.492
Informally subsidized	.179	.146	.001	.162	.159	.759
Age of head	43.399	43.941	.100	43.676	43.769	.781
Age of spouse	40.101	39.931	.587	40.171	40.137	.916
Single parent	.256	.184	.000	.233	.249	.175
Education level head:						
Incomplete primary	.360	.365	.739	.372	.386	.284
Complete primary	.164	.155	.374	.153	.156	.785
Incomplete secondary	.142	.128	.125	.139	.136	.720
Complete secondary +	.049	.064	.015	.049	.041	.190
Education level spouse:						
Incomplete primary	.383	.363	.122	.388	.398	.448
Complete primary	.180	.168	.232	.170	.170	.982
Incomplete secondary	.144	.140	.659	.139	.141	.849
Complete secondary +	.056	.063	.252	.057	.049	.210
House walls:						
Mud	.272	.250	.065	.277	.282	.693
Good quality wood	.067	.120	.000	.071	.053	**.007**
Poor quality wood	.038	.018	.000	.033	.028	.343
Cardboard/none	.016	.018	.530	.014	.016	.704
Has piped gas	.153	.124	.002	.151	.147	.676
Has piped water	.826	.818	.477	.819	.838	.074
Has sewage system	.467	.410	.000	.462	.497	**.012**
Has rubbish collection	.549	.568	.164	.547	.564	.225
No telephone	.858	.860	.861	.859	.857	.796
Communal telephone	.016	.005	.000	.010	.010	.976
Toilet connected to sewage	.704	.641	.000	.699	.711	.316
Own house	.953	.959	.311	.953	.956	.661
Household suffered from violence 2000–2002	.021	.034	.003	.022	.024	.570
Mean absolute standardized bias		7.050			2.824	
Median absolute standardized bias		4.027			1.461	
Pseudo R^2		.091			.026	

Note. The absolute standardized bias is taken over all regressors. Pseudo R^2 of probit model for the selection of treated households. Bolded *p*-values indicate that differences are significant at less than the 5% level.

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