The Extent of the Impact:* Identifying Externalities

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Abstract

Conditional Cash Transfer (CCT) programs have been implemented as a powerful tool to fight poverty. Even though the evaluations show significantly positive results, externalities could arise that may cause the problem of underestimation or overestimation of the programs' total effects. Using information of a CCT implemented in Colombia, this paper estimates the impact on the targeted population as well as the indirect effects on individuals not directly involved in the program.

The results suggest that the program has been very efficient at improving the school attendance of individuals. Nevertheless, the program has not changed their working situation: children and teenagers continue to work. In terms of externalities, women who have children inscribed in the program experienced a significant decrease in their labor supply. Finally, cross-village externalities indicate that treated children exert an important effect on the schooling decisions of their untreated peers: if children attend school more regularly, it is very likely that the school attendance of their peers will also increase.

JEL-Classification: C93, D04, D62, I25, I38

Keywords: school attendance, labor supply, propensity score matching, externalities.

1 Introduction

There is a growing interest in the net impact of Conditional Cash Transfer (CCT) programs. In the last decade, many countries have implemented some kind of CCT and because the evaluations have demonstrated important positive effects, policy makers have begun to spread this type of intervention into different areas. Currently CCT programs cover millions of people around the world.[1] Nonetheless, CCTs are not magic bullets. One of the main criticisms concerns the programs' evaluations and the fact that they do not account for the externalities that might arise as a result of the interventions. These indirect effects can be either positive or negative, and therefore, excluding them in the evaluations will underestimate or overestimate the global assessment of the programs (Miguel & Kremer, 2004), (Lehmann, 2010).

The relevance of CCTs relies on the change in the beneficiaries' behavior through monetary incentives. Therefore, such programs can have effects that reach beyond the individual beneficiaries because externalities might arise from a spread of the resources to non-beneficiaries through market transactions, and from the effect of treated individuals on their peers through the presence of social interactions (Moffitt, 2001). Nonetheless, measuring the extent of the externalities is not an easy task, mainly because of the lack of information and the quality of it. Most of the programs' evaluations collect data on the individuals directly involved in the program, and just a few of them include information about the community. The reason for this is that extended evaluations are too costly in terms of time and monetary resources.

Regarding the direct impact of CCTs, there is a vast body of literature that verifies their important effects on the targeted population. In general, most of the antipoverty initiatives reveal a significant boost in the school attendance of children and teenagers. However, results are not conclusive regarding school attainment. Studies have also demonstrated an increase in the use of health providers, as well as in the frequency of health checkups (de Janvry & Sadouled, 2004), (de la Briere & Rawlings, 2006). In the same way, many papers indicate an important improvement in the nutrition of newborns and pregnant women. CCTs also seem to decrease the probability of children working. However, this result depends on whether the measured outcomes account for

[1] In absolute terms, CCT programs range from 11 million families in Brazil to 215,000 households in Chile. There exist several pilot programs with a few families in Kenya and Nicaragua. The budgets vary from 0.5% of the GDP in Brazil, Mexico, and Ecuador to about 0.08% in Chile. For an extended analysis see World Bank Report (2013).

in-home activities (Galiani & Mc.Ewan, 2011).

Studies that measure the secondary impact of CCTs, found positive and negative effects. In general, CCTs increased the school attendance of children not directly targeted by the program but who share spaces (such as school and villages) with treated children (Lalive & Cataneo, 2009), (Galiani & Mc.Ewan, 2011), (Jishnu *et al.*, 2005), (de la Briere & Rawlings, 2006). In the same way, the program had an enormous effect not only on the health status, but also on the schooling decisions of their peers (Miguel & Kremer, 2004). Finally, when estimating the impact of CCTs on the labor supply, the results were heterogenous. Ribas & Soares (2011) concluded that the transfers increased the participation of the head of the household in the labor market. However, the results differed depending on the geographic location of the families.

Using information of a CCT launched in Colombia named "Familias en Acción," this paper evaluates the program's impact on the schooling and labor decisions made by the individuals directly involved, and the indirect impact on other individuals, or externalities of the program, using two frameworks. First, by analyzing the households' compositions, it is possible to determine the effect of the program on the individuals living within a treated household, who are not obliged to follow any specific behavior. Second, because the Program was designed to work at the village level, by using the geographic location, it is possible to estimate the cross-village externalities generated on the schooling decisions of the children living in control villages closely located to a treated village.

Results confirm that the program is very efficient at increasing school attendance, and, at the same time, decreasing the probability of working of children and teenagers. Externalities have shown to be significant, and, in spite of criticism regarding misguided incentives, the program generated a significant increase in the probability of adults members in treated families having employment. Nevertheless, this result varies in intensity and significance depending on the age and gender of the individuals. Finally, cross-village externalities created an important effect on the school attendance of children living in villages near treated villages.

This paper intends to contribute to the literature in three ways. First, due to the program's design, the timing of the treatment has varied within the treated villages. We have taken advantage of this characteristic to distinguish the results between short- and mid-term effects. It is not possible to talk about long-term effects because evaluation data is available for only the first 3 years after the implementation. Second, some

documents have analyzed the responses to the treatment of the targeted group, which consist of children and teenagers, and on their peers. However, not many studies have exploited the families's composition and their responses to the treatment. Finally, the inclusion of the cross-village analysis has allowed us to determine the presence of social interactions caused by the effect of treated children on their non-treated peers.

The document is organized as follows. In the next section we present the background information on Colombia and its educational and labor market characteristics. It also describes the program, "Familias en Accion," its design, structure, and goals. In a third section, the paper presents the identification framework and the data and main descriptive statistics are presented as well as the econometric model to be estimated. Finally, in the last section, the results and conclusions are presented.

2 Background Information

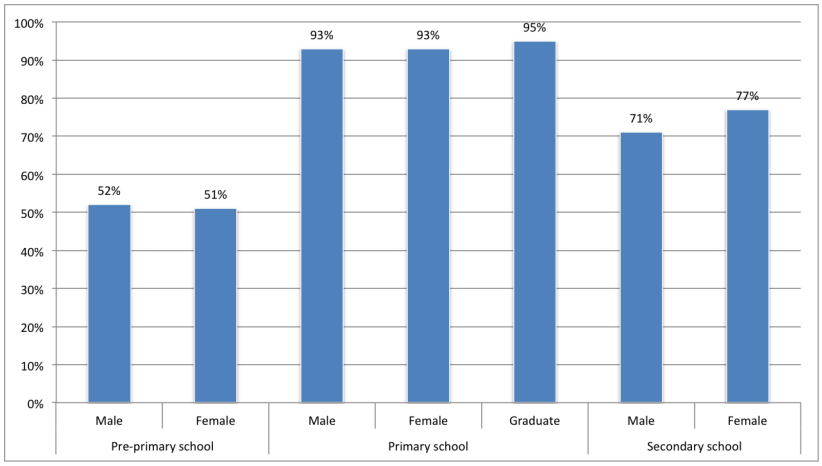
2.1 Schooling Conditions in Colombia

The school system in Colombia consists of six different levels: initial education, preprimary school, primary school, secondary school, and superior education. Despite important improvements in the last decades in its educational system, many problems remain unsolved in the country. In fact, primary school has been public and mandatory since 1920, but it is only recently that the matriculation rate has reached a significant level. Between the years 2007 and 2010, pre-primary school attendance was close to 55%, primary school attendance was about 95%, and secondary school about 75% (OECD Report, 2010). See Fig. (1).

This last result is a reflection of the situation at the national level. However, when looking more closely at the statistics, it is evident that the situation varies from one department to another.[2] Specifically, there is an important difference in the educational indicators between urban and rural areas. In a technical report, Ramírez *et al.* (2007) show that, for example, while the illiteracy rate in some departments is close to 5%, in other areas of the country this rate is closer to 20%. Moreover, when evaluating different indicators, it is clear that in spite of the high attendance rate at initial levels

[2]Colombia is divided into 32 departments and one unique district: Bogotá. The departments are autonomous and have their own political and administrative organization. Each department is divided into municipalities; in total, the country has 1120 municipalities. For more detailed information, see Banco de La República (2012).

Figure 1: Schooling Conditions in Colombia



Source: UNICEF, Colombia (Data 2007-2010). Web page: Basic Indicators Statistics.
Notes: The table reports the school enrollment by schooling level.

of education, the number of students that stay in high school and graduate is still very low. In a study Cox *et al.* (2008) determine the elements that explain this situation in Colombia:

1. The cost of attending high school is relatively high: transportation costs (time and money), matriculation costs, and costs of materials.

2. The opportunity cost to continue in high school becomes considerably high.

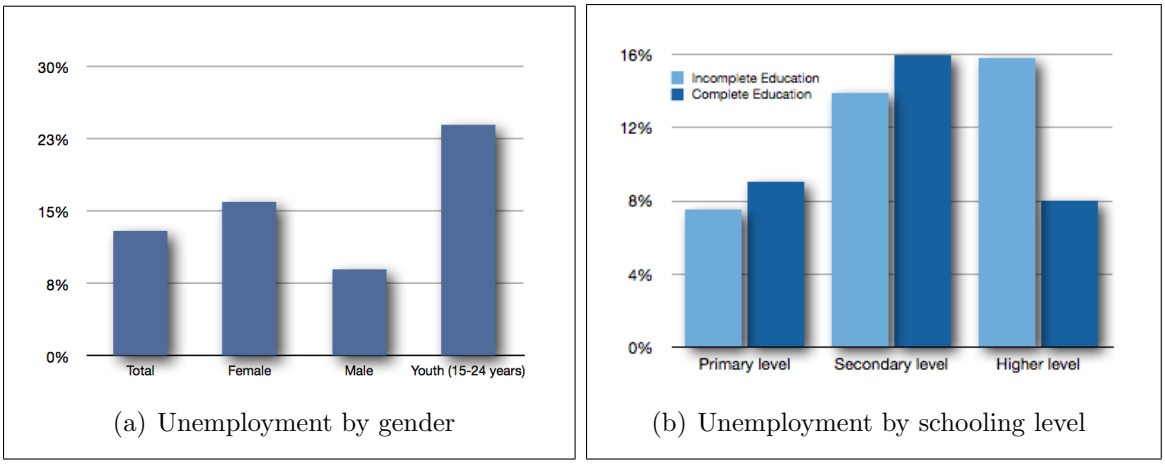
3. The quality of education is low and there is a general low perception of the relevance of what is taught.

4. There is no significant increase in the job opportunities for a high school graduate compared to an elementary school graduate.

2.2 Labor Conditions in Colombia

In spite of significant improvements in working conditions in recent years, Colombia still faces important levels of unemployment compared to other countries in the OECD group, or even to other Latin American countries. The unemployment rate is approximately 10% for men, 17% for women, and 25% for young individuals (15 to 24 years old) in the year 2010 (OECD Report, 2010). See Fig. 2 (a).

Figure 2: Labor Conditions in Colombia



(a) Unemployment by gender

(b) Unemployment by schooling level

Source: OECD Report: Colombia Economic Assessment (Data 2010).

One puzzling situation is that people with complete secondary school have more trouble finding a job than people who have completed primary school. Fig. 2(b) shows that the difference in the unemployment rates between primary and secondary graduates is about 8%. The numbers also indicate that people with a third-level degree, however, have a lower level of unemployment. This situation could be the consequence of two possible causes. The first is that a large segment of economic activity involves labor intensive jobs where a high level of specialization is not required, as in the case of agriculture.[3] Individuals therefore do not need more than primary school to start working. The second possibility is more complex and is related to the quality of education. If people are better off with low levels of education, it means that the acquired knowledge in higher levels does not satisfy the requirements of the market: what people learn in secondary school does not give them any additional skills for working in more productive activities.

Finally, among people with working status, the country faces high rates of informality: 50% to 70% of the total workforce. The informality is very high in rural areas and among people with low schooling levels. All these conditions together create very unstable working conditions, and most of the opportunities people have access to are related to jobs with low productivity, and low wages (OECD Report, 2010).

[3]By 2007, the agricultural sector in Colombia represents approximately 14% of GDP. This sector satisfies domestic and international demand. It generates about 21% of total employment in the country. For a more detailed information, see PNUD Colombia (2007).

3 Familias en Acción

"Familias en Acción" is a Conditional Cash Transfer program (CCT) launched by the Colombian Government in 2001 and 2002. The main goal of the program is to bolster the human capital among the poorest individuals living in small villages in the country.[4] The program grants poor families monthly payments contingent on specific behavior, such as sending their children to school, attending frequent medical checkups and attending monthly lectures about health and nutrition.

The structure of "Familias en Acción" is based on a similar program in Mexico: PROGRESA.[5] The program works at the village level. In an earlier stage, it selected a number of villages where the program was implemented (treated villages), and a number of villages that functioned as the control group. To be eligible for the program, the villages were required to meet two criteria: 1. enough health and educational infrastructure to cover the demand in the village, and 2. a financial institution capable of transferring the monetary grants to the eligible individuals.

The villages that fulfilled these two requirements became part of the universe of potential beneficiaries. The sample selection of treated villages was accomplished using a stratified and probabilistic design, controlling for regional, socio-economic, and infrastructure variables. The control villages were chosen with a controlled matching procedure, taking into consideration the population density, the urban-rural composition of the village, and an index of life quality. The criteria for selection of the control villages was to choose villages that were very similar to the treated ones; the only difference was that control villages did not qualify for the program primarily because their political and administrative authorities were not interested.

The program is widespread in the country, and it covers about 99.54% of the Colombian territory. Fig.(3) shows the geographic location of the villages participating in

[4]The selected villages have a maximum of 100.000 inhabitants. See, Methodological Instructive: Familias en Acción (2002).

[5]PROGRESA is Mexico's principal antipoverty initiative. Launched by the government in 1997, the program awards cash grants to families living in poverty. The grants are conditioned on criteria such as preventive health check-ups and regular school attendance for children. Its main goal is to break the intergenerational transmission of poverty by increasing the investment that treated families make in the human capital of their children.

the program. The map differentiates between villages that received the treatment, and the villages part of the control group. Within the treatment, we divide the group in two: villages that received the treatment first, and villages that received the treatment later. The program was designed to begin in all the villages at the same time; however, due to administrative and political circumstances, it was implemented in some villages before others. In total, we have information on 66 control villages, 26 villages treated for 1 year, and 31 villages treated for 2 years.

The first evaluation began in 2002 and it was planned to continue for three more years. Within the treated villages, the beneficiaries of the Program are poor families, or families in vulnerable situation with children younger than 18 years old. The program determined which families were poor by using the information from a national database (SISBEN) that tracks the socio-economic conditions of households and categorizes them with a poverty index that goes from 1 to 6, where 1 represents the poorest group. Only families in the 1 were eligible to receive the cash grants.

The program covers three areas including: nutrition, health and education:

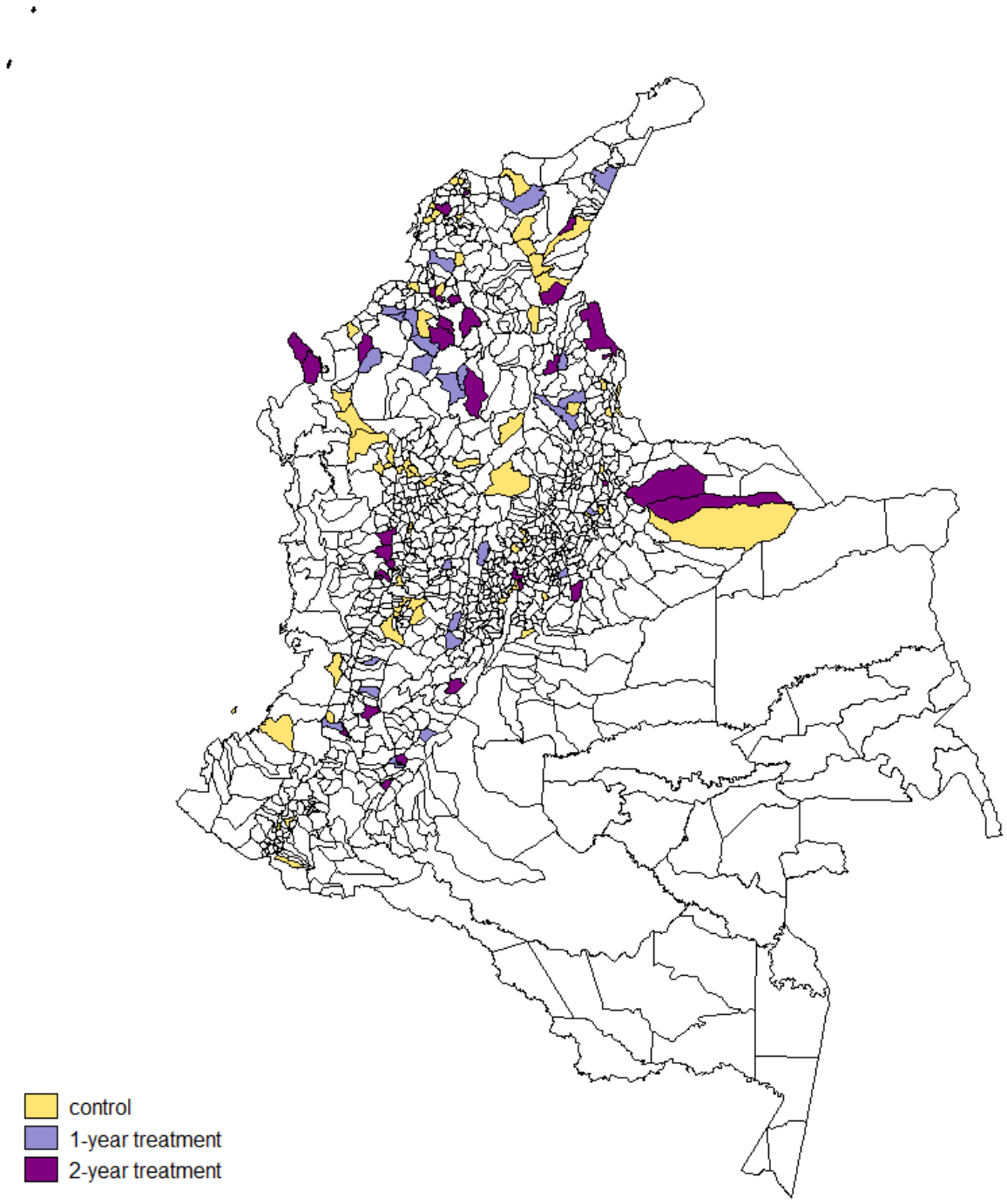
1. The nutritional supplement program grants cash payments to treated families with children younger than 7 years old. The conditions for receiving the grants include vaccinations for infants and regular health checkups for infants and pregnant mothers. During the first year of evaluation, in 2001, the grants were $40.000 pesos per month ($17 USD); by 2002, the grants increased to $46.500 pesos ($18.50 USD).[6]

2. The health component incorporates a series of activities the family must pursue in order to receive the transfers. In particular, mothers commit to arrange regular health checkups for their children.

3. The education component requires the eligible children to have a minimum level of school attendance. The grants for this component vary from $14.000 pesos ($5.60 USD) for children in primary school to $28.000 pesos ($11.16 USD) for children in secondary school.

[6]The exchange rates are from the Banco de la República, institution that calculates the rates using information from the Superintendencia Financiera de Colombia. The exchange rates are calculated using the annual average. The reported values used the exchange rate of the year 2002.

Figure 3: Coverage of Familias en Acción



Source: Departamento Nacional de Planeamiento, Colombia.

Note: Familias en Acción was implemented at the village level. The map shows the geographic localization of the control and treated villages. Within treated villages, it differentiates between the villages that received the treatment first (purple), and the villages that received the treatment later (light purple).

4 The Identification Framework

4.1 The Data

"Familias en Acción" had three evaluation surveys. The first evaluation began in 2002, and the remaining two were planned for the next 2 years. Table (1) details the program's structure and the information outlines the composition of the program at family and at individual levels depending on the village status.

For the baseline, for example, the total number of households included was 11,462; among them, 6,773 were treated and 4,689 were part of the control group. This number decreased after the first evaluation, when the same families were interviewed. However, 5% of them could not be found. The reasons were that some families either moved to a different department, or during the second evaluation, none of their family members were younger than 18 years old. This number changed again for the second evaluation, where many more families could not be reached.

Table 1: Program's Structure

	Baseline			1st. Evaluation			2nd. Evaluation		
	Total	*Treated*	*Control*	**Total**	*Treated*	*Control*	**Total**	*Treated*	*Control*
Households	11,462	6,773	4,689	10,742	6,316	4,426	9,566	6,676	2,890
Individuals	68,608	40,340	28,268	64,337	37,641	26,696	57,411	40,097	17,314

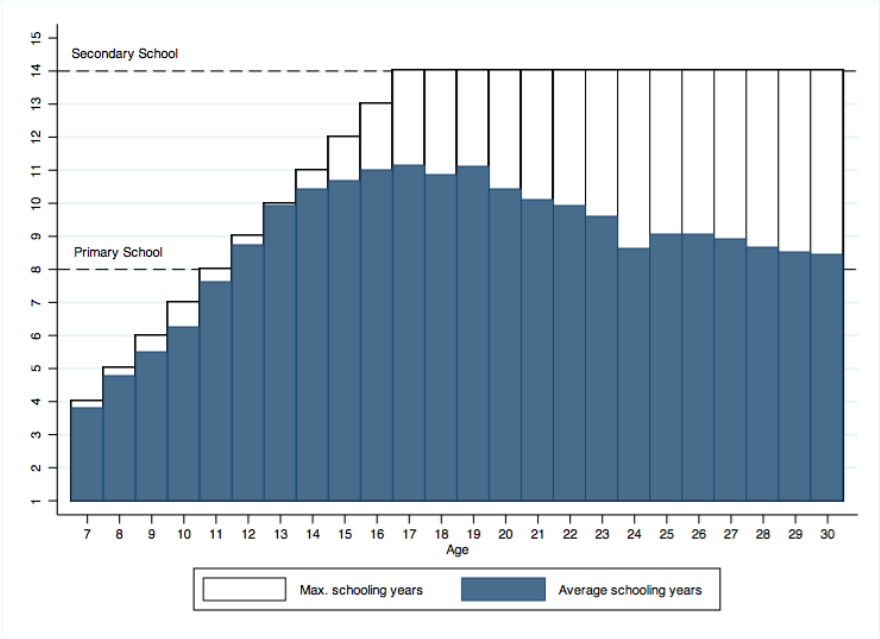
Source: Methodology Instructive. Familias en Acción. SINERGIA.
Notes: The table details the total number of households and individuals by treatment status.

The evaluation of the program was originally planned to begin with a baseline. However, when the first survey took place, some of the treated villages were already receiving the cash grants. For this reason, in the data set we have a group of villages that began receiving the cash transfers before the original design. Therefore, for each evaluation survey, this paper will divide the treated group into two: villages that have been part of the program for 1 year, and villages that have been part of the program for 2 years.

4.2 Descriptive Statistics

Table (2) details the background characteristics of the sample before the implementation of the program. The data is divided into the villages that received the treatment

Figure 4: School Attainment by Age



Source: Familias en Acción Data.

for 1 year, the villages that received the treatment for two years, and the control villages.

The sample has a great number of young individuals: 40% of the sample group is younger than 18 years old and only about 3% is older than 65 years. In terms of gender, 50% of the sample is male. Families are numerous: on average, six person live in a household, from them, at leas one is an infant, and one is a child. In addition, 21% of the households report to live with only one parent. It is important to note that most of the families are not only composed by parents and their children: different families live together in the same household. In addition, a family is very likely to be integrated by a close cousin, uncles, grandparents, and other relatives, besides close friends.

In terms of education, parents have attended no more than primary school, and almost 9% of the sample has never attended school. Fig.(4) describes the schooling decisions of the individuals. The figure compares the actual number of school years to its expected value by age, assuming high school as the maximum school level from 17 years old onwards. At early ages, school attainment is close to the optimum: most

Table 2: Descriptive Statistics: Baseline Information

	(A)	(B)	(C)	(A-C)	(B-C)
	1-Year Treatment	2-Year Treatment	Control		
Baby	0.188	0.204	0.185	0.003	0.019^{+}
				(0.004)	(0.004)
Child	0.205	0.212	0.207	-0.002	0.005
				(0.004)	(0.004)
Young	0.140	0.135	0.143	-0.003	-0.008^{+}
				(0.003)	(0.003))
Adult	0.429	0.418	0.431	0.001	-0.014^{+}
				(0.005)	(0.005)
Old	0.036	0.031	0.033	0.003*	-0.002
				(0.002)	(0.002)
Male	0.505	0.505	0.505	0.005	0.005
				(0.005)	(0.005)
Both parents at home	0.786	0.786	0.797	-0.023^{+}	-0.012
				(0.009)	(0.009)
Family's members	6.052	5.832	6.091	-0.195^{+}	-0.064
				(0.004)	(0.057)
Number of babies in family	1.099	1.242	1.117	-0.017	0.127^{+}
				(0.025)	(0.027)
Number of children in family	1.197	1.295	1.247	-0.550**	0.047*
				(0.023)	(0.025)
Mother education level	1.748	1.702	1.724	0.024	-0.022
				(0.023)	(0.024)
Father education level	1.608	1.580	1.607	0.061^{+}	-0.027
				(0.026)	(0.027)
Never attended school	0.084	0.092	0.101	-0.017^{+}	-0.009^{+}
				(0.003)	(0.003)
Years of education	8.901	8.645	8.948	-0.048	-0.304^{+}
				(0.039)	(0.040)
Working people in sample	0.636	0.624	0.633	0.003	-0.009
				(0.007)	(0.007)
Family's head work	0.837	0.809	0.819	0.018**	-0.009
				(0.008)	(0.009)
Number workers in family	1.697	1.718	1.750	-0.055^{+}	-0.032
				(0.023)	(0.025)
Agriculture	0.369	0.483	0.460	-0.090^{+}	0.023*
				(0.012)	(0.013)
Domestic worker	0.312	0.034	0.019	-0.008**	0.011^{+}
				(0.004)	(0.004)
Own activity	0.561	0.435	0.465	0.095^{+}	-0.030^{+}
				(0.012)	(0.013)
Own business	0.027	0.042	0.032	-0.005	-0.010**
				(0.004)	(0.005)
Familiar worker	0.012	0.006	0.021	-0.008^{+}	-0.014^{+}
				(0.003)	(0.003)

Significance levels: *: p<0.010 ; **: p<0.005 ; $^{+}$: p<0.001. Standard errors in parenthesis.

[1.] T-test difference between the treated and control groups.

Notes: The table details the background characteristics of the sample before the program's implementation.

children attend school, and they get the schooling years that they should get. Nonetheless, this behavior decreases over time: as children grow, the difference between the expected and the actual number of school years increases. This happens either because individuals drop school or because they perform poorly in school so they have to repeat some years. The average adult has not even completed secondary school.[7]

The working conditions in the sample indicate that a large portion of the individuals reported engaging in a payed activity. More than 80% of the families' head of household work. On average, almost two people per family work. In terms of the activity, most of the individuals develop their own activity, which, in general, is not fixed. Another portion of participants work in the agricultural sector, and as domestic workers. Most of these jobs are very unstable, meaning that people change from one activity to another easily.

Finally, columns 4 and 5, we perform a t-test on the mean differences between the treated and control groups to determine whether they are actually comparable. Because the treatment and control groups have significant differences in their background characteristics, the groups are not statistically comparable. For this reason, before the econometric modeling, we implemented a propensity score $p(x)$ matching procedure so that, in a first stage, the $p(x)$ balances each group's characteristics depending on their probability of receiving the treatment. In a second stage, a matching, one-to-one procedure pairs the closest observation in the control group to a treated observation.

Because we have two treatment groups, we estimate one $p(x)$ for each group. In the estimation of the $p(x)$, all of the variables reported in Table (2) were included. The resampling was done at the household level, so the p-score matched treated households in 1-Year treatment group with control households and treated households in 2-Year treatment group with control households. After the matching of the households, it is possible to check the balance of the sample. In the Appendix, Fig.(6) graphically shows the results reported by the p-score matching procedure. The results do not show off-support observations: all treated households found a comparable observation in the

[7]The Colombian government, among others in the region, implemented additional programs intended to increase the school years of individuals that left school at some point. These types of programs include not only formal education (primary-secondary school), they also cover technical education. The idea is to help individuals increase their productivity and therefore their options in the labor market. The program are scheduled during non-working hours, normally at night. For more detailed information, see Programa Nacional de Alfabetización y Educación Básica de Jóvenes y Adultos (2002).

control group.

Finally, Table (3) reports the mean statistics of the resampling. We performed a t-test for each group to determine whether the groups were comparable. The reported tests show that the matching procedure successfully balanced the groups: there are no significant differences between treated and control groups.

Table 3: Descriptive Statistics: After Matching

	1-Year Treatment	Control 1	Diff.	2-Year Treatment	Control 2	Diff.
Male	0.525	0.523	0.002	0.523	0.523	0.005
			(0.006)			(0.005)
Both parents at home	0.999	0.999	0.000	0.999	0.999	0.000
			(0.000)			(0.000)
Family's members	5.884	5.887	0.003	5.832	0.523	0.064
			(0.062)			(0.057)
Number of babies in family	1.141	1.132	0.009	1.288	1.298	0.001
			(0.031)			(0.034)
Number of children in family	1.217	1.261	0.044	1.334	1.316	0.018
			(0.031)			(0.003)
Mother education level	1.800	1.783	0.017	1.734	1.737	0.003
			(0.003)			(0.027)
Father education level	1.708	1.700	0.008	1.605	1.604	0.001
			(0.019)			(0.333)
Working people in the sample	0.655	0.669	0.014	0.669	0.658	0.011
			(0.008)			(0.008)
Number workers in family	1.661	1.668	0.007	1.700	1.692	0.008
			(0.025)			(0.033)
Agriculture	0.387	0.387	0.000	0.508	0.520	0.052
			(0.000)			(0.675)
Domestic worker	0.007	0.005	0.002	0.011	0.005	0.005*
			(0.002)			(0.003)
Own activity	0.563	0.569	0.006	0.431	0.425	0.006
			(0.014)			(0.015)
Own business	0.031	0.029	0.002	0.044	0.044	0.000
			(0.005)			(0.000)
Familiar worker	0.012	0.021	0.013	0.005	0.005	0.000
			(0.002)			(0.000)

Significance levels: *: p<0.010 ; **: p<0.005 ; $^{+}$: p<0.001. Standard errors in parenthesis.
[1.] T-test difference between the treated and control groups.
Notes: The table shows the means of the treated and control groups after the matching procedure.

5 Econometric Application

The estimation is divided into three subsections. The first defines the direct impact of "Familias en Acción", the second subsection details the estimation of the externalities within families, and a third subsection determines the presence of cross-village externalities.

5.1 Direct Impact

The CCT program was designed to increase the school attendance (S), and, at the same time, reduce the labor supply (L) of individuals in primary (children) and secondary school (teenagers). Each group, children and teenagers, is delimited by age. While the children (C) are between 7 and 12 years old, teenagers (T) are between 13 and 17 years old. Therefore, the group directly impacted by the program is defined by $A = \{C, T\}$, where the effect of the program can be estimated using the following equation:

$$Y_{iv}^{A} = \alpha + \gamma D_v + \sum_{h} \delta X_{ih} + \tau_t + \epsilon_{iv} \tag{1}$$

where $Y_{iv}^{A} = \{S, L\}$ stands for the schooling or labor outcome of individual i, living in village v. The super index A represents the age group in analysis: children or teenagers. D_v is the treatment indicator of the village v. The equation controls for the background characteristics, X_{ih}, of household h and individual i. This vector includes the number of siblings, the head of households' education (mother and father), the labor situation of the head of household, the number of workers and the number of infants in the family, and the productive activity of the head of household. The regression also includes dummies for the level of schooling and controls for the year of the surveys τ_t. Finally, the error term clusters the observations per village. Even though the notation describes a linear regression, the dependent variable (Y_{iv}^{A}) is a binary outcome; therefore, we use a nonlinear regression (logit) to estimate it.

The average treatment effect (ATE) is determined by:

$$E[Y_{iv}^{A}|D_{vt} = 1] - E[Y_{iv}^{A}|D_v = 0] = \gamma + \underbrace{E[\epsilon_{iv}|D_v = 1] - E[\epsilon_{iv}|D_v = 0]}_{=0} \tag{2}$$

It is possible to eliminate the second term in the equation because the matching eliminated the error associated with the assignment to the treatment. Therefore, the ATE is given by γ. If $\gamma > 0$, being part of the targeted group, and living in a treated household increases the probabilities of attending school, or working, compared to the counterpart in the control group. In the same way, if $\gamma = 0$ is not different from zero, then the program did not have an impact on the targeted group.

Table (5) details the results of Equation (1), which was estimated for different samples, by gender and timing of the treatment. Each column represents whether the sample received for 1 or 2 years the treatment. In general, being part of the treatment significantly increased the school attendance of treated individuals. If we compute the marginal effects of the parameter of interest (γ), we see that the effect of the treatment becomes stronger if a person has received the treatment for a longer period of time.[8] Specifically, for the 1-Year treatment group, the treatment increased the school attendance in 3.9%. For the 2-Year treatment group, the school attendance increased in 4.4%. This pattern is repeated when estimating the effect on children and teenagers by gender.

It is interesting that males seem to receive a greater benefit from being part to the program than females. In the same way, teenagers attended school more often than children. Nonetheless, this result might be the effect of many more teenagers not attending school in the sample, compared to the percentage of children not attending school before the program started.

Among the covariates, the number of siblings controls a potential negative impact numerous families have on the schooling decisions of individuals. The numbers show that an additional school-age sibling has a negative effect on the attendance. This result is not significant for children but it becomes very strong for female teenagers. In fact, if a family has an extra school-age member who is in the schooling age, girls between 12 and 17 years old are more likely to decrease their school attendance. Parents' education levels, as expected, have an important effect on the school attendance of their children.

In the same way, table (6) illustrates the results with respect to labor supply. It was not possible to divide the sample into children and teenagers to analyze school attendance because in the survey this question was only asked to individuals older

[8]Table (5) reports the marginal effects of the variable of interest at the bottom of the table.

than 10 years. Therefore, to determine the effect on the labor decisions of the targeted group, we use information of individuals between 10 and 17 years old. For each group, the estimations are divided by the timing of the treatment and by gender.

In general, the treatment does not have an important effect on the labor decisions of the individuals directly involved in the program. When contrasting these numbers with the results for school attendance, it seems that even though individuals are attending school more frequently school, they are not reducing their working hours. In the control group, an extra member in the family will constraint its budget; this constraint will increase if the new member is a person in the school-age of studying and cannot work. The results reveal, as expected, that both variables are positive and significant in most of the cases: an additional family member increases the probability of working, a result that affects men more than women. In spite of this, if the extra member is a child receiving the treatment, the impact is less negative.

Finally, Fig. (7) reports the behavior of the outcome variable (school attendance and work) at different ages and classifies it according to whether a person received the treatment. The figures are built using a non-parametric estimation using the predicted results of the Equation (1). The graphs are estimated at different treatment timings for male and female groups. The solid lines represent female and male treated groups. The control groups are represented by the dashed lines.

The figures summarize the main results. First, the graphs representing school attendance show that both male and female significantly improved their school attendance compared to the control group, an effect that is stronger for individuals who have received the treatment for a longer period. The effect is stronger for women in the group. Additionally, the impact seems to be more important as individuals get older: teenagers increased school attendance to a greater extent compared to children.

The figures reporting the estimated behavior of individuals with respect to labor decisions reveals interesting differences especially when a participant has been in the program for a longer time. In fact, the group that has received the treatment for just 1 year has not been affected by the program: children and teenagers are working as much as their peers in the control group. In contrast, for people who have received the treatment, it seems that the program has had some effect with a reduction in the labor supply of treated individuals. However, as the regression had indicated, the results are not significant.

When contrasting the results of school attendance and labor supply, one might

expect that children who attend school more frequently would work less. However, given our results, this is not the case in the sample. What seems to occur is that the program encourages children and teenagers to go to school more frequently. However, they continue working as much as much as children and teenagers in the control group. In any case, the nonparemetric regression show that, over time the results might in fact change and become significant.

5.2 Within-Family Externalities

One of the main concerns of antipoverty policy interventions is that granting money to families living in poor conditions might decrease the incentives of other members in the family (adults) to work. But, it is also possible that the effect of the program and the commitment of the parents to send their kids to school leaves the non-targeted members of the family with more time that can be spent on other productive activities. It is important to take note of the fact that even though the grants represent an important part of the family's income, the amount is still very small, it cannot, by itself, solve many budget problems. The intention of the grants is to encourage children to attend school and not discourage parents to work.

For this reason, this paper estimates the presence of within-family externalities on the labor decisions of the family members who are not directly targeted by the program but who live in a treated household. In other words, this paper estimates whether having a treated relative in the household has an impact on the labor decisions of older individuals. The estimation uses an extended version of Equation (1):

$$L_{iv} = \rho_0 + \rho_1 D_v + \rho_2 W_{ih} + \rho_3 (W_{ih} * D_v) + \sum_h \delta X_{ih} + \tau_t + \mu_{iv} \tag{3}$$

where L_{iv} stands for the labor decisions of individual i, living in the village v. Equation (3) assumes two potential transmission mechanisms through which the program can impact the untreated group: through a share of the extra resources from the treated to the untreated, effect captured by D_v; or through an impact of the treated children on their older relatives, effect captured by $(W_{ih} * D_v)$, where W_{ih} stands for the number of treated individuals living in the household h. "Familias en Acción" targets individuals between 0 and 17 years old. Therefore, the estimation of the within-family externalities includes information on all the individuals older than 17 years living in

a treated household. This group includes parents, older siblings, other relatives, and some friends.

It follows that the ATE for the untreated group is given by the impact of the cash grants plus the effect of the treated peers.[9]

$$E[L_{iv}|D_v = 1] - E[L_{iv}|D_v = 0] = \rho_1 + \rho_3 E[W_{ih}|D_v = 1] \tag{4}$$

Table (7) lists the results for the within-family externalities. In general, living in a treated household seems to have no impact on the labor decisions of the individuals. Even though the results are positive for most of the groups, suggesting an increase in the probability of working, if a person lives in a household receiving "Familias en Acción," the results are not significant.

It is interesting to note the effect of school-age individuals on the adults' labor decisions: the results show that an additional student at home increases the probability of adults working. Nonetheless, if we compare this variable with regard to treated and control households, it seems that the presence of more school-age individuals who are living in a treated household has a negative effect on labor decisions. It is clear that a family's participation in the program implies many conditions: parents have to take children to school and to medical checks and monitor children's nutrition. This situation might be negatively impacting on the labor situation of adults, especially parents.

Finally, a non-parametric estimation of the results illustrates the behavior of parents regarding labor decisions given the number of eligible family members, and it compares treated and control group households (Fig.8). The results indicate that the labor supply of the head of the household will significantly differ depending on whether he is a male or female. In fact, having more school-age children does not change the labor decisions of fathers. In contrast, if the head of the household is female, her labor decision will totally depend on the number of children in the family: having more children causes mothers to work more; however, if the children are part of the program, the probability of mothers working is significantly lower compared to their peers in the control group. This results becomes stronger for families that have participated in the program for a longer period of time.

[9] As assumed in the estimation of the direct impact, the matching procedure balanced the control and treated samples; therefore, $E[\mu_{iv}|D_v = 1] - E[\mu_{iv}|D_v = 0] = 0$.

5.3 Cross-Village Externalities

Because "Familias en Acción" was randomized at the village level and because some of the treated villages geographically located very close to some control villages, it is possible to estimate the presence of cross-village externalities. Cross-village externalities imply that the program has also had an impact on control villages that are not part of the program, but due to their proximity to a treated village, they might benefit from it through the presence of positive externalities, such as market transactions, or social interactions.

To estimate cross-village externalities, this paper has employed the information on the control villages (65 villages in total) and has divided them into two groups. The first group, or group G_1, contains all of the control villages that have as a neighbor a treated village; the second group, or group G_2, contains all of the control villages that are geographically isolated. It is clear that a control village having a treated village as a neighbor is not the only source of a potential impact of the program. In fact, in order for a control village to benefit from the program, its households should have contact with the households living in the treated village, or, at least, the connection between the two villages should be easy enough.

For these reasons, to select the villages part of the G_1 group, we have computed the time of transportation from the center of the control village to the center of its treated neighbor. In fact, for many control villages, in spite of being geographically located next to a treated village, a connection between them was not feasible, either because of distance (big villages), because of the lack of transportation, or simply because there is no infrastructure (in the form of highways or roads). In the end, the analysis included in the G_1 group all the control villages that had a treated neighbor, and the time of transportation between the two villages did not require more than 1 hour.[10] In total, the analysis includes 13 control villages geographically connected to a treated village and 52 control villages that were not close a treated neighbor. See Fig. (5).

The analysis has been performed on the school attendance of children. We have focused our analysis on this group only because the young individuals' outcomes show

[10]We limited the transportation time to 1 hour since we consider it as a standard feasible time for a daily commute. If we assume that children within control villages have an effect from children in treated villages, it is because they are constantly in contact. If the time to reach a treated village is higher, it is less likely that individuals have contact in a daily basis, which, at least for schooling decisions, we consider very important.

significant differences before the program's implementation. Table (4) details the main characteristics of children living in a control village connected to a treated village (group G_1) and children living in isolated control villages (group G_2). The table reports the difference between these two groups, and for each variable it reports the t-stat of this difference.

In terms of gender, family conditions, and family structure, health status, and schooling decisions, the sample is well-balanced. The only significant difference is the number of years of school of the parents. However, because the educational level of the adults in the sample is very low, where many parents reported never having attended school, in addition to controlling for the number of school years, we have also controlled for the illiteracy level of adults, which is statistically the same in both groups. In total, the sample has 3,544 children in the G_1 group and 12,240 children in the G_2 group.

Table 4: Cross-Villages Externalities: Descriptive Statistics

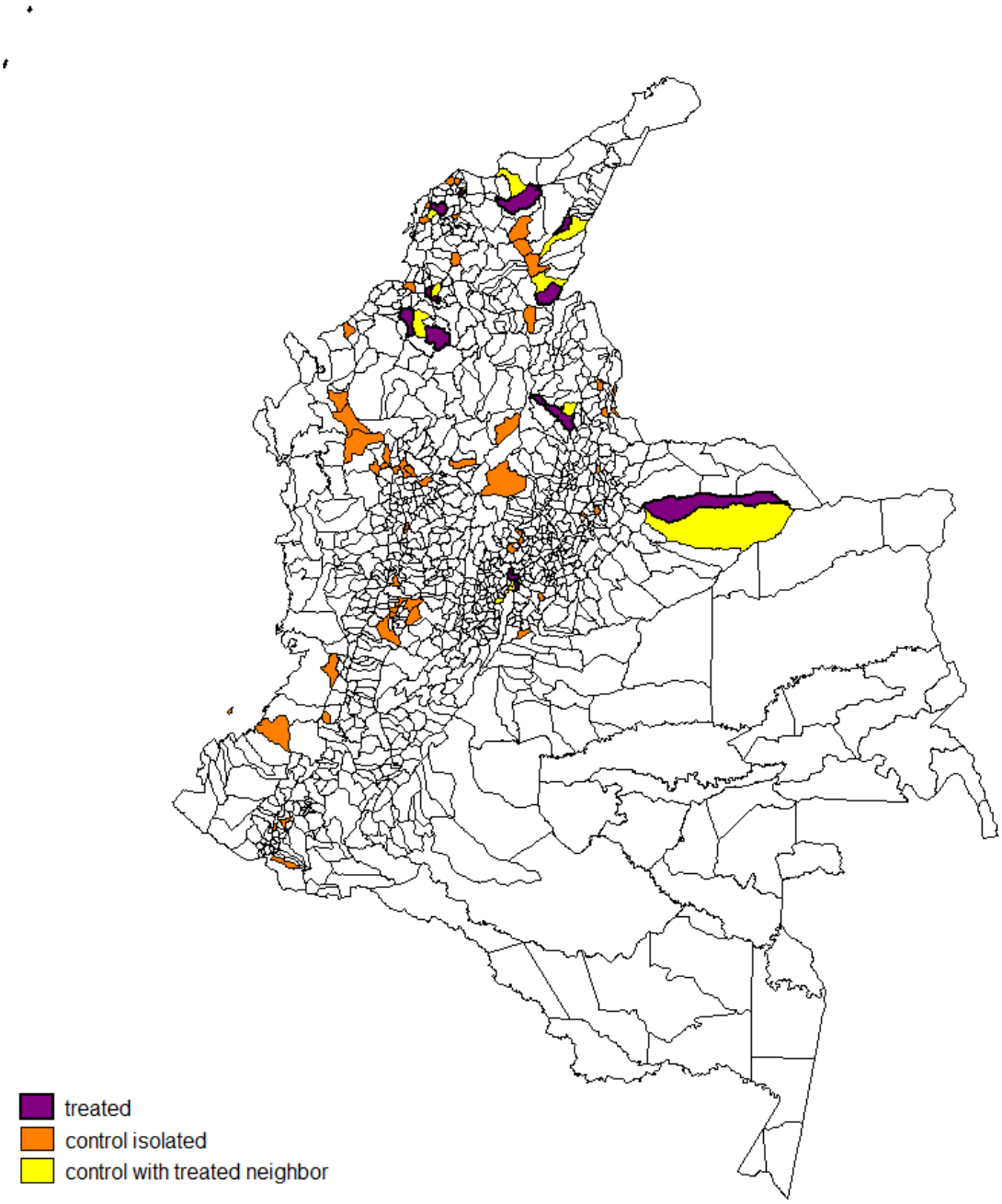
	Treated Neighbor (G_1)	Isolated Village (G_2)	Diff.
Boys	0.507	0.524	0.017
			(0.016)
Family's head work	0.838	0.841	0.002
			(0.011)
Father absent	0.141	0.145	0.004
			(0.011
Parent's schooling years	6.710	5.970	0.740***
			(0.150)
Family's member with disability	0.034	0.028	0.005
			(0.005)
Parents illiterate	0.292	0.311	0.019
			(0.014)
Number of members in the family	6.875	7.046	0.171
			(0.090)
Girls: school attendance	0.871	0.842	0.026
			(0.016)
Boys: school attendance	0.845	0.823	0.023
			(0.016)
N_0: before the program	1,337	4,514	
N_1: wave 1	1,227	4,175	
N_2: wave 2	980	3,551	

Significance levels: *: p<0.010 ; **: p<0.005 ; +: p<0.001. Standard errors in parenthesis.

[1.] T-test difference between the treated and control groups.

Notes: The table shows the background characteristics of children living in control villages. The G_1 group includes the children living in a village neighboring a treated village. The G_2 group includes the children living in villages that are isolated from a treated village.

Figure 5: Familias en Acción: Cross-Village Externalities



Source: Departamento Nacional de Planeamiento, Colombia.

Notes: The map shows the location of all the control villages in the program, and it highlights the treated villages that had a feasible connection to their neighbor. The control villages that have a treated neighbor become part of the G_1 group; the isolated control villages are part of the G_2 group.

The estimation of the cross-village externalities uses information on the children living in control villages, where the treatment D'_v is defined as whether a child living in a control village has a neighbor in a treated village.

$$S_{iv} = \beta_0 + \beta_1 D'_v + \beta_2 N_{iv} + \beta_3 (N_{iv} * D'_v) + \sum_h \delta X_{ih} + \tau_t + \epsilon_{iv} \tag{5}$$

The variable N_{iv} stands for the effect of treated peers on their untreated peers. This variable is utilizes two approaches: first, by including only the number of peers (children in the same school year), and second, by weighting the number of peers with the average distance from the control to the treated village. With this last option, it is possible to determine whether closer individuals have a stronger impact on their untreated peers.

Since the randomization revealed a balance between the G_1 and G_2 group, the ATE is given by:

$$E[S_{iv}|D'_v = 1] - E[S_{iv}|D'_v = 0] = \beta_1 + \beta_2 E[N_{jv}|D'_v = 1]$$

Table (8) shows the results of the estimation. Each column controls for different variables, and the results for the total sample demonstrates that having a treated village as a neighbor has a positive effect on the school attendance rate in the control village. Nevertheless, these results are significant only when using the weighted number of treated peers. Furthermore, by dividing the estimation in subgroups it is evident that for girls, living in a village closely located to a treated village has a positive and significant effect on their school attendance, while for boys, this result is negative.

The effect of having treated peers is significant for girls and boys, and it becomes stronger if the estimation uses the weighted definition. For girls, having an additional peer who is treated increases in 0.2% its school attendance. However, if we account for the village distance, this effect increases to 1.5%. For boys, a treated peer increases his school attendance by 0.2% on average, and by 1% if we account for the distance between villages.

Finally, Equation (5) could be informative in endogenous social interactions. It could be the case that children in control villages improve their school attendance because of the improvement in the school attendance of children living in treated

villages. Using the next equation:

$$S_{iv} = \beta_0 + \beta_1 \bar{S}_v + \sum_h \delta X_{ih} + \tau_t + e_{iv} \tag{6}$$

where S_{iv} stands for the school attendance of children i, living in village v and $\bar{S}_v$ stands for the average school attendance of the peer treated group, we intent to estimate the effect that peers' decisions have on the decisions of individual i. Nonetheless, the equation has a clear problem of endogeneity because the peer group might also be affected by the school attendance of children i, i.e., $E[\bar{S}_v|e_{iv}] \neq 0$. Nevertheless, because we are working at the village level and are focussing our analysis on the control villages, it is possible to use as an instrument the treatment condition D_v. In this way, D_v has a direct effect on the school attendance of children living in the treated villages, and it impacts the school attendance of children living in control villages through the interaction of children between villages.

Table (9) shows the results of Equation (6). We estimate two regressions, one using a standard linear model and the second regression taking into account the fact that the outcome variable (school attendance) is binary but the endogenous regressor is continuos. Both equations control for gender, parents' education, working status of the head of the household, whether the father is absent, whether there is a family's member with some kind of disability, the size of the household, year effects, age controls, and village controls.

The results suggest a strong presence of social interactions: children living in control villages increase their school attendance if their peers living in a treated village increase their attendance. In the lower section of the table, each regression presents some test to verify the quality of the instruments: the results suggest that the instruments are strong.

Fig.(9) summarizes the last results. The graph is a non parametric regression using the estimated results. On the vertical axis, the figure describes the predicted probability of children attending school; the horizontal axis represents the number of peers individual i has. The graph differentiates between the villages that are near a treated village (red line) and the villages that have no neighbor participating in "Familias en Accion" (black line).

We assume that a child living in a control village that has as a neighbor a treated

village will interact with other children of the same age and school year (peers) from both his home village and the neighbor village. On the contrary, children living in isolated villages will only interact will other children of the same age and school year who are not part of the program. If the peer group has, in fact, some influence over the child i, it is very likely that treated children, committed to attending school more regularly, will influence control children to go more frequently, too. When comparing the behavior of children between the two groups of villages, it is evident that children who have treated peers increase their school attendance compared to their peers in isolated villages.

6 Conclusions

This paper has analyzed the different effects, direct and indirect, of an antipoverty program implemented in Colombia as a CCT. The main goal of the program has been to increase the matriculation and attendance rate among children and teenagers by granting families monetary incentives that require specific behavior.

We have information on treated households living in treated villages and untreated households living in control villages. Therefore, to estimate the extent of the program's impact, we have utilized the available data in three ways. First, we have exploited the information within villages to determine the direct effect of the program on the targeted individuals (individuals younger than 17 years old). Second, we have employed the information within families to determine the potential externalities the program creates on the individuals not directly targeted by the program but who cohabit with treated relatives. And finally, we have utilized cross-village information to identify the externalities that individuals living in treated villages have created for individuals living in nearby control villages; for this method we have only used information on the control villages and have divided them into control villages that have close a treated villages, and the control villages that are geographically isolated.

Because the randomization of the program was performed at the village level, when we utilized the within-village and family information, we did not have a random sample. Therefore, we calibrated the samples by means of a matching procedure to render the individuals and households living in control and treated villages comparable. For the cross-village analysis, we use the data as available.

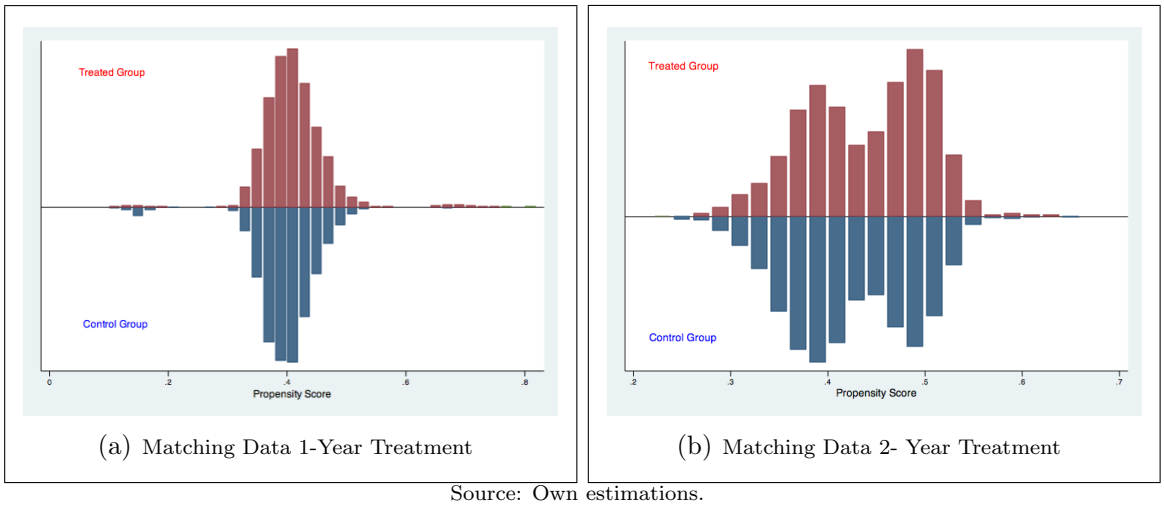
The results suggest that the program, "Familias en Acción," was successful at improving the attendance rate of the targeted group. In general, school-age children (7 to 17 years old) increased, on average, their attendance rate by close to 4% as a result of the program. This result becomes more pronounced (4.4%) if the individuals have been receiving the cash grants for longer periods. When determining the effect on the labor decisions of children and teenagers, it seems that, in spite of an increase in school attendance, individuals continue working as much as they did before: the treatment has had no effect on the probability of an individual to work.

In terms of within-family externalities, we have analyzed the effect of having a treated relative in the same household. We have determined the effect of the program on the labor decision of adults. One of the concerns of any policy intervention intended to reduce poverty is to create the correct incentives. In fact, it could be the case that the money transfers reduce the individuals' incentives to search for a job or even stay working. In spite of this, the results reveal that the program has no impact on the labor decisions of the adults in the participating households. Nevertheless, more treated individuals in the household negatively impacts the labor supply of mothers.

Finally, the cross-village estimation gave us a nice framework for analyzing the extent of the externalities of the program. In fact, since we were able to divide the control villages into villages that had a treated neighbor and villages that were isolated, it was possible to estimate the presence of externalities and the presence of social interactions. The results show a positive and significant effect of the peer group on the schooling decisions of individual i. This is the case not only because there might be a transference between villages of resources in the form of the cash grants, but also because of the interactions and learning process between the treated children and their friends. In other words, if the program succeed at increasing the attendance rate of children, it is very likely that their friends, in spite of not being part of the treatment, will increase their school attendance, too.

7 Appendix

Figure 6: Propensity Score Matching Groups



(a) Matching Data 1-Year Treatment

(b) Matching Data 2- Year Treatment

Source: Own estimations.

	0.442^{+}	0.506^{+}	0.486**	0.658^{+}	0.679^{+}	0.771^{+}	0.327*	0.483**	0.370**	0.390**
	(0.157)	(0.166)	(0.235)	(0.232)	(0.196)	(0.191)	(0.192)	(0.207)	(0.179)	(0.192)
family	-0.081**	-0.031	-0.110	-0.028	-0.124*	-0.063	-0.038	-0.035	-0.064	-0.011
	(0.034)	(0.039)	(0.103)	(0.095)	(0.073)	(0.075)	(0.056)	(0.061)	(0.050)	(0.062)
	-0.404^{+}	-0.468^{+}								
	(0.062)	(0.056)								
ng years	0.096^{+}	0.088^{+}	0.126^{+}	0.080^{+}	0.082^{+}	0.075^{+}	0.110^{+}	0.106^{+}	0.080^{+}	0.076^{+}
	(0.011)	(0.011)	(0.032)	(0.028)	(0.023)	(0.022)	(0.016)	(0.015)	(0.015)	(0.014)
	-0.144	0.012			-0.963	-0.748	0.230	0.415	-0.033	0.251
	(0.234)	(0.242)			(0.956)	(0.932)	(0.399)	(0.381)	(0.352)	(0.398)
	0.029	0.048	0.098	0.150	0.045	0.083	-0.065	-0.005	0.082	0.062
	(0.099)	(0.101)	(0.216)	(0.214)	(0.179)	(0.182)	(0.109)	(0.100)	(0.117)	(0.129)
he family	-0.272*	-0.300**	-0.556	-0.419	-0.032	-0.280	-0.188	-0.490**	-0.491**	-0.202
	(0.145)	(0.148)	(0.405)	(0.475)	(0.337)	(0.340)	(0.259)	(0.209)	(0.210)	(0.228)
s	-0.037	-0.064**	0.015	-0.044	0.007	-0.031	-0.090**	-0.076*	-0.025	-0.061
	(0.025)	(0.028)	(0.067)	(0.058)	(0.059)	(0.059)	(0.041)	(0.044)	(0.041)	(0.048)
literate	-0.033	-0.213^{+}	-0.090	-0.220	-0.098	-0.315**	0.077	-0.089	-0.017	-0.235**
	(0.083)	(0.081)	(0.214)	(0.219)	(0.159)	(0.156)	(0.152)	(0.143)	(0.126)	(0.117)
	-0.845**	-0.633*	0.012	0.630	0.797	1.001	-0.203	-0.064	-0.032	-0.213
	(0.350)	(0.346)	(0.588)	(0.614)	(1.034)	(1.021)	(0.557)	(0.502)	(0.503)	(0.520)
	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
vity	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	2457.559	1865.300	315.819	152.296	285.238	282.608	302.617	239.732	309.916	391.033
	13381.000	13864.000	3872.000	3937.000	4224.000	4293.000	2436.000	2613.000	2673.000	2879.000
s:										
	0.037^{+}	0.041^{+}	0.016**	0.018^{+}	0.023+	0.028^{+}	0.082**	0.093**	0.079**	0.102**
	(0.013)	(0.013)	(0.007)	(0.006)	(0.009)	(0.008)	(0.038)	(0.041)	(0.040)	(0.044)

= 1 if a person is attending school; 0, otherwise.

els: *: p<0.010 ; **: p<0.005 ; $^{+}$: p<0.001. Standard errors in parenthesis.

treatment	0.082	-0.093	0.039	0.044	0.007	-0.086
	(0.188)	(0.173)	(0.249)	(0.250)	(0.166)	(0.174)
eligible-peers in family	0.037	0.073	-0.012	0.042	0.016	0.040
	(0.055)	(0.063)	(0.088)	(0.087)	(0.055)	(0.068)
male	0.717^{+}	0.726^{+}				
	(0.102)	(0.112)				
parents' schooling years	-0.047^{+}	-0.067^{+}	-0.044	-0.117^{+}	-0.048^{+}	-0.055^{+}
	(0.013)	(0.014)	(0.028)	(0.023)	(0.016)	(0.017)
father works	0.858**	0.676	0.731	1.025	0.872*	0.498
	(0.430)	(0.555)	(0.777)	(1.232)	(0.449)	(0.620)
mother works	0.403**	0.086	0.512*	0.012	0.324**	0.070
	(0.164)	(0.168)	(0.266)	(0.335)	(0.151)	(0.143)
discapacity in the family	-0.035	0.341*	-0.377	0.264	0.110	0.367
	(0.210)	(0.199)	(0.350)	(0.322)	(0.282)	(0.278)
number of family members	0.052	-0.004	0.053	0.062	0.102**	0.007
	(0.044)	(0.047)	(0.066)	(0.060)	(0.045)	(0.052)
family's head illiterate	-0.161	-0.122	-0.394*	-0.685^{+}	-0.009	0.124
	(0.111)	(0.130)	(0.203)	(0.235)	(0.129)	(0.149)
constant	-2.209^{+}	-1.285*	-2.590^{+}	-1.664	-2.277^{+}	-1.291*
	(0.633)	(0.661)	(0.995)	(1.437)	(0.528)	(0.690)
Age controls	Yes	Yes	Yes	Yes	Yes	Yes
Type of activity	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	No	No	Yes	Yes	Yes	Yes
Schooling level	Yes	Yes	Yes	Yes	Yes	Yes
chi2	596.073	416.073	93.617	68.711	171.427	176.244
N	2497.000	2345.000	785.000	694.000	1706.000	1646.000
Marginal Effects:						
treatment (γ)	0.019	-0.021	0.008	0.008	0.002	-0.020
	(0.043)	(0.039)	(0.049)	(0.047)	(0.040)	(0.042)

Dependent Var.= 1 if a person reported to had a productive payed activity; 0, otherwise.
Significance levels: *: p<0.010 ; **: p<0.005 ; $^{+}$: p<0.001. Standard errors in parenthesis.

nt	0.066	0.043	-0.008	0.025	0.463	0.401	0.309	0.033
	(0.113)	(0.113)	(0.175)	(0.149)	(0.324)	(0.499)	(0.260)	(0.217)
members in family	0.071^{+}	0.074^{+}	0.128^{+}	0.133^{+}	0.221^{+}	0.259^{+}	0.040	0.020
	(0.024)	(0.025)	(0.030)	(0.030)	(0.078)	(0.082)	(0.055)	(0.055)
members*treatment	-0.040	-0.061^{*}	-0.042	-0.094^{**}	-0.294^{+}	-0.194	-0.037	0.002
	(0.043)	(0.033)	(0.059)	(0.040)	(0.110)	(0.172)	(0.075)	(0.074)
	-0.349^{+}	-0.359^{+}	-0.394^{+}	-0.520^{+}	-0.406	0.239	-0.047	0.055
	(0.056)	(0.060)	(0.084)	(0.084)	(0.614)	(0.649)	(0.151)	(0.156)
	3.386^{+}	3.468^{+}					1.163^{+}	1.160^{+}
	(0.095)	(0.092)					(0.138)	(0.134)
education							-0.073	-0.036
							(0.050)	(0.046)
ducation							0.020	0.007
							(0.043)	(0.041)
	-0.084	-0.416^{*}	0.618^{**}	0.782^{**}	2.522^{+}	1.611^{**}	-0.381	-0.869^{*}
	(0.263)	(0.237)	(0.310)	(0.336)	(0.729)	(0.637)	(0.459)	(0.483)
n level	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
activity	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
cts	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	1870.149	2145.089	138.292	124.528	114.993	208.058	149.445	150.279
	17955.000	18842.000	6585.000	6935.000	8158.000	8604.000	2202.000	2291.000
l Effects								
nt ρ_1	0.012	0.008	-0.002	0.005	0.003	0.003	0.061	0.007
	(0.020)	(0.020)	(0.036)	(0.030)	(0.002)	(0.004)	(0.050)	(0.045)
members*treatment ρ_3	-0.007	-0.011^{*}	-0.009	-0.019^{**}	-0.002^{***}	-0.002	-0.007	0.001
	(0.008)	(0.006)	(0.012)	(0.008)	(0.001)	(0.001)	(0.015)	(0.015)

ent Var.= 1 if a person reported to had a productive payed activity; 0, otherwise.

nce levels: *: p<0.010 ; **: p<0.005 ; $^{+}$: p<0.001. Standard errors in parenthesis.

: 1st. group to receive the treatment.

: 2nd. group to receive the treatment.

		(0.003)	(0.002)	(0.003)	(0.003)	(0.005)	(0.003)	(0.004)
neighbor village[1]		0.018^{+}	0.015^{+}		0.021**	0.013**		
		(0.005)	(0.003)		(0.009)	(0.006)		
neighbor village/distance[2]				0.205^{+}			0.253^{+}	0.173**
				(0.044)			(0.074)	(0.086)
			−0.161^{+}	−0.156^{+}				
			(0.036)	(0.038)				
orks			−0.066	−0.051	−0.021	−0.096	−0.020	−0.095
			(0.047)	(0.051)	(0.065)	(0.065)	(0.065)	(0.065)
works			0.103**	0.109**	0.146**	0.087	0.148**	0.088
			(0.048)	(0.047)	(0.071)	(0.068)	(0.072)	(0.068)
schooling years			0.023^{+}	0.021^{+}	0.023^{+}	0.018**	0.023^{+}	0.018**
			(0.005)	(0.005)	(0.007)	(0.007)	(0.007)	(0.007)
bsent			−0.200^{+}	−0.167^{+}	−0.291^{+}	−0.053	−0.290^{+}	−0.053
			(0.062)	(0.057)	(0.070)	(0.082)	(0.069)	(0.083)
ity in the family			−0.232^{+}	−0.242^{+}	−0.286**	−0.200*	−0.286**	−0.200**
			(0.080)	(0.083)	(0.113)	(0.103)	(0.113)	(0.102)
head illiterate			−0.148**	−0.162**	−0.113	−0.247^{+}	−0.119	−0.245^{+}
			(0.066)	(0.067)	(0.090)	(0.088)	(0.089)	(0.088)
of family's members			−0.042^{+}	−0.043^{+}	−0.041^{+}	−0.047^{+}	−0.040^{+}	−0.046^{+}
			(0.009)	(0.010)	(0.013)	(0.012)	(0.013)	(0.012)
	1.016^{+}	0.704^{+}	2.280^{+}	1.977^{+}	1.589^{+}	2.446^{+}	1.566^{+}	2.442^{+}
	(0.031)	(0.055)	(0.150)	(0.169)	(0.209)	(0.288)	(0.209)	(0.293)
ntrols	*No*	*No*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
evel controls	*No*	*No*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
trols	*No*	*No*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
controls	*No*	*No*	*No*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
lihood	−6529.148	−6305.114	−3452.655	−3336.558	−1461.068	−1822.531	−1462.018	−1822.413
	15624.000	15624.000	14190.000	14190.000	6871.000	7201.000	6871.000	7201.000
l Effects								
nt	0.032**	−0.035	0.013^{+}	0.029^{+}	0.038+	0.051^{+}	−0.030**	−0.008
	(0.013)	(0.036)	(0.005)	(0.003)	(0.006)	(0.003)	(0.015)	(0.005)
neighbor village[1]		0.004^{+}	0.001^{+}		0.002^{+}		0.001**	
		(0.001)	(0.000)		(0.001)		(0.001)	
neighbor village/distance[2]				0.018^{+}		0.017^{+}		0.018**
				(0.004)		(0.004)		(0.008)

dent Var.= 1 if a child attends school; 0, otherwise.

cance levels: *: p<0.010 ; **: p<0.005 ; $^{+}$: p<0.001. Standard errors in parenthesis.

mber of peers living in the neighbor treated village.

mber of peers living in the neighbor treated village weighted by the distance between the two villages.

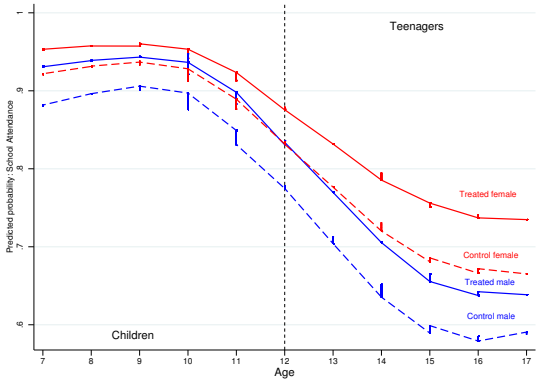
Table 9: Cross Village Externalities: Instrumental Variables Estimates

	IVReg	IVProbit
average school attendance peer group	0.455^{+}	1.745^{**}
	(0.163)	(0.688)
male	-0.019^{+}	-0.093^{+}
	(0.005)	(0.028)
peers per schooling year	0.006+	0.030+
	(0.000)	(0.002)
father works	−0.000	−0.032
	(0.007)	(0.036)
mother works	0.022^{+}	0.114^{+}
	(0.007)	(0.036)
parents' schooling years	0.001	0.002
	(0.001)	(0.003)
father absent	-0.066^{+}	-0.302^{+}
	(0.008)	(0.039)
disability in the family	-0.034^{+}	-0.172^{+}
	(0.011)	(0.046)
family's head illiterate	-0.066^{+}	-0.329^{+}
	(0.009)	(0.038)
family's members	-0.024^{+}	-0.098^{+}
	(0.001)	(0.005)
constant	0.553^{+}	0.050
	(0.141)	(0.538)
athrho		
constant		0.109
		(0.084)
lnsigma		
constant		-2.091^{+}
		(0.010)
Age controls	*Yes*	*Yes*
Village controls	*Yes*	*Yes*
F	19.909	
chi2		1210.103
ll	−4411.327	5030.615
N	15097.000	15097.000
Kleibergen-Paap rk Wald F stat	199.247	
Stock-Yogo critical values: 10% maximal IV size	16.380	
Wald test of exogeneity		1.690

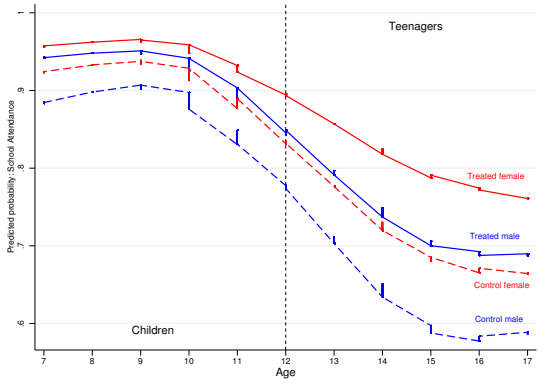
Dependent Var.= 1 if a child attends school; 0, otherwise.

Significance levels: *: p<0.010 ; **: p<0.005 ; $^{+}$: p<0.001. Standard errors in parenthesis.

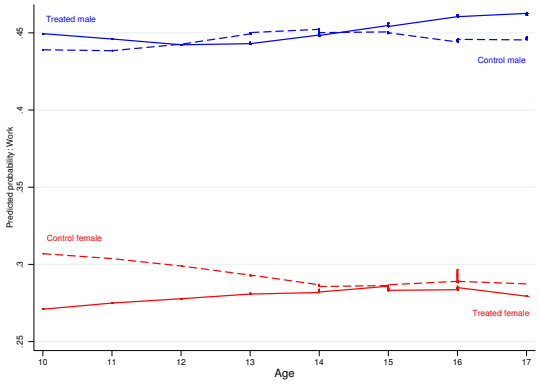
Figure 7: Direct Effect: Individuals 7-17 years old



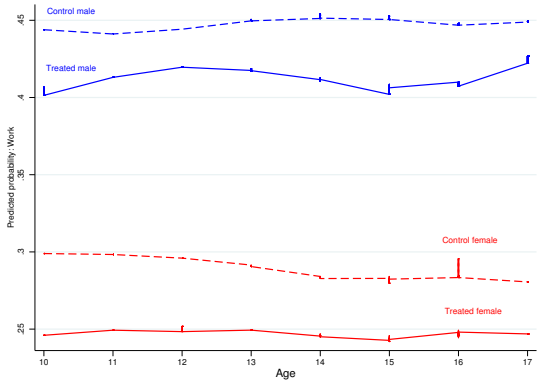
(a) School attendance: 1-year Treatment



(b) School attendance: 2-year Treatment

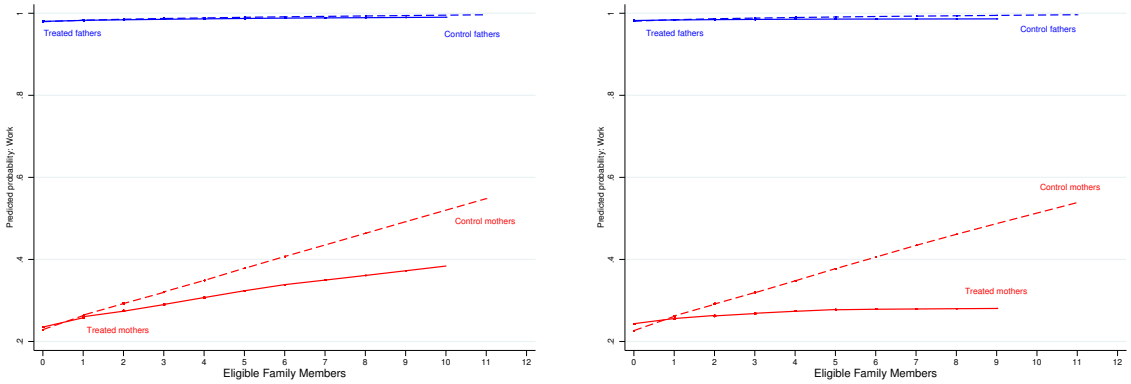


(c) Work: 1-year Treatment



(d) Work: 2-year Treatment

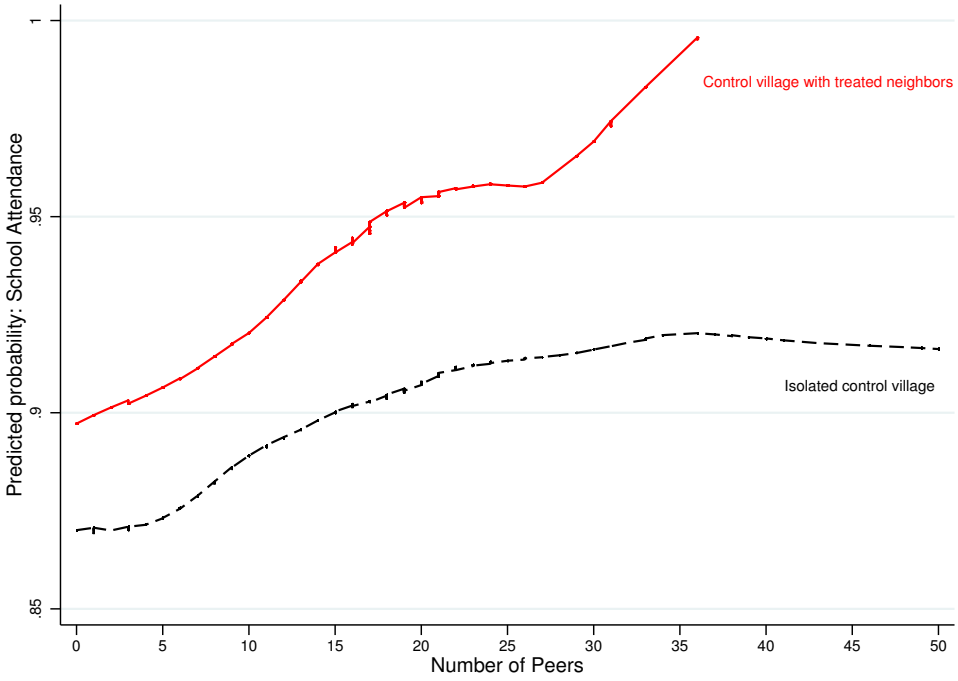
Figure 8: Within Family Externalities: Labor Supply



(a) 1-year Treatment

(b) 2-year Treatment

Figure 9: Cross Village Externalities: School Attendance



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