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Antipoverty Programs
Impact Evaluation, Externalities and Limitations

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Diana PACHECO BARZALLO

Accepted by the dissertation committee:

Prof Jean-Marie Grether, University of Neuchâtel, thesis director

Prof Mehdi Farsi, University of Neuchâtel

Prof Olivier Cadot, University of Lausanne

Marco Fugazza Ph.D, United Nations Conference on Trade and Development

Prof María Laura Alzua, National University of La Plata, Argentina

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Faculté des Sciences Economiques
Avenue du 1er-Mars 26
CH-2000 Neuchâtel
www.unine.ch/seco

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La Faculté des sciences économiques,
sur le rapport des membres du jury

Prof. Jean-Marie Grether (directeur de thèse, Université de Neuchâtel)
Prof. Mehdi Farsi (président du jury, Université de Neuchâtel)
Prof. Olivier Cadot (Université de Lausanne)
Prof. Maria Laura Alzua (National University of La Plata)
Dr. Marco Fugazza (United Nations Conference on Trade and Development)

Autorise l'impression de la présente thèse.

Neuchâtel, le 17 décembre 2013

Le doyen

J-M Grether

Jean-Marie Grether

En tu memoria, Emilia

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Abstract

This doctoral thesis utilizes different microeconometric methods to estimate the direct impact, the generated externalities, and the limitations of two antipoverty programs implemented in Latin America in the form of Conditional Cash Transfers (CCT).

I focus on CCT programs for two reasons. First, due to the important positive effects shown, many governments have rapidly adopted this tool as their main initiative to fight poverty. And second, CCTs are one of the very few initiatives that were actually generated in the developing world and later imported in developed economies.

The thesis is comprised of three separate, but closely related chapters. I perform empirical analyses using data from two programs in Mexico and Colombia, employing program evaluation techniques, as well as parametric regression modelling.

The main contribution of this thesis is to show that social interventions, besides having important effects on the targeted population, have also a great impact on people not part of the program. In fact, individuals respond to incentives, pushing a change in their decisions, either through monetary transfers, or by a change in the observed behavior of their peers. Thus, CCT programs can be expected to have an effect beyond the targeted group.

The externalities shown by CCT are mainly positive, specially when analyzing health, nutrition and educational outcomes. The results are less clear when analyzing labor outcomes. Nevertheless, as any policy, these programs are perfectible. It is important to identify these limitations, as well as the changes to the designs for future work.

Keywords: causal analysis, impact evaluation, propensity score matching, randomized experiment, spillovers.

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Part I

Introduction

Introduction

Motivation

Despite important improvements over the last decades, people living in poverty still make up a great part of the world's population. It has been estimated that, by the year 2010, 2.4 billion people were living on less than USD 2 a day. In addition, in many developing countries there is a widening gap between rich and poor, and between those who can and cannot access opportunities. This means that access to good quality schools, health care, and basic services still remains elusive for many people, The World Bank [2013].

At the risk of oversimplifying, the many alternatives for tackling the problem that have been debated can be roughly organized around two proposals. The first one, held by Sachs [2005] claims that extreme poverty can be eliminated through planned development aid. He suggests that poor countries cannot develop because they do not even have enough resources to reach the "bottom rug" of the ladder of economic development. Therefore, once the bottom is reached, poor countries can pull themselves out of poverty.

In contrast, Easterly [2006] criticizes the ineffectiveness of Western organizations to mitigate global poverty. He suggests that aid is more a problem than a solution because it creates rents over which people fight. And more than that, aid prevents countries from achieving their own path to development.

Many contributions have been put forward in favor of each proposition; however, up to now, the solution has not reached a consensus [Sen, 2006]. There is no way to know what would have happened to poor countries if no aid had been provided; maybe the situation would have been worse. In fact, aid has been present, although its magnitude has not been large enough. If we look closer at the statistics, the official development assistance (ODA) reported by the OECD [2013] shows that aid towards development has not reached more than 1% of donors' national income, .

These two propositions see the problem at the macro level, which precludes the discussion over specific solutions. Perhaps the answer is not to fight the problem at the macro level, but to start looking at the micro level to identify the problems and define the policies to implement. It is clear that there will not be a unique solution, but, instead, it is possible to start by learning what works and what does not.

Following this argument, many governments and non profit organizations have tried to intervene directly in poor populations with different social programs intending to fight specific problems. Most of them are based on incentives, and the goal is to increase the human capital of those living in poverty through the improvement in health, education, and nutrition. As a result, many of these programs have been successful, and many others have failed in their intentions, either because of the program's design, or because of a misidentification of the way incentives work.

This thesis studies one of the most successful initiatives implemented in the developing world: Conditional Cash Transfers (CCT). This type of programs appeared in the 90's in developing countries as a new system of social protection intended to replace the costly and inefficient system of subsidies. In fact, the history of social assistance in Latin America has been characterized by the implementation of subsidies in different areas. Mexico, for example, implemented more than 15 food subsidies, most of them targeting poor families in rural areas, and other few related to specific targeted group. In spite of bad experiences, the country also had subsidies for electricity and running water. In the same way, Chile implemented water price and housing subsidies, besides subsidies for other uses. Ecuador, Colombia and Brazil, countries facing significant inequalities, also applied several type of subsidies.

Studies made on the effectiveness of subsidies show that most of these initiatives were badly targeted and had limited impact on poverty. In fact, most of the subsidies targeting poor families in rural areas were absorbed by the non poor. In addition, since the targeting mechanism, in most of the cases, was not controlled, analyzing the potential effects and global impacts was not possible due to an inexistent system of evaluation. In general, authors agree that subsidies created an imbalanced government spending favoring non poor individuals in urban areas [Levy & Rodriguez, 2004], [Levy, 2006].

It is in this context that CCT programs appear as an innovative tool for direct redistribution. The programs are based on the idea that people respond to incentives, so they grant money to poor families conditional upon specific behavior. Program's beneficiaries are committed to send their children more frequently to school, maintain regular health checkups, and monitor the nutrition of pregnant women and newborn children. The goal is to increase the human capital of children in order to break the intergenerational transmission of poverty [Rawlings & Rubio, 2005]. CCTs run nationwide, or focus on regional areas, and in small-scale pilot efforts.

Due to the growing literature showing significantly positive results, CCTs have been rapidly expanded around the world[1]. In addition, CCTs are particularly interesting because they are one of the few initiatives originally designed in developing countries and expanded to developed economies[2].

The great success of these programs has made some analysts refer to them as "magic bullets," The Economist [2010]. However, as any policy intervention, there are still many things to take into account. First, most of the evaluation has focused on the direct impact of CCTs; however, there is a growing evidence on the presence of externalities (positive and negative) [Miguel & Kremer, 2004], [Angelucci & Giorgi, 2009], [Ribas *et al.*, 2010], and [Durlauf & Young, 2001]. In fact, since the programs grant money to poor households on condition of specific behavior, there might be a spillover effect through market transactions, or through the presence of social interactions among individuals. And second, the programs' design fails to account for some limitations, such as problems in the quality of education and health services, market failures, or even in the direction of the incentives.

Overview

This doctoral dissertation is an empirical work that utilizes different microeconometric methods to estimate the global impact, externalities, and limitations of antipoverty programs in the form of Conditional Cash Transfers (CCT). It is comprised of three separate but closely related chapters. I perform empirical analyses using data from two nationwide antipoverty programs implemented in Mexico and in Colombia, employing program evaluation techniques, as well as parametric and nonparametric regression modeling.

It is important to mention that this work takes the definition of "externalities" in a

[1]By 1997, Mexico and Brazil implemented CCT programs as their main policy to fight poverty. In 2008, there were more than 30 countries running some type of CCT [Fiszbein *et al.*, 2009]. Only in Latin America the programs reach more than 113 million of people, which account for almost 20% of the region population

[2]Opportunity NYC is the first Conditional Cash Transfer (CCT) initiative to be implemented in the United States, or any other developed nation. It was announced in April 2007, under the administration of Mayor Michael Bloomberg, see NYC Center for Economic Opportunity [2013]

general sense, and not in its strict definition. Public Economics claims that externalities arises whenever the actions of one economic agent make another economic agent worse or better off, yet the first agent neither bears the costs nor receives the benefits of doing so. In the case of this thesis, externalities can be understood as spillovers that arise from interventions targeting a specific group in the population, but have also an effect on the untreated group through social and economic interactions with the treated.

The body of the thesis comprehends three related studies. The first paper estimates direct and spillover effect of Programa de Educación, Salud y Alimentación (PROGRESA) on the health and nutrition of individuals. The program, a CCT initiative launched in 1998, constitutes the main antipoverty initiative of the Mexican government. Nowadays, it covers more than 25 million people, and its evaluations have shown great improvements on the conditions of people living under poverty.

The importance of PROGRESA relies on its design: a randomized experiment that includes a pre-program evaluation, and several follow up surveys with information of the people part of the program and its neighbors. To estimate the impact of the program, the paper uses several health and related outcomes for different age groups in the sample. To estimate the spillover effect, I use the GPS coordinates of each household in order to identify the number of individuals with whom a treated person might have contact.

The results suggest that the direct effect of PROGRESA significantly improved the health status of treated babies, children, and adult individuals. In terms of the spillover effect, people not directly targeted by the program, but who live in villages where PROGRESA is working, reveal an improvement of their health conditions. In fact, untreated children significantly increased their health checkups and nutrition monitoring, young individuals show shorter sickness spells, and adult individuals have less problems dealing with daily activities.

In the second paper, I use data from an antipoverty program named "Familias en Acción" to estimate the direct effect and generated externalities on the schooling decisions and labor participation of individuals. "Familias en Acción" is a CCT program implemented in Colombia in 2001. The program was intended to have the same structure and design as PROGRESA; however, due to some administrative and political issues, some of the villages, part of the treated group, started receiving the cash grants before than planned. This created some imbalances in the sample requiring the implementation of matching techniques before the estimation of the results. The paper took advantage of this structure of the data set and divided the analysis into two groups: villages that receive the program for one year, and villages that receive the program for two years.

This division helped to compare the results in the short and medium term.

The paper is divided into three blocks, one intended to estimate the effect of the program on the targeted population, a second block analyzing the presence of within family externalities on the labor participation of young adults, and adults, and a third block exploiting the geographic distribution of the village in the country to estimate the presence of externalities on the school participation of children. The results suggest that the program was quite successful at increasing the school attendance of children and teenagers. Nonetheless, it had no effect on their labor decisions. In terms of externalities, untreated peers living nearby treated children show an important positive effect on their school attendance: if a child regularly attends school, his closest peers are more likely to show up to school. Finally, within family externalities show a negative effect on the labor decision of the family's head, specially is she is a woman. In fact, having an extra child who is committed to follow the conditions of the program, decreases the probability of the mother to work. This last result rises the question about how positive it is for children to have their mother taking care of them all the time, but, at the same time, how negative it is for a women to be bound for housework and babysitting.

Finally, the third paper again refers to the data set of PROGRESA, but this time the analysis goes further and estimates when individuals decide to drop out of school to start working. The analysis contrasts a duration model estimating the time beneficiaries of the program decide to stay in school before dropping out of school to start working, with a returns to schooling model. The paper focuses only on the targeted group of the program to evaluate how effective the cash grants have been in the medium term. It is not possible to make an analysis in the long term since the included information covers only three evaluation surveys, and, specially for schooling outcomes, a long run analysis requires a longer period of analysis.

The main results show that individuals continue dropping out of school as much as before the program's implementations. I explain this behavior by contrasting this result with the returns to schooling in the sample. In fact, it seems that the reason for this dropout behavior is that, in poor populations, most individuals work in low productive activities that, besides experience, require being able to write, read, and to do simple math operations, things that a person learn in primary school. Higher levels of education do not show a significant increase in the wage rates, meaning that an extra year of school has a higher cost than benefit. It is important to highlight that this result corresponds to individuals living in rural areas. In urban areas, however, related literature show that CCT programs have an important significant effect on the school attainment of individuals.

The last chapter summarizes the work made in the thesis, and its main conclusions. It highlights the importance of the study, and suggests some ideas for future work.

Part II

Research Papers

Paper 1

Health Conditions & Social Interactions*

This chapter assesses the direct and spillover effects of an antipoverty program on the health conditions of individuals. The program, a conditional cash transfer intervention, grants money to eligible families conditioned on specific behavior. Its purposes are to improve the living conditions of the households and to promote their investment in their children through school attendance and in providing basic health services. Because the design of the program includes information on eligible and ineligible families, evaluating its direct impact through the effect of the cash grants on eligible individuals is possible. The indirect or spillover effect is estimated by the effect generated from the treated individuals on their non-treated peers. In the case of the direct impact, the results show that eligible individuals significantly improved their health status. Specifically, the incidences of sickness decreased, sickness spells were reduced, and people seemed able to manage normal activities with less difficulty. In the case of the spillover effect, the estimates show that having more treated households in the neighborhood improves the health conditions of non-eligible individuals by lowering their probabilities of being sick and by decreasing the length of a sickness. Adults as well as old individuals have less difficulty dealing with daily activities.

*Paper presented in The European Conference, organized by The Clute Institute, Barcelona, Spain 2011. It was also presented in The Third Australian Workshop on Econometrics and Health Economics, Sydney, Australia 2012.

1.1 Introduction

Despite improvements over the last decades, health problems and deficiencies in child nutrition persist in developing countries[1]. For this reason, many governments have implemented social programs that attempt to intervene in poor populations through cash grants[2]. These programs can be classified into two groups. In the first group are conditional programs that make cash transfers contingent on some behavior, usually investments in the human capital of children, such as sending them to school or bringing them to health centers. In the second group are unconditional programs that focus on a specific age group and in which the transfers have no conditions.

This chapter analyzes the impact of *Programa de Educación, Salud y Alimentación (PROGRESA)*, a CCT program, on health and nutrition. The hypothesis of this document is that CCT programs impact not only the targeted individuals through their money grants, but, by imposing a specific behavior, the Program also impacts non-eligible individuals who interact with the treated individuals. The change to a positive behavior by some families will impact their neighbors by changing their behavior with respect to health and nutrition in a positive way: families are learning from each other.

Serving approximately 25 million people, 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 related literature has studied the direct impact of CCT programs on school attendance and on health and nutrition. In general, these studies have suggested that antipoverty programs that combine education, health, and nutrition interventions can be quite successful at improving the capacity of families to pull themselves out of poverty: Behrman & Hoddinott [2005], Bouillon & Tejerina [2006], Cohen & Franco [2006], and Rawlings & Rubio [2005]. Health and nutrition are very important factors, not only because they directly increase an individual's welfare but also because they improve a

[1]According to the UNICEF, in 2008, 22,000 children died each day from poverty (most of the causes being easily preventable diseases); 148 million children in developing regions under the age of five were underweight for their age; and more than 500,000 women died each year from causes related to pregnancy and childbirth. For more detailed information, see Development Data Group [2008].

[2]For detailed information on countries that have applied different antipoverty initiatives, see Rawlings & Rubio [2005] and Fiszbein *et al.* [2009].

child's physical and cognitive development [Haas *et al.*, 1996]. In particular, healthier children more regularly attend school, whereas healthier adults with better cognitive ability are more productive and substantially increase their wages [Gertler, 2000].

Unconditional programs, which have been implemented in countries such as Ecuador and South Africa, also show positive results. In the case of Ecuador, the authors concluded that, by the age of five, children who were inadequately nourished have already fallen well behind the cognitive development of their better nourished peers. Assuming that the disparities persist, the malnourished children will have poorer performance in school, accumulate less human capital, and earn less than their peers [Paxson & Schady, 2007]. In the case of South Africa, several working papers have shown that unconditional payments bolster early childhood nutrition. However, the general consensus among authors is that antipoverty programs that condition payments on a specific behavior further increase the program's effects [Kakwani *et al.*, 2005].

In the specific case of PROGRESA, many documents have concluded that the Program achieved its goals. In the case of health and nutrition, for example, Gertler & Boyce [2001] found that treated individuals significantly increased their use of public health clinics and lowered the number of inpatient hospitalizations and visits to private providers. There was an important reduction in the incidence of illness, an increase in children's height, and a reduction in anemia. In the case of adults, the Program significantly increased the number of kilometers they were able to walk without becoming fatigued, and decreased the number of days of difficulty experienced with normal activities. In the same way, Fernald *et al.* [2008] found that PROGRESA is associated with higher height-for-age scores, a decrease in the prevalence of stunting, a decrease in the body-mass index for age percentile, and a reduction in the prevalence of being overweight; however, in a later publication, Fernald *et al.* [2009] showed that the positive effects are not that strong and, in some cases, disappear.

In the case of spillovers in health outcomes, Avitabile & Maro [2007] exploited PROGRESA's design and found significant spillover effects in the demand for specific health services in rural areas in México: non-eligible women increased the utilization of papanicolaou cervical cancer screening. Angelucci & Giorgi [2009] analyzed the indirect impact of the Program on consumption by families and concluded that non-eligible individuals benefited from their treated peers and also increased their consumption level from an increase in the money transfers, an increase in borrowing resources, and a decrease in precautionary savings. In general, there are not many studies analyzing the presence of spillovers in health outcomes in PROGRESA, but in the case of educational outcomes there are several documents studying the potential presence of spillovers: Bobba [2008], Lalive & Cattaneo [2009], Macours & Vakis [2009]. Most papers on program evaluations, however, agree on the importance of including externalities in the evaluations because failing to do so underestimates the impact: Miguel & Kremer [2004], Angelucci & Giorgi [2009], Ribas *et al.* [2010], and Durlauf & Young [2001].

In the estimation framework, the program evaluation literature has primarily used a difference-in-difference estimation [Gertler, 2000], [Behrman *et al.*, 2005]. In the case of PROGRESA, and specially for health outcomes, this method is not feasible to apply because the survey evaluations changed every year: the variables before and after the program's implementation were not always measured in the same way.[1] It is possible, however, to use a difference-in-difference estimator if we would focus on a specific age group and analyze single health outcomes, which is not the case of this paper. In fact, an important aspect of this paper is that it assesses the global impact of PROGRESA (direct and spillover impact) on health and nutrition outcomes of the entire sample involved in the Program. Other authors have used data at the individual level and applied propensity score matching methods [Diaz & Handa, 2005] [Behrman & Hoddinott, 2005].

This work estimates PROGRESA's impact using two evaluations' surveys where the dependent variables are actually comparable from one year to another. The regressions are estimated using a pooled regression at the individual level, in poor and non-poor subgroups, using different age-groups and controlling for time and state effects. Even though the Program was randomized at the locality level, it is possible to see that the randomization persists at the individual level. We estimate the impact on the health conditions of the individuals using different health and related outcomes that can be continuous, binary, or count variables. For continuous outcomes, we use a linear (OLS) estimation. For binary outcomes, we estimate a Logit specification, and for count variables, depending on their distribution, we estimate either a Poisson or a Zero-Inflated Negative Binomial (ZINB) model.

Regarding the data, the sample was composed of household socioeconomic surveys from rural areas in Mexico. The analysis is done on different age groups using different

[1] From one year to another, the questions, specially the ones related to health are not comparable either because the age-group of the respondents was different, or because the questions were formulated in a different way. For example, for individuals below 2 years old, in the first evaluations, the questionary asked about the number of visits to the doctors for nutrition checkups, while in later surveys, to the same age-group, the questionary asked about the number of visits to the doctors for health checkups. Therefore, it is not possible to assume that the answers in both cases are comparable. Maybe, for the second case, besides nutrition checkups, we also have information about health issues.

health and related outcomes. Desegregating the impact by groups eliminates the problem of misrepresentation: the results do not correspond to the average individual. The results suggest that the direct effect of PROGRESA, measured by the impact of the treatment on the targeted population, significantly improved the health status of the individuals. Specifically, children are less likely to get sick and, if sick, they face shorter periods in this state. Children also increased the frequency of visits to the doctor to monitor their height and weight. Adult individuals showed fewer problems in dealing with daily/moderate activities, and can walk longer distances before becoming tired.

The effect of PROGRESA on the non-targeted population reveal an important positive impact. Ineligible children show a significant increase in their frequency of medical checkups: ineligible households, despite not receiving the money, visit the doctor more frequently to monitor the height and weight of their children. Not only children, but also young and old individuals, exhibited shorter spells of sickness. Adult individuals improved their ability to deal with normal and difficult activities. Yet, the results also suggest that ineligible adults have more trouble when bathing and dressing. Finally, the spillover effect, measured by the effect of treated neighbors on the non-treated, showed that ineligible children aged between 0 and 12 years have shorter spells of sickness and are less likely to get sick. They also increased their visits for medical checkups.

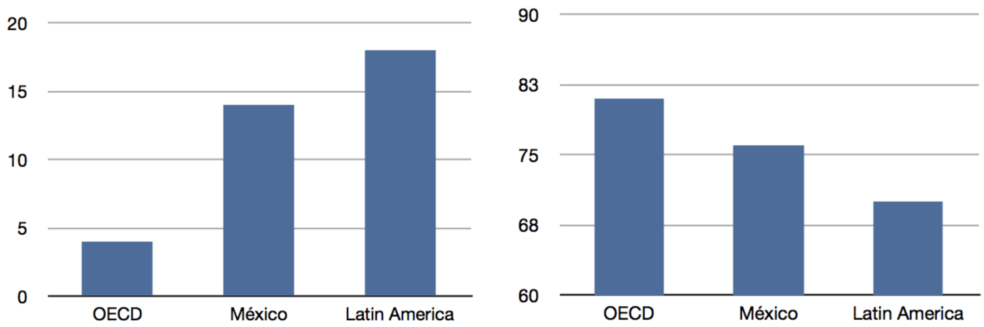
The present work is organized as follows. The next section provides a general view of the health situation in Mexico. It also gives some background information on the PROGRESA program: its main goals, structure, and design. The third section presents information and descriptive statistics that explain the organization of the sample. The fourth section describes the econometric analysis. The final section presents the estimation results and offers a brief summary and some conclusions.

1.2 Background

This section describes health in Mexico. In addition, it provides background information on PROGRESA: its main goals, design, and coverage.

1.2.1 Health Access in Mexico

The health care system places Mexico's standards a bit higher that Latin American standards but far below OECD standards. Figure 1.1 shows that, for the year 2010, the infant mortality rate in Mexico was 14 per 1,000 live births, whereas in the OECD, the rate was only 4 per 1,000 live births. The average life expectancy in Mexico was 76 years in 2010 and was 81 years in OECD countries [Development Data Group, 2010].



(a) Infant Mortality Rate (b) Life Expectancy, total (years)

Figure 1.1: Health Outcomes Comparison
Source: World Development Indicators, The World Bank [2010]

Access to health services varies widely in the country. Some workers have access to superior health care services with significant pensions, whereas most people that work in the informal sector access either public providers or affordable private health care. In general, people employed in the public sector are covered by the national ISSSTE social security institute that, on average, brings better quality health care and more generous pension benefits. Finally, people who belong to unions, because of their bargaining power, can generally access better benefit packages and services [Development Data Group, 2010].

1.2.2 The Program

PROGRESA is an antipoverty program run by the Mexican government. The program relies on conditional cash grants to poor households. It was introduced in 1997 in small rural communities and has gradually expanded to urban areas. Nowadays, it serves approximately 25 million people. Its main purposes are to improve the living conditions of eligible households and to promote investment by these households in the human capital of their children through school attendance and basic health services.

The program beneficiaries receive bi-monthly cash transfers that represent approximately 20% of household consumption. The money transfers are granted to mothers because of important evidence that suggests that, if that is the case, larger shares of these resources are directed towards child health and nutrition [Parker & Teruel, 2005]. If the mother does not live with the family, the money is granted to the person in charge of purchasing and preparing food, or to the person who takes care of the children's health and education. The program's health component relies on four specific strategies.

1. **Cash transfers.** Money transfers require the entire family to register at the nearest health care provider to schedule regular visits to monitor health and nutrition. Furthermore, the person receiving the money has to attend monthly lectures, in the form of small talks, on health and hygiene. If a family member does not fulfill one of these conditions, the cash grants are suspended. The idea of the transfers is to increase the family's income, resulting in a more frequent use of health services and an increase in food consumption. Estimates indicate that a 10% increase in income translates into a 3% to 4.5% increase in caloric availability [Behrman *et al.*, 2004].

2. **Participation in the *platicas*:** Conducted by trained physicians and nurses, the meetings teach nutrition and health topics. This training is delivered in the form of lectures to mothers. The emphasis is placed on preventive health care, including how to prevent disease through water treatment, safe food handling, and immunizations.

3. **Nutritional supplements.** The Program not only provides daily nutritional supplements to mothers with infants and small children, but also to children between four months and two years of age, and pregnant and lactating mothers. Once a month, mothers receive six packets of supplements per eligible child. The packets provide calories, protein, vitamins, and other micro-nutrients.

4. **Growth monitoring.** To receive the nutritional supplements, monitoring the growth of their preschool children is required. This monitoring enables parents to be aware of nutritional problems.

Table 1.1: Components of the Program

Components		Conditions	Services/ monthly transference
Health	Medical attention	Registration at the nearest health unit	Basic package
	Prevention of under-nourishment	Monthly visit to health unit	Nutritional supplements
	Education		Informative sessions
Feeding		Attendance at health services and informative sessions	115[1] (household)

[1] Values in Mexican pesos January–June 1999. In June 1999, the peso/dollar exchange rate = 9.4409. About 12.18 USD. (Banco de México)
Source: Análisis del Programa de Educación, Salud y Alimentación PROGRESA, RIMISP-FAO, p. 21, July 1999.

Table 1.1 summarizes the components, services, and conditions of the program that is used in this work.

The Design. The importance of PROGRESA relies on how the Program was implemented. It originally started with 506 localities randomly allocated to treated (320 localities) and control groups (186 localities). The identification of the localities was conducted, in the first stage, by a geographic focalization in which the program verified the communities' deprivation and access to health services and basic school systems. The selection used a deprivation index based on social indicators.[1] The index classified the localities into five deprivation categories: very high, high, medium, low, and very low. Localities with a high or very high deprivation level received priority for inclusion in the program.[2]

The experimental communities are located in the seven states that were among the first states to receive PROGRESA, including Veracruz, Guererro, San Luis Potosi, Hidalgo, Queretaro, Michoacan, and Puebla. Figure 1.2 shows the geographical locations of

[1] The Deprivation Index was constructed from the sociodemographic data of the XI General Population and Household Censuses of 1990 and 2000 and the 1996 Population and Household Count.

[2] For further details about the construction of the index, see General Rural Methodology Note [2005].

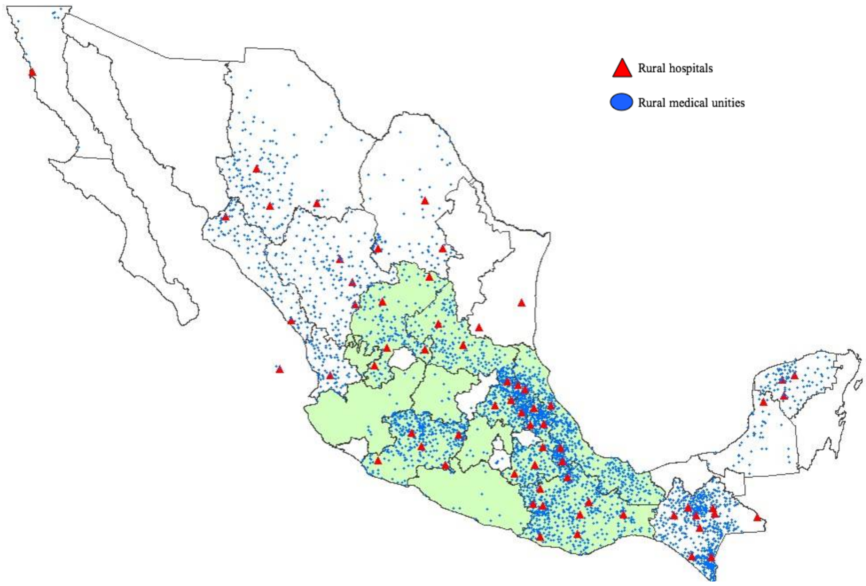


Figure 1.2: Map of Mexico: Location of the Experimental Communities (PROGRESA)
Source: Unidad IMSS-Oportunidades, SISPA, January-December 2007

the beneficiary states and the available health infrastructure around the country. By January 2008, the Program had approximately 35,848 medical units and 69 public hospitals [Lara, 2008].

During each intervention for which a locality was identified as a potential beneficiary, the Program implemented a socioeconomic data collection (ENCASEH 97) with the goal of evaluating the poverty situation of each household. Using a discriminant linear analysis, it was possible to classify families as either extreme poverty (poor) or in non-extreme poverty (non-poor). Households categorized as poor that were living in a treated locality received the cash grants. In the final phase, the results were presented in a public assembly to the community to seek comments and make corrections and additions. Once the beneficiaries were identified, the head of the family received an identification card, a unique document that provided access to the program. See Fig. 1.3.

The Program conducted survey evaluations every six months after the baseline survey in March 1998. A number of core questions about the demographic composition of the households and their socio-economic status were asked in each round: family background, schooling, health and nutritional status, health care use, consumption of food and non-food items, income, allocation of the time of household members, and productive activities. The baseline included information on 112,319 individuals in the experimental communities, of which approximately 60% belonged to treated areas.

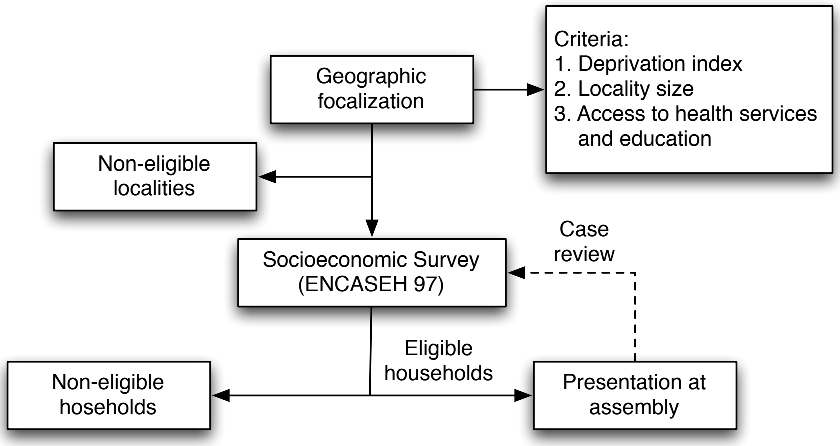


Figure 1.3: PROGRESA: Criteria Selection
Source: General Rural Methodology Note, SEDESOL, November 2005

Figure 1.4 describes the organization of each locality by beneficiary household and their socio-economic condition. Each locality includes both poor and non-poor families, and only poor families in the treated villages received the cash grants. The important point with respect to PROGRESA's structure is that the poor/non-poor groups of families in the treated and control localities have exactly the same background characteristics. Thus, comparing the situation among the groups was possible before, during, and after the implementation of the program.

In this way, the direct impact of PROGRESA is compared with respect to the health outcomes between poor families in the treated and control areas (A_T). The spillover effect, or indirect impact, is estimated by the effect generated by the poor treated families on their non-poor neighbors living in treated localities, (B_T). In other words, the estimation of the spillover effect assumes that the families receiving the money transfers will impact their neighbors through the change in their behavior. In this way, non-poor families in treated localities will improve their health condition through their interactions with poor families more than will the non-poor families living in the control villages (A_{NT}).

This document assumes that the positive externalities of PROGRESA are generated from the impact of the behavior of poor-treated families on the behavior of their non-poor neighbors. This study sets aside the notion of a potential impact on non-poor families from an improvement in the health of poor families. This assumption is justified because all health outcomes included in the analysis are primarily related to non-transmissible diseases so that the impact on the health of the non-treated families can only happen

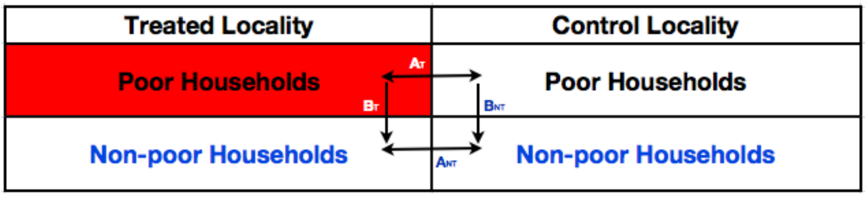


Figure 1.4: PROGRESA: Villages Structure

from a change in their behavior, not from a decrease in the transmission of disease. We also set aside the notion of an effect from the money transfers on the non-poor families because the channels through which this could happen are not clear. In fact, many studies on PROGRESA suggested that most of the resources of the transfers are spent on health services and education: the localities have very few shops or stores through which the money could have been transferred from one family to another [Lalive & Cattaneo, 2009].

1.3 Descriptive Statistics

The data are divided between the control and treatment groups; within each group, people are categorized as poor (eligible) and non-poor (ineligible). Table 1.2 describes PROGRESA's coverage. The participation of poor people in the program increased each year. In 1997 (the baseline), almost 37% of the individuals categorized as poor received the treatment; by 2000, this share was 49.4%. After 2003, all eligible individuals became part of the program, thus there is no information regarding the share of poor and the non-poor.

Table 1.2: PROGRESA Composition[1]

	Baseline		**1998**		**1999**		**2000**	
	Treated	Control	Treated	Control	Treated	Control	Treated	Control
Non-poor	0.247	0.165	0.242	0.161	0.118	0.082	0.112	0.075
Poor	0.368	0.221	0.372	0.224	0.484	0.316	0.493	0.320
N	125449		130279		131668		130889	

[1.] Values are ratios respect the total number of individuals (N).
Source: PROGRESA evaluation Data

Table 1.3 provides details about the main variables of the baseline survey (ENCASEH 97). The summary statistics describe the socio-economic characteristics of the sample. Each variable is divided into the treated and control groups, and eligibility condition.

Additionally, the table reports the results of a t-test on the mean differences between groups (treated–control). The results suggest that the randomization worked almost perfectly in the sample: there are no significant differences between the treated and the control group.

Table 1.3: Descriptive Statistics: Baseline (ENCASEH 1997)

Variable	Poor			Non-poor		
	Treated	Control	Diff.[1]	Treated	Control	Diff.[1]
Baby (0-5 years)	0.206	0.206	0.000 (0.935)	0.103	0.103	0.000 (0.956)
Child (5-12 years)	0.245	0.246	-0.001 (0.732)	0.142	0.139	0.004 (0.243)
Young (12-18 years)	0.143	0.142	0.001 (0.863)	0.162	0.167	-0.005 (0.319)
Adult (18-65 years)	0.378	0.376	0.002 (0.638)	0.518	0.519	-0.001 (0.717)
Old (>65 years)	0.028	0.030	-0.001 (0.297)	0.075	0.073	0.002 (0.540)
Male	0.500	0.500	0.000 (0.523)	0.512	0.508	0.004 (0.431)
Parents' education[2]	0.246	0.229	0.017 (0.077)	0.189	0.177	0.012 (0.199)
Actual schooling	0.378	0.375	0.003 (0.369)	0.246	0.240	0.007 (0.100)
Secondary school[3]	0.030	0.027	0.002 (0.192)	0.031	0.029	0.001 (0.525)
Health disabilities[4]	0.062	0.063	-0.001 (0.652)	0.052	0.056	-0.004 (0.123)
Employment[5]	0.229	0.232	-0.002 (0.450)	0.365	0.359	0.006 (0.169)
Income[6]	36.01	34.44	1.572 (0.107)	49.28	51.06	-1.780 (0.286)
Mothers' age at first job	16.50	15.58	0.919 (0.126)	17.332	17.679	-0.347 (0.585)
Running water[7]	0.175	0.176	-0.001 (0.734)	0.377	0.366	0.011 (0.525)
Wall: asbestos[7]	0.001	0.001	0.000 (0.096)	0.002	0.001	0.001 (0.339)
Wall: metal[7]	0.001	0.001	0.000 (0.690)	0.001	0.001	0.000 (0.777)

* $p < 0.10$, ** $p < 0.05$, + $p < 0.01$. Standard errors in parenthesis.
[1] t-test difference between the treated and control groups.
[2] Refers to parents who completed primary school.
[3] Refers to individuals who are attending secondary school.
[4] % of families that have at least one member of the family blind, mute, deaf, mentally or physically disabled.
[5] Individuals who engaged in a productive paid activity the week before the survey.
[6] Reported income on a daily basis.
[7] % of families that have access to water, or live in a house with that facility.

As to the sample's composition and social background, the sample is divided into five groups: baby (0–5 years), child (5–12 years), young (12–18 years), adult (18–65 years), and old individuals (older than 65 years). This division is attributable to the surveys' composition: different questions were asked to different people depending on their age. Additionally, when analyzing health outcomes, it is important to make distinctions because a person's health status depends on various characteristics conditioned on age. A check of the data shows that the poor group contains an important percentage of young people (0–18 years), approximately 60%, whereas the non-poor group contains a lot of middle-age people (18–65 years). In contrast, both groups have similar ratios of men and women. In the case of education, parents mostly only attended primary school. One-third of the sample is now attending school. If we contrast this number with the number of individuals under the age of 18 years old (60%), who are supposed to be engaged in educational activities, it is possible to see that a great part of them are not studying any more. This indicator is even worse when comparing the percentage of people in high school (3%).

A potential indicator of health status is given by the variable "health disabilities," which indicates whether one of a family's members has a physical problem such as blindness, deafness, or the loss of a limb. The statistics show that the control and treated groups present very similar values. On average, 6.2% of the poor have a family member with some kind of disability. This indicator is a bit lower for the second group, at approximately 5%.

Labor conditions among the eligible and ineligible groups varied considerably. Whereas in the first group only 23% of the individuals able to work reported being actually employed, in the ineligible group this rate increases to 36%. Furthermore, the non-poor group, on average, earned per day 15 pesos more than people in the poor group.

Then, a number of variables describing "social background" were reported. The results suggest important differences between the poor and non-poor groups, but in terms of treatment and control, the differences are insignificant. In the poor sample, only 17.5% of the households have access to running water, whereas this variable is almost 38% in the second group.

Finally, two variables define the wall characteristics of the houses, but the results suggest that not a lot of people live in houses with asbestos and metal. The randomization of the Program allows its evaluation using simple mean differences. For this reason, and before addressing the econometric application, Table 1.4 shows the effect of PROGRESA on health outcomes in different years. For sickness incidence, the values show that people who did not receive the treatment were sick more frequently than people who received the cash transfer.

Furthermore, this impact is not significant in the first year of the program (1998); however, in later years, the effect becomes stronger and statistically significant. An important fact is that the results show a more significant impact on the non-poor group than on the poor group, suggesting the presence of important externalities. Additionally, people who belong to the treated group seem to stay sick for shorter periods than do people who were not in the program. However, in this case, the impact was greater for the eligible group.

Table 1.4: Descriptive Evidence of the Cash Grants on Health Outcomes

Variable	Poor			Non-poor		
	Treated	Control	Diff.[1]	Treated	Control	Diff.[1]
Sickness incidence in 1999[2]	0.188	0.192	-0.004 (0.080)	0.133	0.145	-0.012+ (0.001)
Sickness incidence in 2000[2]	0.203	0.211	-0.008+ (0.002)	0.264	0.290	-0.027+ (0.000)
Sickness spell in 1999[3]	9.429	10.14	0.711+ (0.007)	10.94	11.56	-0.623+ (0.269)
Sickness spell in 2000[3]	9.874	10.75	-0.876+ (0.006)	11.48	11.83	-0.356 (0.586)
Weight monitoring in 1999[4]	0.932	0.917	0.015 (0.026)	0.911	0.912	-0.001 (0.515)
Weight monitoring in 2000[4]	0.923	0.920	0.003 (0.447)	0.869	0.891	-0.021 (0.171)
Weight monitoring (times) in 1999[6]	3.526	3.112	0.413+ (0.000)	3.235	3.181	0.054 (0.253)
Weight monitoring (times) in 2000[6]	4.795	4.568	0.227 (0.561)	4.041	4.096	-0.055 (0.686)

* $p < 0.10$, ** $p < 0.05$, + $p < 0.01$. Standard errors in parenthesis.
1. *t*-test difference between the treated and control groups.
2. Ratio of people who reported a sickness during the previous 4 weeks.
3. Days people reported being sick.
4. Children younger than 5 years old that in the last six months had their weight and height monitored.
5. Number of times a child was weight-monitored in the previous year.
6. Number of times a child was weight-monitored in the previous 6 months.

Weight monitoring improved for the eligible group. Although these results are not surprising, given that people were entitled to regular health checkups, it is important to point out that people receiving cash transfers significantly increased their visits to the doctor to check the health and nutrition of their child; hence, the program is accomplishing its main goals.

1.4 Identification Framework

1.4.1 The Data Set

The empirical analysis includes information about two different surveys from rural areas in Mexico. The total sample has observations from 125,028 individuals interviewed in 1999 and 2000. In total, the analysis has 262,577 observations. The analysis does not include the year 1998 because most of the questions related to the health status of a family are not the same as those in the later surveys. In addition, although evaluations after the year 2000 already exist, the surveys had many inconsistencies[1]; therefore, this document limits the information to the year 2000.

Furthermore, the analysis only uses data from the second yearly round of surveys: October–November. Most of the families are engaged in agricultural activities during the first round (March–April), so many family members go from one place to another in search of job opportunities. Therefore, including the first round considerably decreases the number of observations in the sample compared to the second round because many members of the family are not present at the moment of the survey.

The inclusion of only rural areas is justified for two reasons. First, in urban areas the targeting mechanism was substantially different and many authors agree that an element of self-selection exists [Parker & Teruel, 2005]. However, this conclusion is only valid for evaluations made before the year 2000 Gertler & Boyce [2001] and Gertler [2003]; after this year, attrition bias becomes a serious concern, even in rural areas [Lagarde *et al.*, 2009]. The second reason is because the presence of externalities is estimated based on the assumption that interactions among families is easier in rural villages. For urban information, this assumption is not likely to hold[2].

Although the data set has a panel structure, the use of fixed effects is not the best

[1]Many families showed an important increase in the number of children they have. This could be the result of mistakes in the data recollections, or there are some pervasive incentives on the fertility rate of families. However, in some cases, the number of newborn child exceeds the possible for the time in analysis: it seems that some families are inscribing untreated children as their own.

[2]In fact, families in rural areas are related to similar productive activities. Besides, since the Program's design targeted small villages, that had, in most of the cases, just one school, one medical institution, one hospital, it is very likely that families, within a village, interact among them constantly. In contrast, in urban areas the type of productive activities differs from one family to another, and the distances become a problem when trying to contact other people. Besides, in big cities individuals have many public schools, hospitals; therefore, assume a frequent contact among PROGRESA's individuals is very difficult.

option because the "treatment" variable is time-invariant: once individuals become part of the Program, they keep their status. Therefore, the implementation of fixed effects will eliminate all of the individuals that started with the Program and will only keep the "new observations." For this reason, the econometric analysis uses the data set as a cross-section.

Finally, to deal, in some extent, with unobserved heterogeneity, besides including specific controls for households' characteristics, the estimations include controls for the state location of each village (seven states in total), and controls for the year of the survey. We do not account for village fixed effect because it imply the inclusion of 506 extra variables, which makes the estimation time cumbersome, and, given the nature of the econometric models applied, in some cases, the software does not find a solution. In fact, most of the econometric models applied are nonlinear specifications based on maximum likelihood estimation (MLE). However, we allow for correlation by clustering the errors at the village level.

1.4.2 The Model

The topic of treatment evaluations concerns measuring the impact of the interventions on the outcomes of interest. A measure of the causal impact is the average difference in the outcomes of the treated and non-treated (control) groups. Within a framework of a potential outcome model, which assumes that every individual in the population is potentially exposed to the treatment, the outcomes of the different groups form a basis for treatment evaluation.

Define $D_v \in \{0, 1\}$ as the variable that describes whether a person lives in a treated village, and H_{i1} and H_{i0} as the health outcomes for whether an individual i lives in treated or control village v. Therefore, the effect of the cause D_v on the health, or related outcome H_{iv} is estimated by the next equation:

$$H_{iv} = \varphi_0 + \varphi_1 D_v + \pi_1 n_{iv}^P + \pi_2 N_{iv}^T + \sum_k \gamma_k X_k + \mu_{iv} \tag{1.1}$$

For simplicity of exposition, Eq.(1.1) is written in linear form, but in the estimation we also use a Logit and a ZINB model because H_{iv} can be a binary or a count variable. The equation is estimated by poor and non-poor subgroups. Since local population density may affect disease transmission, the regression includes the percentage of poor peers (n_{iv}^P) individual i has in the village. In the same way, to determine the potential spillover effect from the elegible individuals to the program (=poor) to the ineligible (non-poor), the equation includes the total number of treated peers (N_{iv}^T) individual i has. While for n_{iv}^P we expect a negative impact because a greater density of poor neighbors imply worse conditions in the village, for the second term (N_{iv}^T), we expect the reverse effect: poor treated individuals are supposed to improve their health status due to PROGRESA, which will positively impact their closer peers.

A peer is defined as an individual who lives in the same village, is part of the same age group, attends the same schooling level, and lives nearby individual i i.e., in a radius of 3km.[1] Therefore, to construct the variables ${n^P}_{iv}$ and ${N^T}_{iv}$, we use the GPS coordinates of every household in the village to compute the distance between i and all their peers. In addition, we assume that closer peers have a higher effect on the health status of i than peers located far away, so in the computation of the number of peers, we weighted each peer by their distance.

Specifically, define d_{ij} as the distance in kilometers from individual i to peer j, where $d_{ij} \leq 3$; P_j determines the poverty condition of the peer j, and D_v whether i and j live in a treated village:

$$\begin{aligned}
\text{total number of neighbors:} \quad & N_{iv} = \sum_j \frac{1}{d_{ij}} \\
\text{total number of poor neighbors:} \quad & N_{iv}^P = \sum_j \left(\frac{1}{d_{ij}} * P_j \right) \\
\text{total number of treated neighbors:} \quad & N_{iv}^T = \left(\sum_j \frac{1}{d_{ij}} * P_j \right) * D_v \\
i = 1, 2, 3, ..., N \quad ; \quad & j = 1, 2, 3, ..., n \quad ; \quad i \neq j,
\end{aligned}$$

where finally, the percentage of poor peers is given by:

$$n_{iv}^P = \frac{N_{iv}^P}{N_{iv}}$$

Figure 1.5 shows how the model incorporates the estimation of this variable. For each individual, depending on his/her age group, we take into account the number of indi-

[1] We chose 3km. as the maximum distance because it corresponds to the average distance a person reported to walk daily.

viduals living nearby. Given a spatial connection, poor treated individuals can influence their closer peers by just interacting with them.

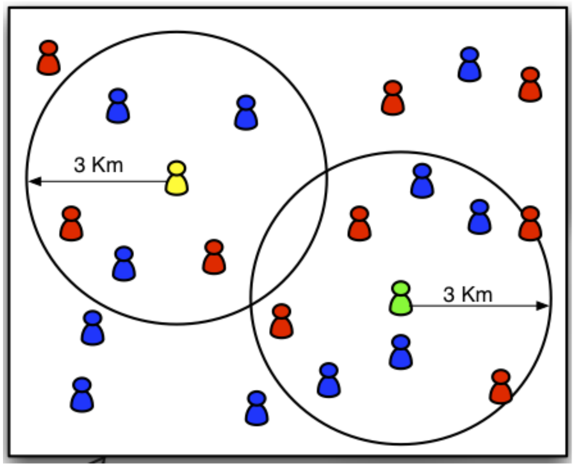


Figure 1.5: PROGRESA: Interactions within villages

The estimation divided the groups by age because it is reasonable to think that not everybody is related or socializes with everybody else in the villages. The interactions among parents are not the same as those among children or young individuals. Given the village characteristics, and taking into account socio-economic conditions, it is very difficult to think of different types of interactions rather than the one given by closeness when analyzing health outcomes.

The dependent variable, H_{iv}, takes different values defining each group's health status. If $H_{iv} \in \{0, 1\}$, this work uses a logit specification. When H_{iv} is continuous, we use a linear model; when H_{iv} is a binary variable, we use a Logit specification, and when it is a count variable, such as the number of days an individual reported being sick, this document estimates a zero inflate negative binomial model.[1] If the count variable has

[1]The negative binomial distribution is given by

$$Pr[y|x] = \frac{\Gamma(y + \alpha^{-1})}{y!\Gamma(\alpha^{-1})}(\frac{\alpha^{-1}}{\alpha^{-1} + \lambda})^{\alpha^{-1}}(\frac{\lambda}{\alpha^{-1} + \lambda})^{y}.$$

In the context of count regression models, the negative binomial distribution can be thought of as a Poisson distribution with unobserved heterogeneity that, in turn, is conceptualized as a mix of two probability distributions, poisson and gamma.

This specification supplements a count density with a binary process. If the binary process takes the value 0 with probability $f_1(0)$, then $H_{igv} = 0$. If the binary process takes the value of 1 with a probability of $f_1(1)$, the $H_{it} = 0, 1, 2....$ This definition allows zero counts to occur in two ways: as a realization of the binary process and as a realization of the count process. The an excessive number of zeros, the model that best fits the distribution is a Zero-Inflated Negative Binomial ZINB. This model is applicable when a Poisson estimation is inappropriate because of over-dispersion (the general behavior of the explained variables). In a Poisson distribution, the mean and variance are equal. If the variance is greater than the mean, the distribution is said to display over-dispersion.[1]

A zero-inflated negative binomial regression generates two separate models and then combines them. First, a logit model (inflate) is generated for the "certain zero" cases, predicting whether an individual is in this group. Then, a negative binomial model is generated that predicts the counts for individuals who are not certain zeros.

H_{iv} is defined by different health and related outcomes intended to measure the health status in the sample. It is important to notice that some of the outcomes are not always an indicator of health, but instead they represent the behavior of individuals. Specifically:

1. **Sickness incidence:** reports on whether an individual was sick in the last month;
2. **Days sick:** if a person reported sick last month, this variable gives the number of days in that state;
3. **Sickness diarrhea, fever, cough, respiratory problems, other:** reports whether a person had any of these sicknesses in the last month;
4. **Monitor height and weight:** reports whether the baby visited the doctor to monitor his/her height and weight in the last year;
5. **Monitor weight (times):** number of times a child between 0 and 2 years old visited a doctor in the last 12 months;
6. **Problems with normal, hard, moderate, and simple activities:** reports whether a person had trouble dealing with any type of activities;
7. **Problems walking:** reports whether a person had any trouble walking;
8. **Problems dressing:** reports whether a person, at the time of the interview, can dress by themselves; and,

density for this specification is defined by

$$g(y) = \begin{cases} f_1(0) + (1 - f_1(0))f_2(0) & \text{if } H_{igv} = 0 \\ (1 - f_1(0))f_2(y) & \text{if } H_{igv} \geq 1 \end{cases}$$

[1] In Appendix A (refer to Sect. A.1) a summary statistics plus a histogram for each count variable is reported to check whether a Poisson or a ZINB model was more appropriated for the estimation.

9. **Number of km able to walk:** reports the number of kilometers a person is able to walk before getting tired.

In the case of the "monitoring" variables, for example, this document assumes that a higher frequency of visits to the doctor to monitor height and weight has a positive effect on the health of children. In fact, one of the main problems in poor populations is that people are reluctant to maintain a preventive behavior respect to health, and they only visit a doctor when they are already sick. For this reason, antipoverty programs, such as PROGRESA, work not only by reducing the incidence of specific diseases, but they also target individuals' behavior. The goal is that people keep a positive behavior to prevent many diseases at their initial stage.

The variables referring to problems to develop certain activities, such as walking, dressing, or the ability to perform different strength tasks, are intended to capture the state of health of older individuals. Given the sample characteristics, having an objective record of peoples' health status is very difficult. Therefore, to measure and track, in some sense, individuals health status, the surveys designed direct and simple questions. Nevertheless, the interpretation of the results must be cautious. If a person, for example, shows less problems dealing with normal activities, it might be because, due to the program, he or she is getting better quality food. When analyzing the potential peer effect, however, the interpretation is less obvious. If a person living in an untreated household reports to have less problems dealing with some activities, it might be because there is a spread of information from his/her treated peers about the services the hospital, or the medical unities provide in the village. This will be translated in a higher demand of healthcare services from the untreated individuals, which, in turn, will show an improvement of their health status.

Finally, the vector X_i contains the background characteristics that are balanced between the treated and the control groups. Among these controls, we include: gender, parents' education, working status of the mother, whether the father is absent, migration in the family, number of family members, age controls, year controls, and state controls.

The descriptive evidence presented in Section 3 shows that the randomization process that assigned individuals to the control and treatment groups was successfully implemented. The random assignment implies that individuals exposed to the treatment are chosen randomly, indicating that the treatment assignment does not depend on the outcome and is uncorrelated with the attributes of the treated subjects, i.e., $E(\mu_{iv}|D_v) = 0$. An evaluation of Eq. (1.1) determines the average treatment effect (ATE) in a linear framework:

$$\text{ATE}: \varphi_1 + \pi_1 E \underbrace{\left\{[n_{iv}^P|D_v = 1] - [n_{iv}^P|D_v = 0]\right\}}_{=0} + \pi_2 E[N_{iv}^T|D_v = 1] \tag{1.2}$$

Due to randomization, the second term (n_{igv}^P) is balanced between control and treated groups, so we can set it aside. The ATE, therefore, is composed by two effects. The first, or **direct effect**, which depends on whether an individual lives in a treated village, and the second, or **indirect effect**, which estimates the effect of poor treated peers on the health status of individual i[1].

Equation (1.2) is estimated by different age groups in poor and non-poor subgroups. While φ_1 for the poor group is given by the cash grants, for the non-poor group this term can be interpreted as the extent of the externalities of the program i.e., a potential share of resources, or an increase in the medical utilization of hospitals or medical unities by the non-poor. The last term estimates the effect of having more treated peers on the health status of i. In fact, healthier individuals create a healthier environment which also decreases the disease transmission; however, besides contagious diseases, in our data set most of the health outcomes evaluated cannot be thought as contagious, therefore, this last term can be also thought as the effect of the behavioral change of treated peers on their closest friends.

[1]For nonlinear models, [Wooldridge, 2002] and [Angrist & Pischke, 2009] suggest that the definitions of ATE is valid for any response variable either if it is binary, non-negative, or continuously distributed. By definition, ATE is $E[H_{i1} - H_{i0}] = E[H_{i1}] - E[H_{i0}]$, and for this to be well defined the expected value has to exist. If H_{iv} is binary, the expected values are probabilities of success: ATE$=Pr[H_{i1} = 1] - Pr[H_{i0} = 0]$. The coefficients do not give us the size of the effect of D_v on the health status of individual i, though they do have the right sign.

1.5 Estimation Results

The estimation results are detailed in Appendix A. Each equation is reported with all of the covariates included in the estimation. The analysis has been done by age-groups and eligibility conditions. The results next summarized are related to the probability estimated in the tables. Because most of the estimations were done with non-linear models, plus the fact that the distribution of the variables required more sophisticated models, the estimation of the marginal effects is cumbersome and therefore not presented.

1.5.1 Effect of PROGRESA on Poor Individuals

Since PROGRESA targets only poor individuals, the effect of the Program is determined by the effect of the cash-grants, plus the effect of having treated peers in the village. The results suggest the cash grants, in fact, significantly improved the health status of small child (babies). For older individuals, even though the program show to have positive effects on the health status of individuals, the results are not significant. In contrast, the effect of treated peers show important effects non only on babies, but on the health status of all individuals. In a more detailed way, see Table 1.5:

Table 1.5: Effect of PROGRESA on Poor individuals

	Cash Grants	**Treated Peers**
Babies	Higher probability to visit a doctor for general checkups.	Lower probability of getting sick.
	Higher frequency to monitor height and weight.	Decrease in the sickness spell.
		Lower probability of getting sick: Cough..
Children	Do not show significant effects.	Do not show significant effects.
Young	Do not show significant effects of the program.	Lower probability of getting sick.
Adult	Do not show significant effects of the program.	Decrease in problems with normal activities.
Elderly	Do not show significant effects of the program.	Decrease in problems with difficult activities.
		Decrease in problems with moderate activities.
		Decrease in problems when simple activities.

The table summarizes the results presented in Appendix A for the poor group.
Source: Results of Estimation.

1.5.2 Spillover Effect: Effect of PROGRESA on the Non-Poor

An interesting fact about PROGRESA is that the estimation results show an important impact on people who do not receive the cash grants (=non-poor) but live in treated villages. This result can be explained by two factors. First, the presence of externalities generated by the program itself, such as an increase in the medical suppliers in the village, or a share of the extra resources due to market transactions. And, second, by the effect of treated peers on their untreated friends: healthier treated individuals will have a positive effect on the health status of their peers.

The interaction between poor treated and the non-poor untreated individuals can decrease the disease contagion due to an improvement in the health status of the poor group, or it could also be that the conditional behavioral change imposed on the treated individuals provokes a change in the health decisions of the non-poor group. In general, the effect of the program on the untreated group living in a treated village is summarized in see Table 1.6:

Table 1.6: Effect of PROGRESA on Non-Poor individuals

	Live in Treated Village	**Treated Peers**
Babies	Higher frequency to monitor height and weight.	Decrease in the sickness incidence.
	Increase in sickness incidence: cough	More likely to stay healthy.
		Higher probability of getting sick: Diarrhea.
		Lower probability of getting sick: Cough.
Children	Do not show significant effects of the program.	Do not show significant effects of the program.
Young	Longer sickness spell.	Decrease in the sickness incidence.
Adult	More likely to be healthy.	Decrease in problems with normal activities.
	Decrease in problems when simple activities.	
	Decrease in problems when daily activities.	
Elderly	Decrease in problems with moderate activities.	Increase in problems with moderate activities.
	Decrease in problems with simple activities.	
	Decrease in problems to walk 2km.	

The table summarizes the results presented in Appendix A for the non-poor group.
Source: Results of Estimation.

Since the program shows important effects on the health conditions on individuals, not only due to the cash transfers, but due to the presence of spillovers, we estimate the evolution of the response variables given the number of peers, and compare the results between individuals living in treated to those in control villages. The goal is to determine whether having more peers part of the program will differ if an untreated person lives in a control or treated village. To do so, we use a non-parametric graph of the impact on the health outcomes of the number of eligible peers (poor individuals living in the same village).

Figs. A.3 to A.8 show in detail these results. The graphs were constructed using a non-parametric estimation that regress the predicted results of Eq. (1.1) on the number of poor peers a non poor individual has. The spillover is given by contrasting the behavior on the health status of non-poor individuals living in treated villages (red line), with the health status of non-poor individuals living in control villages (blue line). As shown, as the number of poor neighbors increases, the health status of individuals not receiving the cash grants improves in the treated villages. In contrast, the health status of non-poor individuals living in control villages deteriorates as the number of poor neighbors increases.

In general, without any type of intervention, more poor peers imply worse conditions in control villages. In contrast, living in a treated village, in spite of not receiving the grants, improves the health conditions of individuals. This result describes how CCT programs not only impact people directly targeted by the program, but it also affects indirectly non targeted individuals through the presence of externalities and social interactions.

1.6 Summary and Conclusions

This study addresses the impact of an anti-poverty program on health outcomes. The Program combined a traditional cash transfer with financial incentives for families to invest in the human capital of their children: nutrition, health, and education. To receive the cash transfers, the families were required to participate in growth monitoring and nutrition supplement programs, and to regularly attend educational programs about health and hygiene.

This paper studies the direct impact and the potential spillovers generated by PROGRESA. Whereas the direct effect is given by the improvement in the health status of the targeted individuals, the externalities account for the effect that treated individuals have on their nearest untreated peers. The data set includes information of 125.028 individuals, interviewed in two years during the Program's implementation. The estimation was done using different health and related outcomes including all individuals in the sample, going from babies to old individuals. In addition, the estimation took into account the distribution of each measured outcomes and introduced different methods: lineal and non-linear.

The results suggest that the Program actually improved the health status of the targeted population. The impact was measured using several health and related outcomes from different age groups. In general, treated individuals exhibit better health conditions based on their sickness incidence, and sickness spells. Moreover and importantly, families receiving the cash grants increased their probability of visiting a doctor and the frequency of their medical checkups. Young individuals decreased their probability of getting sick, adult individuals were more likely to deal with normal activities, and elderly individuals seemed able to deal with difficult, moderate and simple activities.

Regarding the spillover effect, the results are surprisingly significant. People who live in treated villages but are ineligible for the Program improved their health status, measured by their probabilities of being sick, and their capacity to deal with different daily activities. We attribute this result to the presence of externalities, and the effect that goes from the treated to the non-treated: a more healthy environment, plus the conditioned behavior in one family is spread to those not included in PROGRESA.

In general, the results suggest that the targeted population, in spite of showing a significant positive effect, do not benefit that much from the Program compared to the non-targeted group. This result can be puzzling; however, it might be also a reinforcement of the argument of the importance to account for the presence of externalities. The fact that untreated individuals also benefit from the Program makes that the estimation of the impact on the treated group be underestimated. In other words, since PROGRESA has an impact beyond its limits, untreated individuals are catching up with their treated peers, which, in turn, will show a lower effect on the treated group.

To summarize, the results show that PROGRESA achieved most of its goals, and, in general, the health condition of the treated individuals improved. The most important fact is that the presence of social interactions leaves the door open to an analysis of long-run effects. We agree that money transfers raised an important question about their validity: what happens when eligible individuals stop receiving the money? However, the presence of positive spillovers, in this type of government intervention, importantly increases the impact of such programs. More than that, these externalities may suggest that individuals are actually changing their behavior maybe through a learning process and not only as a response to the monetary grants.

Appendix **A**

Appendix - Paper 1

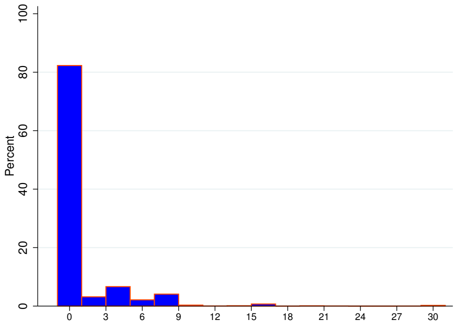
A.1 Variables Description

Table A.1: Summary statistics

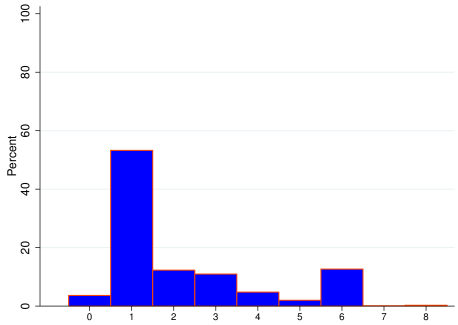
Age-Group	Variable	Mean	Variance	Min.	Max.	N
Babies	nutrition checkups	2.19	3.28	0	8	13.101
	days sick	0.98	8.18	0	30	31.383
Children	days sick	0.06	1.02	0	30	47.290
Young		0.05	1.00	0	30	34.826
Adult		0.26	5.71	0	30	97.028
Elderly		2.22	52.7	0	30	12.062

Notes: Statistics of the count variables included in the regressions as dependent variables

A.2 Histogram: Count Variables

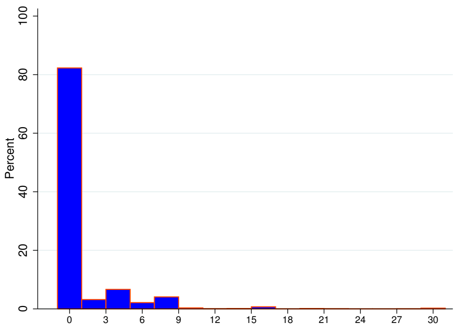


(a) Number Health Checkups

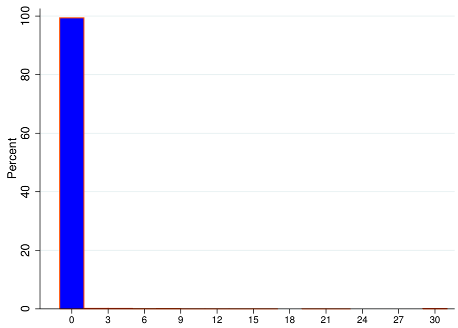


(b) Days Sick

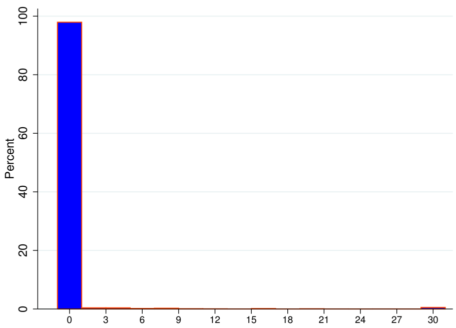
Figure A.1: Age group: Babies



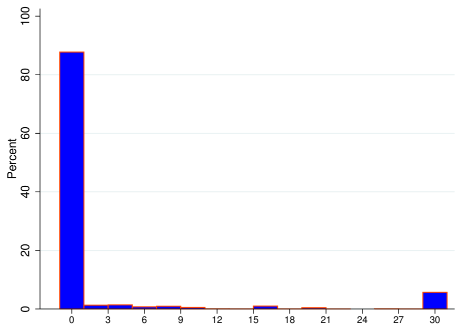
(a) Children



(b) Young



(c) Adults



(d) Old

Figure A.2: Number of days sick by age group

A.3 Model Estimation Results

Table A.2: Baby Health Conditions I

	Logit		ZINB		Logit		Poisson	
	Sickness Incidence		Days Sick		Weight Monitor		Weight Monitor (Freq.)	
	Non-poor	Poor	Non-poor	Poor	Non-poor	Poor	Non-poor	Poor
treatment	0.213	−0.008	−0.135	0.026	−0.538	0.587$^+$	0.104*	0.113$^+$
	(0.169)	(0.075)	(0.103)	(0.037)	(0.424)	(0.170)	(0.055)	(0.027)
% poor neighbors	0.050	−0.169	0.160	0.015	−0.327	0.186	−0.247**	−0.051
	(0.308)	(0.120)	(0.286)	(0.100)	(0.931)	(0.349)	(0.114)	(0.044)
# treated peers	−0.007**	−0.002**	0.001	−0.001**	0.010	−0.003	−0.000	0.000
	(0.003)	(0.001)	(0.002)	(0.001)	(0.008)	(0.002)	(0.001)	(0.000)
male	0.115	0.074**	0.050	−0.025	0.072	0.013	−0.071**	0.006
	(0.100)	(0.037)	(0.080)	(0.032)	(0.258)	(0.103)	(0.035)	(0.014)
village density	0.237	0.065	0.031	−0.023	0.146	0.074	0.160**	0.040
	(0.192)	(0.077)	(0.148)	(0.063)	(0.457)	(0.209)	(0.065)	(0.027)
parents' education	−0.650*	−0.075	−0.300**	−0.159$^+$	−0.144	0.080	−0.155**	−0.022
	(0.385)	(0.105)	(0.132)	(0.062)	(0.375)	(0.287)	(0.074)	(0.033)
father absent	0.018	−0.195	0.200	−0.030	−0.076	0.056	−0.102	−0.019
	(0.307)	(0.148)	(0.148)	(0.077)	(0.576)	(0.326)	(0.092)	(0.049)
mother works	0.083	0.302*	−0.210	−0.161*	−0.846	−0.514*	−0.180*	−0.041
	(0.324)	(0.168)	(0.149)	(0.086)	(0.625)	(0.302)	(0.108)	(0.060)
parents work	0.103	−0.346$^+$	0.235**	0.008	0.152	0.302	−0.027	0.073**
	(0.189)	(0.103)	(0.116)	(0.056)	(0.434)	(0.239)	(0.066)	(0.037)
people in family	−0.070$^+$	−0.074$^+$	−0.026**	−0.007	−0.035	0.003	0.005	−0.002
	(0.019)	(0.010)	(0.013)	(0.006)	(0.055)	(0.022)	(0.006)	(0.003)
cons	−0.755**	−0.826$^+$	1.715$^+$	1.485$^+$	2.910$^+$	2.169$^+$	1.280$^+$	1.150$^+$
	(0.337)	(0.148)	(0.240)	(0.097)	(0.871)	(0.402)	(0.119)	(0.054)
inflate								
treatment			−0.255**	0.005				
			(0.126)	(0.048)				
% poor neighbors			−0.022	0.229*				
			(0.328)	(0.125)				
# treated peers			0.007**	0.002$^+$				
			(0.003)	(0.001)				
cons			0.776**	0.784$^+$				
			(0.307)	(0.123)				
lnalpha								
cons			−0.845$^+$	−0.995$^+$				
			(0.128)	(0.062)				
year effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
state effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
age groups	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
LR chi2	71	321	105	82	26	63	1641	5444
Log-likelihood	−1193.287	−8731.103	−2516.927	−16238.174	−243.088	−1472.461	−1565.508	−11243.347
N	2237.000	19079.000	2214.000	18814.000	1063.000	7434.000	1021.000	7150.000

* $p < 0.10$, ** $p < 0.05$, + $p < 0.01$. Standard errors in parenthesis.

Table A.3: Baby Health Conditions II

	Logit		Logit		Logit		Logit		Logit	
	Diarrhea		Fever		Cough		Respiratory Problems		Other	
	Non-poor	Poor	Non-poor	Poor	no pobre	pobre	no pobre	pobre	no pobre	pobre
treatment	−0.393	−0.034	0.175	−0.041	0.511**	0.103	−0.063	0.020	−0.197	−0.051
	(0.352)	(0.190)	(0.250)	(0.106)	(0.208)	(0.101)	(0.326)	(0.165)	(0.386)	(0.187)
% poor neighbors	0.910	−0.099	0.258	−0.220	0.115	−0.110	−0.148	−0.547*	0.567	−0.379
	(0.888)	(0.393)	(0.500)	(0.183)	(0.385)	(0.175)	(0.733)	(0.305)	(0.855)	(0.441)
# treated peers	0.008*	−0.001	−0.006	−0.001	−0.013+	−0.004+	−0.011	−0.002	−0.010	−0.003
	(0.005)	(0.003)	(0.005)	(0.001)	(0.004)	(0.001)	(0.009)	(0.002)	(0.007)	(0.003)
male	0.298	0.133	0.163	0.078	0.070	0.073	−0.197	0.041	0.081	−0.119
	(0.332)	(0.128)	(0.180)	(0.057)	(0.137)	(0.051)	(0.230)	(0.092)	(0.309)	(0.146)
village density	−0.712	0.151	0.064	0.117	0.150	0.081	0.675	0.097	0.886*	−0.028
	(0.605)	(0.248)	(0.291)	(0.118)	(0.238)	(0.112)	(0.441)	(0.210)	(0.508)	(0.267)
parents' education		−0.039	−0.255	−0.052		−0.182		−0.714		0.374*
		(0.306)	(0.353)	(0.149)		(0.147)		(0.464)		(0.218)
father absent	−0.847	−0.225	0.216	−0.275	0.276	−0.451**	−0.130	0.341	0.137	−0.559
	(1.172)	(0.422)	(0.435)	(0.201)	(0.384)	(0.197)	(0.858)	(0.310)	(1.204)	(0.469)
mother works	0.192	0.575	−0.499	0.330	−0.724	0.436*	0.872	0.105		0.297
	(1.062)	(0.405)	(0.681)	(0.235)	(0.546)	(0.237)	(0.557)	(0.357)		(0.459)
parents work	0.530	−0.105	−0.180	−0.465+	0.087	−0.498+	1.031*	−0.018	0.096	−0.577*
	(0.613)	(0.328)	(0.291)	(0.143)	(0.282)	(0.136)	(0.605)	(0.278)	(0.576)	(0.340)
people in family	−0.005	−0.006	−0.063**	−0.077+	−0.058**	−0.096+	−0.149**	−0.047*	−0.226+	−0.115+
	(0.059)	(0.026)	(0.031)	(0.014)	(0.027)	(0.013)	(0.060)	(0.025)	(0.063)	(0.032)
cons	−4.444+	−4.564+	−1.494+	−1.446+	−1.848+	−1.524+	−2.902+	−3.733+	−3.041+	−3.037+
	(1.084)	(0.466)	(0.478)	(0.199)	(0.447)	(0.209)	(0.883)	(0.442)	(0.901)	(0.482)
year effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
state effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
age groups	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
chi2	153	177	54	213	49	209	92	118	59	52
Log-likelihood	−180.758	−1150.361	−545.858	−4382.865	−719.404	−5188.538	−291.499	−1869.964	−170.733	−1078.718
N	1728.000	15884.000	1870.000	16937.000	1940.000	17269.000	1765.000	16077.000	1681.000	15855.000

* $p < 0.10$, ** $p < 0.05$, + $p < 0.01$. Standard deviation in parenthesis.

Table A.4: Children Health Conditions

	Logit		ZINB	
	Sickness Incidence		Days Sick	
	Non-poor	Poor	Non-poor	Poor
treatment	0.307	−0.107	0.273	−0.121
	(0.221)	(0.102)	(0.487)	(0.250)
% poor neighbors	−0.448	0.224	0.076	0.249
	(0.472)	(0.206)	(1.420)	(0.584)
# treated peers	0.013	0.008	−0.026	−0.001
	(0.017)	(0.006)	(0.049)	(0.021)
village density	0.503	−0.158	1.015	−0.523
	(0.372)	(0.167)	(1.300)	(0.404)
male	−0.135	−0.082	−0.351	0.316*
	(0.168)	(0.062)	(0.296)	(0.178)
parents' education	−0.067	−0.464		
	(0.391)	(0.329)		
father absent	1.114**	0.217		
	(0.462)	(0.197)		
mother works	−0.619	0.571**		
	(0.995)	(0.262)		
parents work	−0.391	−0.547+		
	(0.283)	(0.138)		
people in family	0.041	0.108+		
	(0.035)	(0.019)		
cons	−4.266+	−4.683+	0.646*	1.331+
	(0.557)	(0.270)	(0.337)	(0.423)
inflate				
treatment			−0.520	−0.086
			(0.419)	(0.158)
% poor neighbors			0.155	0.143
			(0.515)	(0.203)
# treated peers			0.037	0.009
			(0.055)	(0.013)
cons			4.517+	4.024+
			(0.426)	(0.270)
lnalpha				
cons			−1.426+	1.245+
			(0.540)	(0.364)
age group	*Yes*	*Yes*	*Yes*	*Yes*
year effects	*Yes*	*Yes*	*Yes*	*Yes*
state effects	*Yes*	*Yes*	*Yes*	*Yes*
LR chi2	117	232	7873	36
Log-likelihood	−604.677	−4242.305	−299.616	−2288.673
N	3331.000	31993.000	3358.000	32520.000

* $p < 0.10$, ** $p < 0.05$, + $p < 0.01$. Standard deviation in parenthesis.

Table A.5: Young Individuals (13-18 years) Health Conditions

	Logit		ZINB	
	Sickness Incidence		Days Sick	
	Non-poor	Poor	Non-poor	Poor
treatment	−0.166	0.026	3.982+	0.377
	(0.162)	(0.081)	(0.989)	(0.248)
% poor neighbors	−0.049	−0.217	−5.144+	1.598**
	(0.368)	(0.142)	(1.783)	(0.656)
# treated peers	−0.033*	−0.011**	−0.094	−0.032
	(0.018)	(0.005)	(0.060)	(0.022)
village density	0.172	0.216**	1.674	−1.237**
	(0.251)	(0.102)	(1.030)	(0.503)
male	−0.208*	−0.432+	−0.406	0.144
	(0.119)	(0.053)	(0.626)	(0.229)
parents' education	0.276	−0.455+		
	(0.182)	(0.156)		
father absent	−0.006	−0.119		
	(0.263)	(0.145)		
mother works	0.544	0.239		
	(0.367)	(0.168)		
parents work	−0.298	−0.272+		
	(0.182)	(0.094)		
people in family	−0.003	0.032+		
	(0.023)	(0.011)		
cons	−2.898+	−2.990+	−3.661+	2.295+
	(0.346)	(0.169)	(0.747)	(0.347)
inflate				
treatment			1.883	−0.189
			(1.271)	(0.207)
% poor neighbors			−4.274**	0.304
			(2.113)	(0.253)
# treated peers			−0.046	0.010
			(0.047)	(0.017)
male			−1.110	0.213
			(1.113)	(0.181)
cons			−0.638	4.617+
			(1.602)	(0.231)
lnalpha				
cons			4.650+	0.296
			(0.608)	(0.209)
age group	*Yes*	*Yes*	*Yes*	*Yes*
year effects	*Yes*	*Yes*	*Yes*	*Yes*
state effects	*Yes*	*Yes*	*Yes*	*Yes*
LR chi2	275	1006	1410	55
Log-likelihood	−1057.849	−5830.033	−207.039	−1217.823
N	4293.000	23612.000	4122.000	22444.000

* $p < 0.10$, ** $p < 0.05$, + $p < 0.01$. Standard deviation in parenthesis.

Table A.6: Adult Individuals Health Conditions I

	Logit		ZINB		Logit		Logit	
	Normal Activities		Days sick		Difficult Activities		Moderate Activities	
	Non-poor	Poor	Non-poor	Poor	Non-poor	Poor	Non-poor	Poor
treatment	−0.072	0.064	0.125	0.005	−0.115	0.055	−0.028	0.067
	(0.119)	(0.072)	(0.159)	(0.096)	(0.099)	(0.073)	(0.103)	(0.076)
% poor neighbors	−0.069	−0.052	0.298	0.296	−0.393*	0.274**	−0.481*	0.273**
	(0.322)	(0.166)	(0.448)	(0.262)	(0.233)	(0.126)	(0.246)	(0.133)
# treated peers	−0.003*	−0.001*	−0.000	0.001	0.001	0.000	0.001	0.001
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
village density	0.193	−0.029	−0.088	−0.131	0.145	−0.056	0.190	−0.080
	(0.163)	(0.094)	(0.208)	(0.151)	(0.118)	(0.071)	(0.126)	(0.072)
male	1.524+	2.180+	0.566+	0.603+	−0.350+	−0.616+	−0.242**	−0.393+
	(0.159)	(0.103)	(0.134)	(0.087)	(0.127)	(0.097)	(0.121)	(0.098)
mother works	0.322	−0.755+	0.577	0.022	−0.240	−0.130	−0.177	−0.210
	(0.323)	(0.264)	(0.660)	(0.270)	(0.220)	(0.153)	(0.217)	(0.151)
work	−1.875+	−2.594+	−0.968+	−1.058+	0.994+	1.449+	1.027+	1.363+
	(0.165)	(0.109)	(0.183)	(0.122)	(0.132)	(0.099)	(0.131)	(0.098)
father absent	0.495**	0.778+	−0.047	−0.075	−0.237*	−0.342+	−0.138	−0.288+
	(0.217)	(0.110)	(0.302)	(0.144)	(0.135)	(0.085)	(0.138)	(0.097)
people in family	−0.012	0.002	−0.010	−0.018	0.031**	0.015*	0.038+	0.025+
	(0.021)	(0.011)	(0.024)	(0.015)	(0.015)	(0.009)	(0.014)	(0.010)
cons	−2.331+	−2.468+	2.350+	2.457+	−0.169	−0.136	−0.306	−0.281**
	(0.233)	(0.141)	(0.255)	(0.163)	(0.185)	(0.127)	(0.189)	(0.120)
inflate								
treatment			0.258*	0.047				
			(0.149)	(0.084)				
% poor neighbors			−0.132	−0.063				
			(0.247)	(0.133)				
# treated peers			0.001	0.001				
			(0.002)	(0.001)				
male			−0.079	0.069				
			(0.129)	(0.074)				
cons			3.210+	3.449+				
			(0.172)	(0.104)				
lnalpha								
cons			0.041	0.248+				
			(0.137)	(0.092)				
age group	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
year effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
state effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
chi2	325	1289	77719	184	786	2829	791	2188
Log-likelihood	−1911.491	−7167.420	−2092.067	−6812.352	−3291.251	−10404.077	−3261.413	−10282.959
N	8521.000	40590.000	8415.000	40087.000	8414.000	40090.000	8413.000	40079.000

* $p < 0.10$, ** $p < 0.05$, + $p < 0.01$. Standard deviation in parenthesis.

Table A.7: Adult Individuals Health Conditions II

	Logit		Logit		OLS		Logit	
	Simple Activities		Cannot walk 2km.		Km. able to walk		Daily Activities	
	Non-poor	Poor	Non-poor	Poor	Non-poor	Poor	Non-poor	Poor
treatment	−0.249**	−0.020	−0.104	0.071	0.061	0.195**	−0.359*	0.009
	(0.119)	(0.086)	(0.105)	(0.073)	(0.149)	(0.093)	(0.214)	(0.136)
% poor neighbors	−0.295	0.464+	−0.284	0.380+	0.267	0.240*	−0.140	−0.316
	(0.271)	(0.161)	(0.240)	(0.139)	(0.270)	(0.145)	(0.497)	(0.286)
# treated peers	0.002	0.000	0.002	0.000	−0.001	−0.001	0.004	−0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
village density	0.086	−0.119	0.110	−0.128	−0.115	0.012	−0.055	0.285*
	(0.133)	(0.087)	(0.127)	(0.078)	(0.166)	(0.076)	(0.247)	(0.160)
male	−0.601+	−0.772+	−0.295**	−0.475+	0.622+	0.679+	−1.237+	−1.383+
	(0.136)	(0.111)	(0.122)	(0.104)	(0.231)	(0.182)	(0.221)	(0.176)
mother works	0.195	−0.055	−0.296	−0.215	−0.244	−0.125	−0.271	0.189
	(0.279)	(0.202)	(0.219)	(0.181)	(0.315)	(0.161)	(0.504)	(0.359)
work	1.295+	1.663+	1.151+	1.511+	0.719+	0.669+	1.946+	2.237+
	(0.142)	(0.112)	(0.130)	(0.103)	(0.232)	(0.185)	(0.250)	(0.180)
father absent	−0.087	−0.275+	−0.216	−0.342+	−0.192	−0.246*	−0.495*	−0.356**
	(0.157)	(0.099)	(0.132)	(0.095)	(0.215)	(0.146)	(0.286)	(0.175)
people in family	0.054+	0.038+	0.034**	0.029+	−0.010	0.003	0.081**	0.045**
	(0.017)	(0.012)	(0.016)	(0.009)	(0.024)	(0.010)	(0.033)	(0.019)
cons	0.144	0.166	−0.135	−0.185	3.302+	3.955+	2.113+	2.332+
	(0.204)	(0.139)	(0.183)	(0.117)	(0.285)	(0.151)	(0.353)	(0.221)
age group	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
year effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
state effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
chi2	644	2113	725	2652			240	721
Log-likelihood	−2511.548	−7009.317	−2917.700	−8691.781	−23711.466	−110282.814	−865.376	−2588.830
N	8406.000	40076.000	8391.000	40003.000	8419.000	40108.000	8227.000	40027.000

* $p < 0.10$, ** $p < 0.05$, + $p < 0.01$. Standard deviation in parenthesis.

Table A.8: Old Individuals Health Conditions I

	Logit		ZINB		Logit		Logit	
	Normal Activities		Days sick		Difficult Activities		Moderate Activities	
	Non-poor	Poor	Non-poor	Poor	Non-poor	Poor	Non-poor	Poor
treatment	0.068	−0.050	−0.286	−0.089	−0.260	0.198*	−0.307*	0.221**
	(0.180)	(0.111)	(0.202)	(0.105)	(0.167)	(0.107)	(0.168)	(0.105)
% poor neighbors	0.064	−0.418*	−0.711	0.062	0.879**	0.204	0.598*	0.076
	(0.480)	(0.233)	(0.464)	(0.228)	(0.356)	(0.251)	(0.348)	(0.239)
# treated peers	−0.015	0.002	0.017	−0.002	0.022	−0.034+	0.030**	−0.031+
	(0.018)	(0.010)	(0.014)	(0.010)	(0.014)	(0.010)	(0.014)	(0.009)
village density	0.309	0.424**	0.317	−0.087	−0.637**	−0.241	−0.511**	−0.155
	(0.295)	(0.175)	(0.340)	(0.177)	(0.255)	(0.182)	(0.246)	(0.181)
male	0.440**	0.444+	0.525+	0.024	−0.301*	−0.468+	−0.169	−0.451+
	(0.189)	(0.105)	(0.179)	(0.096)	(0.165)	(0.125)	(0.161)	(0.129)
mother works	−0.416	0.596	1.584+	0.048	0.296	−0.560**	0.548	−0.724+
	(0.976)	(0.369)	(0.390)	(0.510)	(0.501)	(0.251)	(0.572)	(0.261)
work	−1.218+	−1.338+	−0.644**	−0.236	0.825+	1.189+	0.820+	1.293+
	(0.205)	(0.120)	(0.263)	(0.151)	(0.181)	(0.137)	(0.181)	(0.141)
father absent	0.666+	−0.246*	0.040	−0.060	−0.662+	−0.130	−0.741+	−0.061
	(0.239)	(0.147)	(0.261)	(0.135)	(0.244)	(0.125)	(0.245)	(0.122)
people in family	−0.013	0.001	0.012	0.025**	0.041	0.008	0.069**	0.005
	(0.032)	(0.017)	(0.041)	(0.012)	(0.029)	(0.013)	(0.029)	(0.013)
cons	−0.425	−0.289	3.026+	2.971+	−2.030	−2.809+	−2.223	−2.851+
	(0.982)	(0.300)	(0.485)	(0.218)	(1.412)	(0.537)	(1.392)	(0.523)
inflate								
treatment			0.126	0.069				
			(0.226)	(0.138)				
% poor neighbors			−0.575	−0.268				
			(0.625)	(0.334)				
# treated peers			0.028	−0.002				
			(0.029)	(0.013)				
cons			0.016	0.905+				
			(1.127)	(0.350)				
lnalpha								
cons			−0.409**	−0.267+				
			(0.177)	(0.098)				
age group	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
year effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
state effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
chi2	90	257	135	24	85	398	104	390
Log-likelihood	−585.341	−1675.259	−859.241	−2480.154	−785.279	−2016.631	−779.506	−2026.475
N	1335.000	3612.000	1302.000	3491.000	1305.000	3486.000	1304.000	3485.000

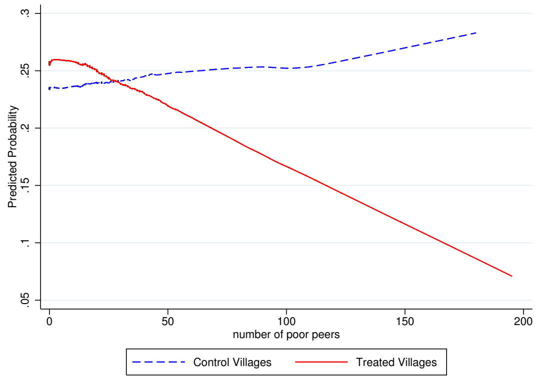
* $p < 0.10$, ** $p < 0.05$, + $p < 0.01$. Standard deviation in parenthesis.

Table A.9: Old Individuals Health Conditions II

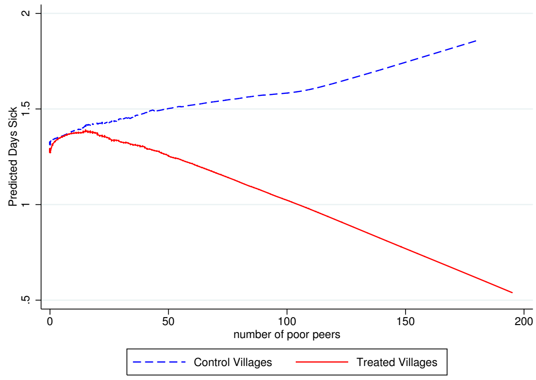
	Logit		Logit		OLS		Logit	
	Simple Activities		Cannot walk 2km.		Km. able to walk		Daily Activities	
	Non-poor	Poor	Non-poor	Poor	Non-poor	Poor	Non-poor	Poor
treatment	−0.284*	0.108	−0.349**	0.154	−0.014	0.130	−0.052	0.066
	(0.160)	(0.098)	(0.171)	(0.105)	(0.335)	(0.397)	(0.207)	(0.136)
% poor neighbors	0.442	−0.113	0.339	0.083	0.223	−1.501**	0.779	0.353
	(0.355)	(0.229)	(0.365)	(0.238)	(1.287)	(0.672)	(0.533)	(0.298)
# treated peers	0.020	−0.025+	0.018	−0.025**	0.055	0.011	0.015	−0.012
	(0.013)	(0.009)	(0.012)	(0.010)	(0.066)	(0.030)	(0.016)	(0.013)
village density	−0.487*	0.003	−0.367	−0.133	0.110	1.041**	−0.954+	−0.164
	(0.254)	(0.171)	(0.256)	(0.177)	(0.941)	(0.505)	(0.356)	(0.203)
male	−0.323**	−0.371+	−0.170	−0.343+	−0.445	0.143	−0.375**	−0.325+
	(0.160)	(0.103)	(0.169)	(0.119)	(0.676)	(0.269)	(0.186)	(0.117)
mother works	0.148	−0.208	−0.085	−0.461*	0.197	0.501	0.812	−0.546
	(0.465)	(0.240)	(0.518)	(0.268)	(0.564)	(1.457)	(0.912)	(0.345)
work	1.032+	1.156+	0.982+	1.296+	1.030**	1.376+	1.361+	1.309+
	(0.181)	(0.119)	(0.182)	(0.135)	(0.476)	(0.274)	(0.245)	(0.148)
father absent	−0.361	−0.006	−0.851+	−0.109	−1.508**	0.741*	−0.140	0.004
	(0.225)	(0.117)	(0.259)	(0.129)	(0.675)	(0.385)	(0.260)	(0.163)
people in family	0.061**	0.029**	0.037	0.004	−0.069	−0.041	0.034	0.001
	(0.028)	(0.013)	(0.025)	(0.013)	(0.052)	(0.060)	(0.037)	(0.018)
cons	−2.190*	−1.979+	−2.219	−2.538+	0.831	0.975	−0.026	−0.633*
	(1.237)	(0.453)	(1.408)	(0.503)	(0.908)	(0.705)	(0.957)	(0.354)
age group	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
year effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
state effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
chi2	109	332	135	484			152	361
Log-likelihood	−814.657	−2165.027	−789.917	−2044.566	−4256.956	−11503.212	−507.371	−1479.241
N	1301.000	3479.000	1301.000	3469.000	1305.000	3496.000	1299.000	3477.000

* $p < 0.10$, ** $p < 0.05$, + $p < 0.01$. Standard deviation in parenthesis.

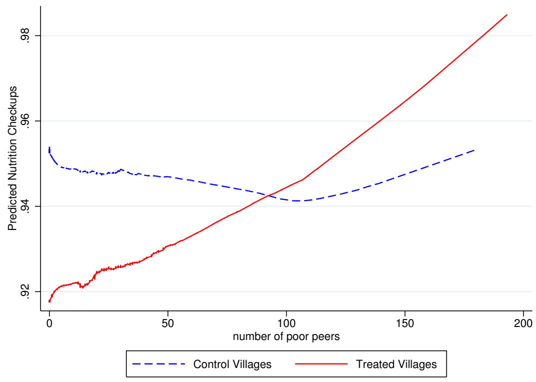
A.4 Spillover Effect: Graphical Results



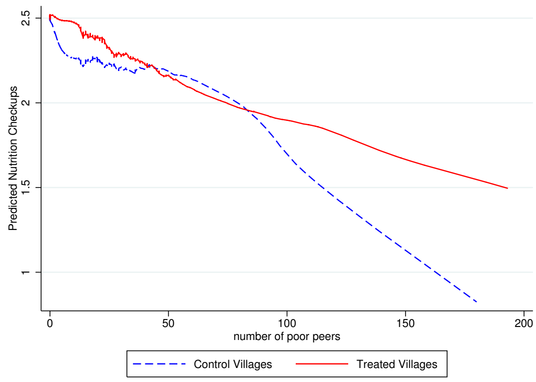
(a) Sickness Incidence



(b) Sickness Spell

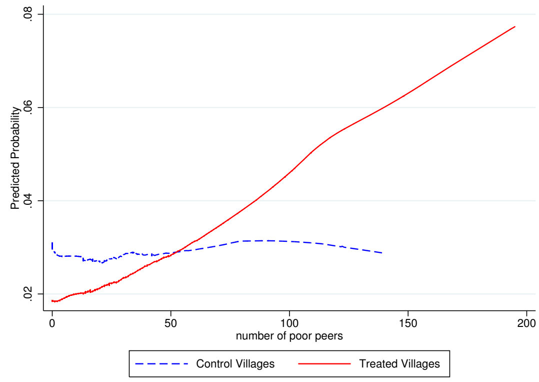


(c) Nutrition Monitoring

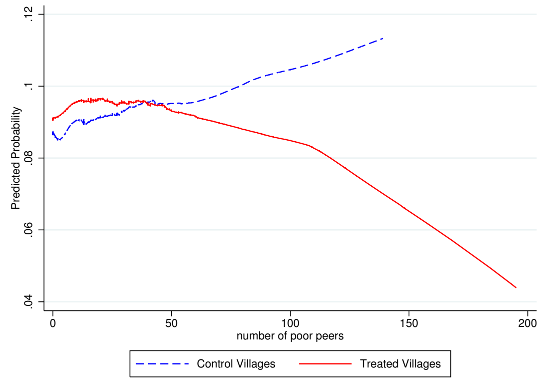


(d) Frequency of Monitoring

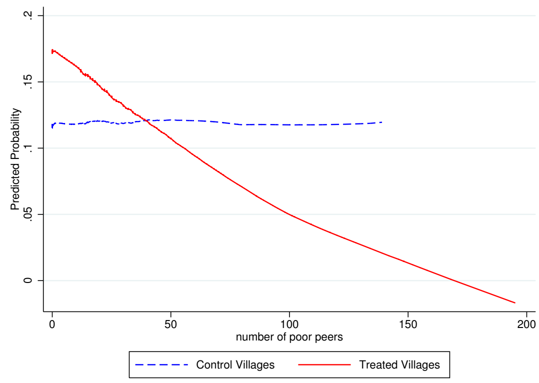
Figure A.3: Spillover Effect: Babies



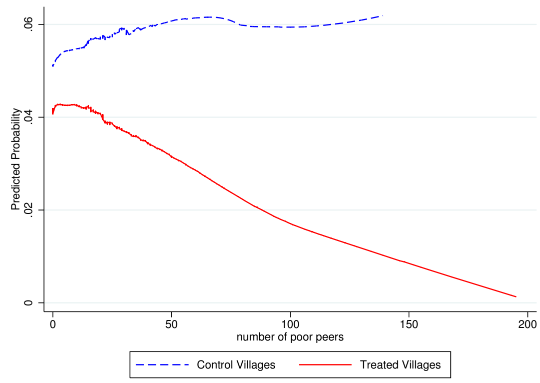
(a) Diarrhea



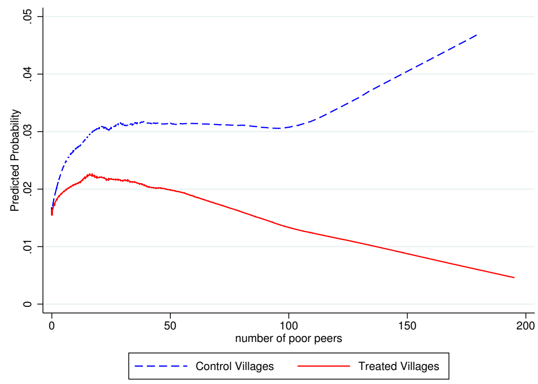
(b) Fever



(c) Cough

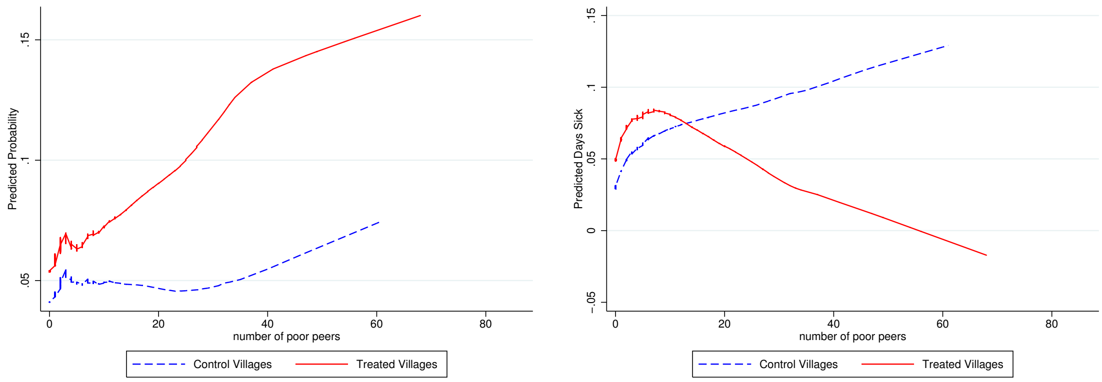


(d) Respiratory problems



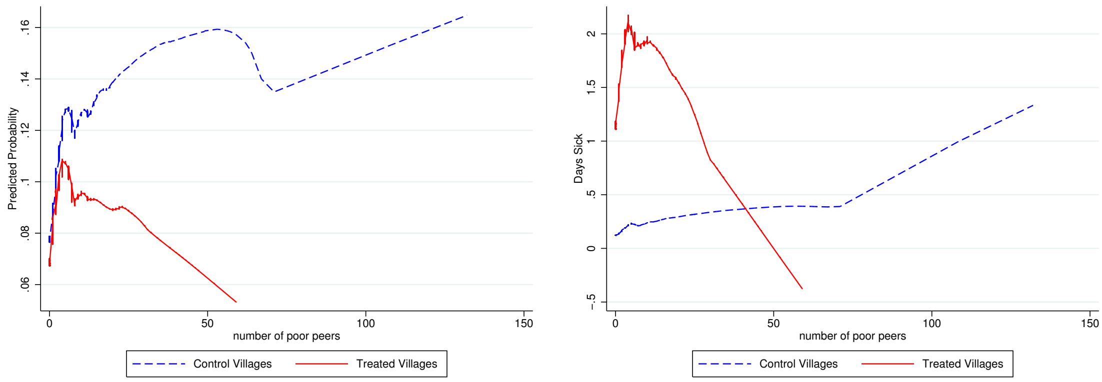
(e) Other

Figure A.4: Spillover Effect: Babies Sickness Incidence



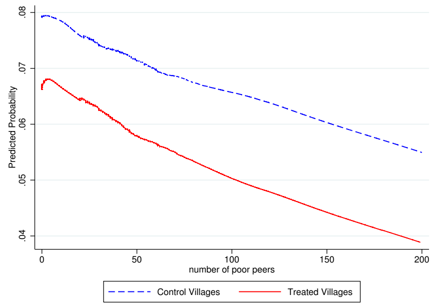
(a) Sickness Incidence (b) Sickness Spell

Figure A.5: Spillover Effect: Children

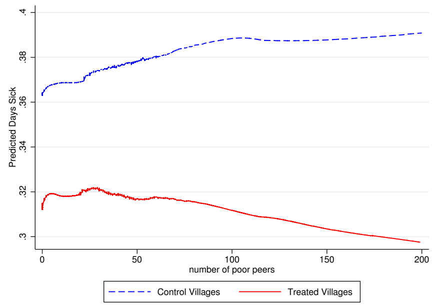


(a) Sickness Incidence (b) Sickness Spell

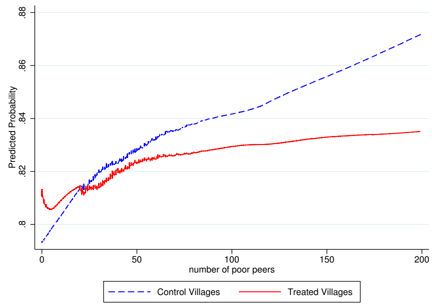
Figure A.6: Spillover Effect: Young Individuals



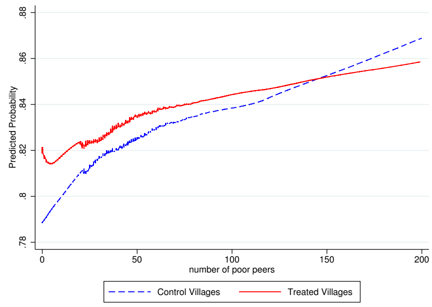
(a) Problems with Normal Activities



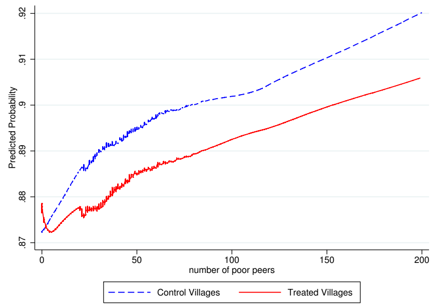
(b) Sickness Spell



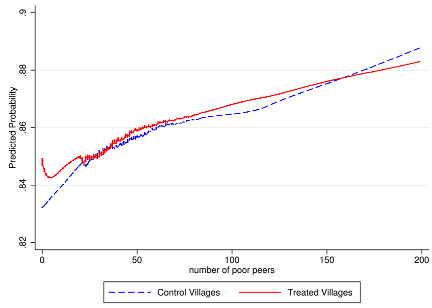
(c) Problems with Difficult Activities



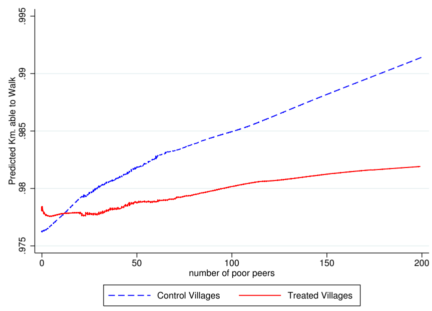
(d) Problems with Moderate Activities



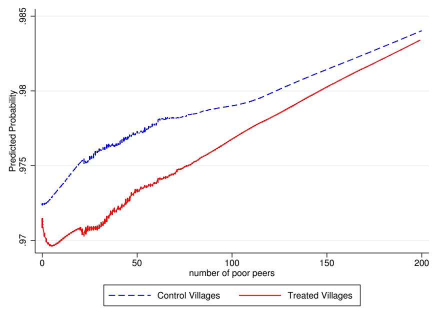
(e) Problems with Simple Activities



(f) Problems Walking

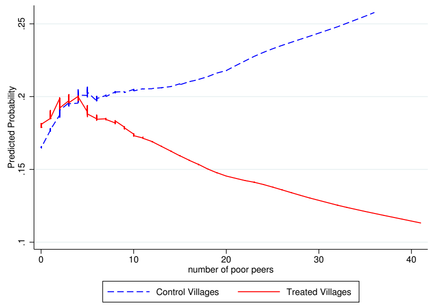


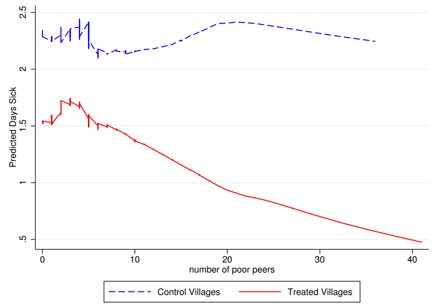
(g) Km. able to walk



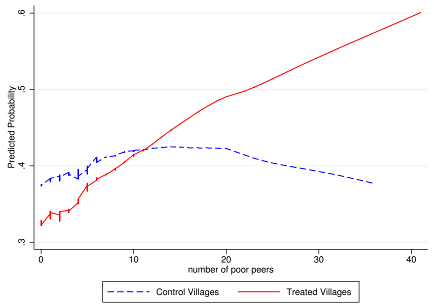
(h) Problems with Daily Activities

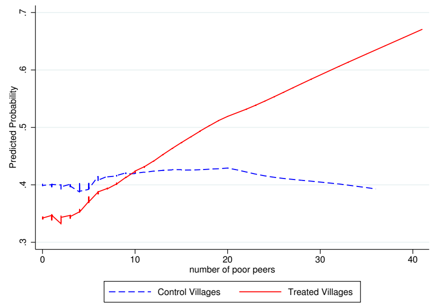
Figure A.7: Spillover Effect: Adult Individuals



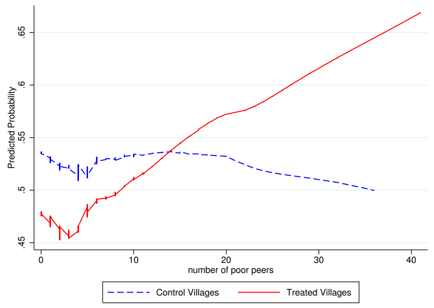


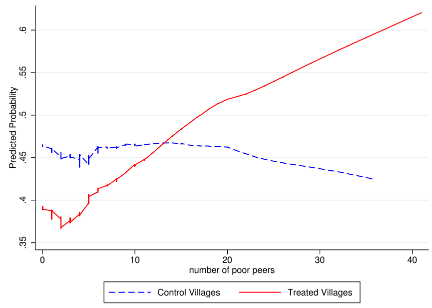
(a) Problems with Normal Activities (b) Sickness Spell



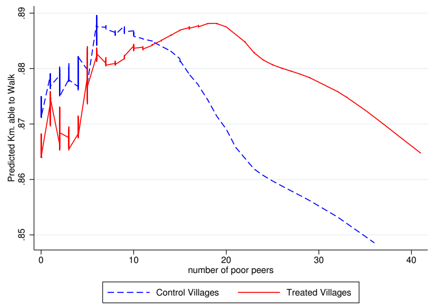


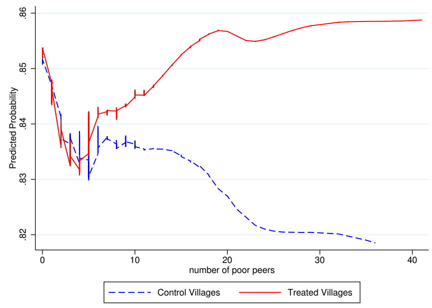
(c) Problems with Difficult Activities (d) Problems with Moderate Activities





(e) Problems with Simple Activities (f) Problems Walking





(g) Km. able to walk (h) Problems with Daily Activities

Figure A.8: Spillover Effect: Elderly Individuals

Paper 2

The Extent of the Impact: Identifying Externalities*

Conditional Cash Transfers (CCT) programs have been implemented around the world 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 children and teenagers. 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.

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2.1 Introduction

There is a growing interest in the net impact of 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 & Sadoulet, 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 in-home activities [Galiani & Mc.Ewan, 2011].

[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].

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 & Cattaneo, 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], for example, concluded that the transfers increased the participation of the head of the household in the labor market; however, the results might vary depending on the geographic location of the families. In contrast, Alzúa *et al.* [2012] in a study that included programs in Mexico, Nicaragua and Honduras found no significant results in the working hours of adults; however, the authors found some evidence on wages and labor income in the short and medium term suggesting an impact on the labor market equilibrium.

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

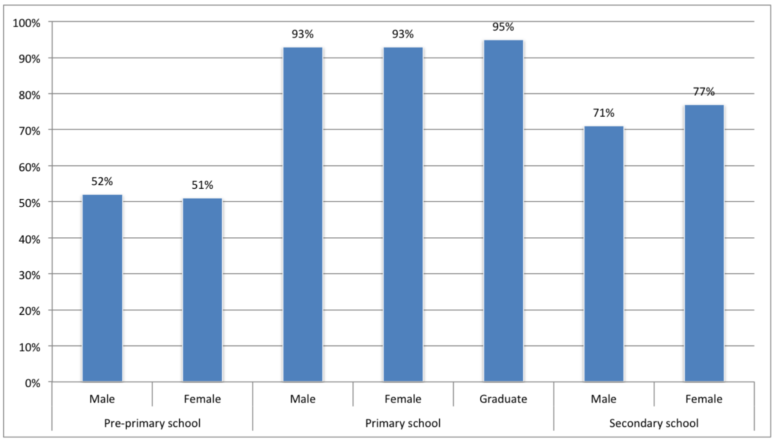


Figure 2.1: Schooling Conditions in Colombia
Source: UNICEF, Colombia (Data 2007-2010). Web page: Basic Indicators Statistics.
Notes: The table reports the school enrollment by schooling level.

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 Acción," 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.

2.2 Background Information

2.2.1 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. 2.1).

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.[1] 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 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.

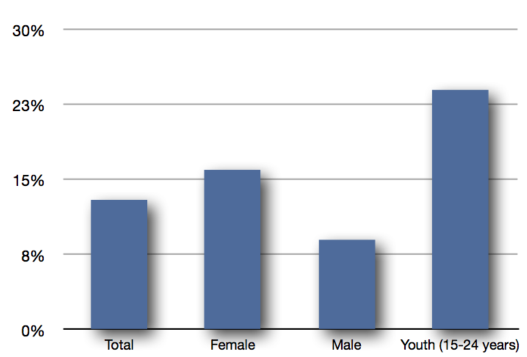
3. The quality of education is low and there is a general low perception of the relevance of what is taught.

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

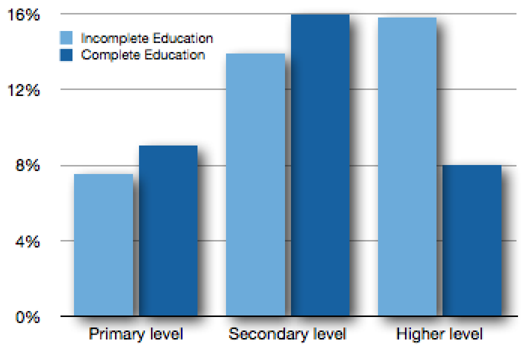
[1] Colombia is divided into 32 departments and one unique district: Bogotá. The departments are autonomous and have their own political and administrative organization. Each department is divided into municipalities; in total, the country has 1120 municipalities. For more detailed information, see Banco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

2.2.2 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. 2.2a).



(a) Unemployment by gender



(b) Unemployment by schooling level

Figure 2.2: Labor Conditions in Colombia
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. Figure 2.2b 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.[1] 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.

[1]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].

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].

2.3 Familias en Acción

"Familias en Acción" is a CCT program 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.[1] 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.[2] 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

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

[2] 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.

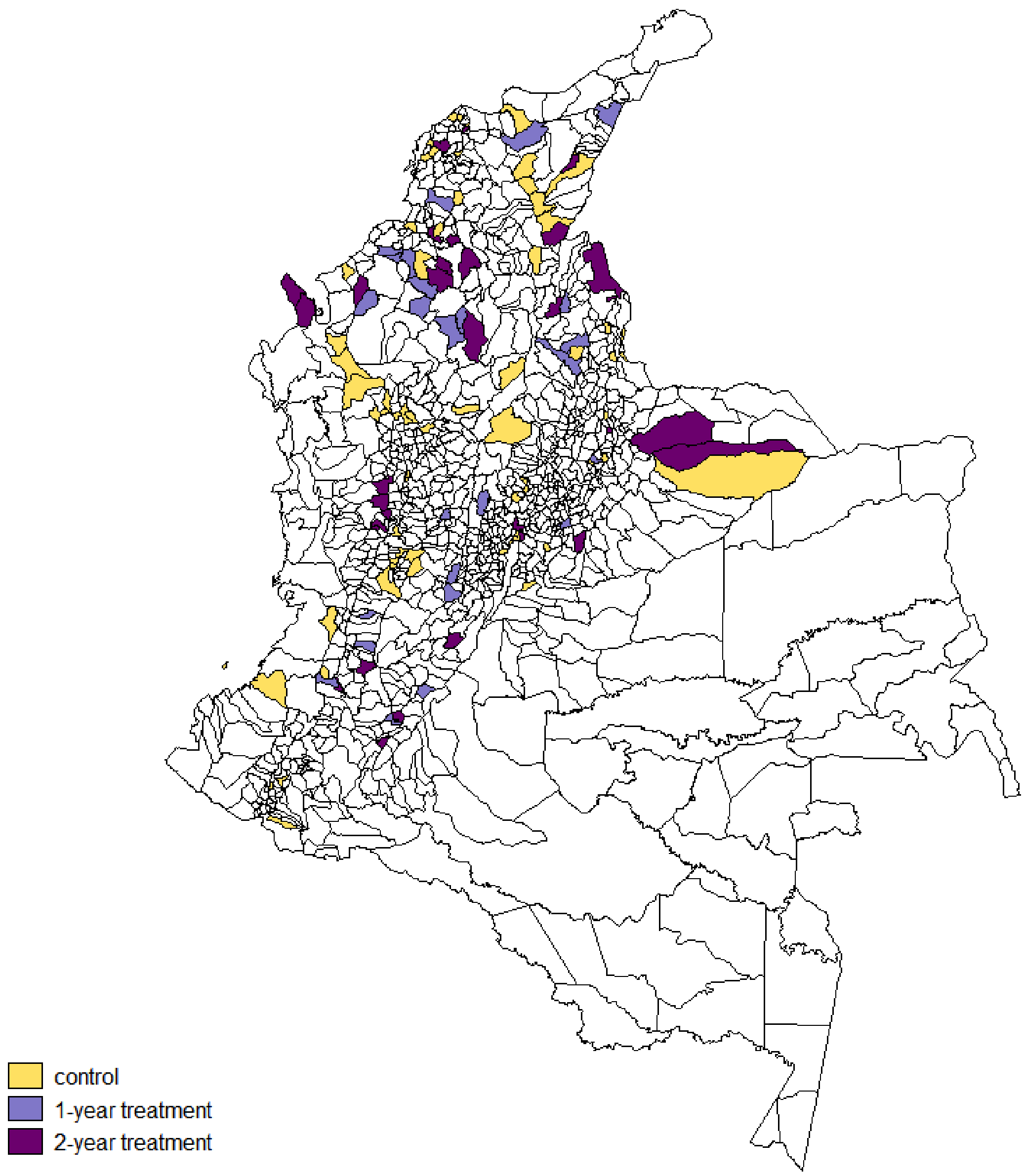


Figure 2.3: Coverage of Familias en Acción
Source: Departamento Nacional de Planeamiento, Colombia.
Note: Familias en acción was implemented at the village level. The map shows the geographic localization of the control and treated villages. Within treated villages, it differentiates between the villages that received the treatment first (purple), and the villages that received the treatment later (light purple).

administrative authorities were not interested.

The program is widespread in the country, and it covers about 99.54% of the Colombian territory. Figure 2.3 shows the geographic location of the villages participating in 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).[1]

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.

[1]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.

2.4 The Identification Framework

2.4.1 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 2.1 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 2.1: 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. For this reason, this paper will use the data set as a cross section, and 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.

2.4.2 Descriptive Statistics

Table 2.2 details the background characteristics of the sample before the implementation of the program. The data is divided into the villages that received the treatment for 1 year, the villages that received the treatment for two years, and the control villages.

Table 2.2: Descriptive Statistics: Baseline Information

	(A) 1-Year Treatment	(B) 2-Year Treatment	(C) Control	(A-C)	(B-C)
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.

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.

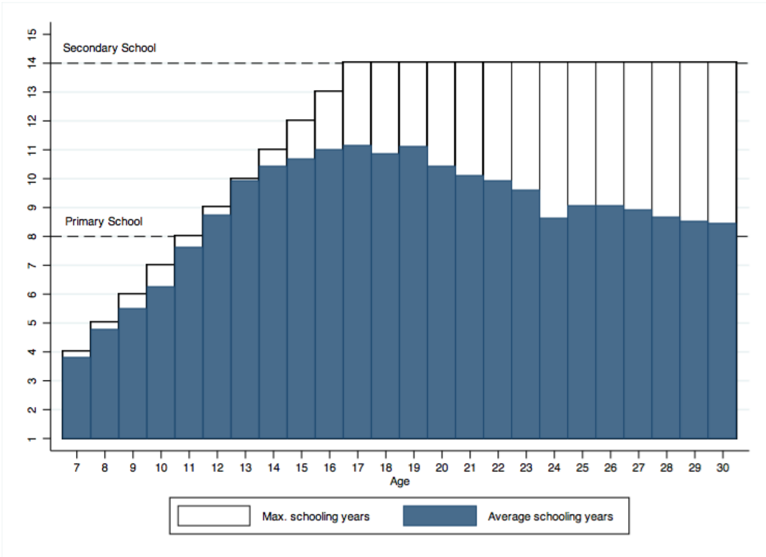


Figure 2.4: School Attainment by Age
Source: Familias en Acción Data.

In terms of education, parents have attended no more than primary school, and almost 9% of the sample has never attended school. Figure 2.4 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 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.[1]

[1]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

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 modelling, we implemented a propensity score matching PSM. This technique uses a vector of observed variables X to predict the probability of experiencing the treatment D_v to create a counterfactual group. Specifically,

$$p(x) = Pr\{D_v = 1|X = x\}$$

where $p(x)$ is estimated using a Logit specification, and the vector X contains all the reported variables in Table 2.2. 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 procedure pairs the observations in the control group to the treated observations. Selecting a matching method will dependent on the degree of overlap between treatment and control groups. When the distributions of the $p(x)$ are similar in the treated and control group, most of the matching algorithms will yield similar results. In the case of our sample, there are some difference between groups, but, in general, the groups have distributions very similar.[1]

Therefore, after evaluating several matching algorithms, we chose a one-to-one procedure with no replacement for two reasons. First, the one-to-one or single comparison unit ensures the smallest distance between the treatment and comparison units [Imbens & Wooldridge, 2009]. And second, the "no replacement" option, where one unit of the control group can be matched only once with a unit of the treatment group, improves

in the labor market. The program is 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].

[1]For a more detailed analysis of propensity score-matching methods, see Dehejia & Wahba [2002].

the precision of the estimates; this option, however, may force the algorithm to match units that are not quite comparable, which can increase the bias. Nevertheless, given the large number of observations of the sample, when comparing the "replacement" with the "no replacement" option, there are no important differences. In fact, the combination of these two options showed no off-support observations: all treated households found a comparable observation in the control group.

Because we have two treatment groups, we estimate one $p(x)$ for each group. The matching was done at the household level, so the PSM matched households in the 1-Year treatment group with control households and households in the 2-Year treatment group with control households. After the matching, it is possible to check the balance of the sample: given the observables included in X, the control and treatment groups are statistically comparable. It is not always the case that the algorithm finds statistically similar groups; it depends on the variables included in X. Therefore, the calibration might require the implementation of different algorithms. Figure B.1 graphically shows the results reported by the p-score matching procedure (see Appendix B).

Finally, Table 2.3 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.

2.5 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.

Given the available information, this paper will run a cross-section analysis comparing the outcomes between the matched treated and control groups. We do not implement a difference-in-difference estimator because that would imply using a reduced number of villages since just few of them have a baseline evaluation. Nevertheless, some studies that have implemented a difference-in-difference strategy focused on this reduced group, or, in the case of Attanasio *et al.* [2008], the authors constructed a pre-program information using several national surveys in Colombia. There are very few studies using a regression discontinuity design [Independent Evaluation Group, 2011]. In any case, the results next presented are consistent with those of related studies.

Table 2.3: Descriptive Statistics: After Matching

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

2.5.1 Direct Impact

The CCT program was designed to increase the school attendance (S), and, at the same time, reduce the labor participation (L) of individuals in primary (children) and secondary school (teenagers). Each group in analysis, 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:

$$Y_{iv}^{A} = \alpha + \gamma D_v + \sum_h \delta_h X_{ih} + \epsilon_{iv} \tag{2.1}$$

where $Y_{iv}^{A} = \{S, L\}$ stands for the schooling or labor outcome of individual i, living in village v. Both outcomes S and L are binary variables that are equal to 1 if individual i reported to be working or studying, and 0 otherwise. For simplicity of exposition, Eq. (2.1) is presented in linear form; however, the estimation was done using a Logit specification.

The super index A in Eq. (2.1) represents the age group in analysis: children or teenagers. D_v is the treatment indicator of the village v, which is invariant over time. 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 the household, the number of workers and infants in the family, and the productive activity of the head of household, and dummies controlling for the level of schooling and the year of the surveys. Finally, the error term clusters the observations per village to allow some correlation among individuals within villages.

The average treatment effect (ATE) is determined by[1]:

$$E[Y_{iv}^A|D_{vt}=1] - E[Y_{iv}^A|D_v=0] = \gamma + \underbrace{E[\epsilon_{iv}|D_v=1] - E[\epsilon_{iv}|D_v=0]}_{=0} \tag{2.2}$$

It is possible to eliminate the second term in the equation because the matching procedure 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 B.1 details the results of Eq. (2.1), 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.[2] Specifically, for the 1-Year treatment group, the treatment increased the school attendance in 3.7% (column 1). For the 2-Year treatment group, the school attendance increased in 4.1% (column 2). 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 compared to females. In the same way, teenagers show a greater impact 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

[1]The ATE is presented for a linear regression; however, in nonlinear specifications it is still valid. The coefficients do not give the size of the effect, but they do have the right sign. To evaluate the magnitude, refer to the marginal effects.

[2] Table B.1 reports the marginal effects of the variable of interest at the bottom of the table.

before the program started. These results are consisted with the literature. [Attanasio *et al.*, 2008], for example, found an increase in the school participation of about 3% for children, and 7% for teenagers.

Among the covariates, the number of siblings controls for a potential negative impact more children in the family have on the schooling decisions of their siblings. The numbers show that an additional school-age sibling has a negative effect on the attendance; however, this result is not significant. Nonetheless, if we see the effect of the family size (number of members), the results are strongly negative for female teenagers. It seems that big families require women to do housework for the rest of the family, which require them to stay at home and reduce their time for school. Finally, parents' education levels, as expected, have an important positive effect on the school attendance of their children.

In the same way, Table B.2 illustrates the results with respect to labor supply. It was not possible to divide the sample into children and teenagers because in the surveys this question was asked to individuals older 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, they are not reducing their working hours. Related literature found similar results; however, [Attanasio *et al.*, 2008] found that when desegregating the results by urban and rural areas, there is evidence of a decrease in the labor participation of children in big cities. This result should be taken carefully because the program was originally designed to work in very small villages. The program in a later stage included some big urban cities. Therefore, the results for urban regions are not really comparable.

This result rises the question about the "real" effectiveness of the program. If children are attending school, but they continue working, what is the result of a higher attendance? If children, besides their school work, have to devote part of their time to other activities, their performance at school will be very poor, which, on time, will have no effect on their long term situation.

Within the covariates, men have higher probabilities to work. Nevertheless, it is important to highlight the fact that housework, or activities that women do are not considered as a “productive activities”. So, in spite of having many responsibilities, women report to not work at all. In addition, more educated parents significantly reduce the probability of their children to work. Finally, if the family's head work (father or mother), have an important positive effect on their children labor decisions. In fact, if the father works, most of their male children will work too.

Fig. B.2 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 curves are built using a non-parametric estimation using the predicted results of the Eq. (2.1). The graphs are estimated at different treatment timings for male and female groups. The solid lines represent female (red) and male (blue) treated groups. The control groups are represented by the dashed lines.

The figures summarize the main results of Table B.2. 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 the female group. Additionally, the impact seems to be more important as individuals get older: teenagers increased their school attendance in a greater extent compared to children.

The figures reporting the estimated behavior of individuals with respect to labor decisions reveals interesting differences especially when the beneficiary 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, people who have received the treatment for two years show a reduction in the labor supply compared to the control group. However, these 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, this is not the case in the sample. What seems to occur is that the program encourages children and teenagers to go more often to school, but they still work as much as children in the control group. In spite of this result, the non-parametric regression suggest that, over time, the results might change and become significant.

2.5.2 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. However, it is also possible that the program encouraging parents to send their kids to school leaves the non-targeted members of the family with more available time which can be spent on other productive activities. It is important to notice that even though the grants represent an important part of the family's income, the amount is still very small, and 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.

Related literature has worked mainly analyzing the impact of the program on the school participation of young adults living in households with eligible individuals [Independent Evaluation Group, 2011]. The results show a positive, but not significant school completion rate for ineligible family's members. In general, the authors suggest that within family externalities could arise through three channels: 1. an income effect, where the family members could benefit from the program due to an increase in their consumption, or investment. 2. a substitution effect, where due to the program's conditions, the relative price of leisure increases. 3. a displacement effect, due to a reallocation of labor to older individuals since children have to attend school.

For this reason, this paper estimates the presence of within-family externalities on the labor decisions of the family's 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 Eq. (2.1):

$$L_{iv} = \rho_0 + \rho_1 D_v + \rho_2 W_{ih} + \rho_3 (W_{ih} * D_v) + \sum_h \delta_h X_{ih} + \mu_{iv} \tag{2.3}$$

where L_{iv} stands for the labor participation of individual i, living in the village v. The specification assumes two potential transmission mechanisms through which the program can impact the untreated relatives: 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. The vector X_{ih} contains background characteristics at the individual and household level, plus controls for each year of evaluation.

"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 friends part of the same household.

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.[1]

$$E[L_{iv}|D_v = 1] - E[L_{iv}|D_v = 0] = \rho_1 + \rho_3 E[W_{ih}|D_v = 1] \tag{2.4}$$

Table B.3 lists the results for the within-family externalities. The table shows the estimates for three different groups depending on the timing to receive the treatment. Therefore, we have the reported results for all individuals between 18 and 30 years old, for the family' head, mothers and fathers, and finally, for all the other family's members older than 17 years old.

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, they are not significant. This result is quite interesting because it shows that receiving the cash grants does not affect the people's incentives with respect to work.

In addition, an extra member constraints the family's budget and pushes the rest of the family's members to search for additional income sources. Nevertheless, when analyzing the impact of an additional school-age individual who is part of the program, the results show a perverse effect on the family's head labor decisions. This results is specially negative for mothers, and it becomes stronger on time: an extra kid reduces her working probabilities in about 2%. This result goes in the same direction for fathers, but at a smaller scale (about 0.2%). In fact, the participation of families in the program implies many conditions, specially for parents. They have to make sure that children go regularly to school, and to visit the doctor for health checkups and nutrition monitoring. This situation might be negatively impacting the labor situation of the parents since they have to spend more time on their children.

This last result should be taken very cautiously. It could be that the time parents, and specially mothers, devote to their children's performance brings greater results than if they were not involved at all. In fact, if what the program pursues is an increase in the human capital of children, the presence of the mother during their children growth could be essential. However, what is the effect on the mothers' situation? If due to the program, they have to stay, more than usual, at home doing housework, or taking care of children, the effect on their personal situation might be negative. In fact, on time, it would be much more difficult for them to find a payed job due to their lack of experience

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

and preparation. If we add the fact that most women had their fist child very young, situation that forced them to quit school at an early stage, the situation for women is not promissory.

Finally, a non-parametric estimation of the results illustrates the behavior of parents regarding labor decisions by the number of eligible family members, and it compares between treated and control households (see Fig. B.3). The results indicate that the labor supply of the head of the household will significantly differ depending on whether he is a male or a female. In fact, having more school-age children does not significantly change the labor decisions of the father. In contrast, if the head of the household is female, her labor decision will be affected by the number of children in the family: having more children push mothers to work more; however, if the children are part of the program, the probability of mothers working is significantly lower compared to those in the control group. This result becomes stronger for families that have participated in the program for a longer period of time.

2.5.3 Cross-Village Externalities

In spite the fact that "Familias en Acción" was not designed as a randomized experiment, the selection and assignment of treated and control villages were done with special care, taking into account many observable characteristics of the villages. As mentioned in a previous sections, the main difference is that control villages do not have a financial institution that could distribute the grants to beneficiaries. Nevertheless, exploiting the villages' localization, it is possible to find some control villages located just next to treated villages. Taking advantage of this geographic condition, it is possible to estimate the potential presence of cross-village externalities in educational outcomes.

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. 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, and the second group, or group G_2, contains all of the control villages that are geographically isolated. The fact that a control village has a treated village as a neighbor is not sufficient condition to assume a potential impact between beneficiaries and non-beneficiaries 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 feasible.

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 the distance (big villages), the lack of transportation, or simply because there is no infrastructure (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.[1] 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. 2.5).

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 significant differences between treated and control groups. Table 2.4 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. The estimation of the cross-village externalities uses information on the children living in control villages, where the treatment $D'_v = 1$ if a child living in a control village has a

[1]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.

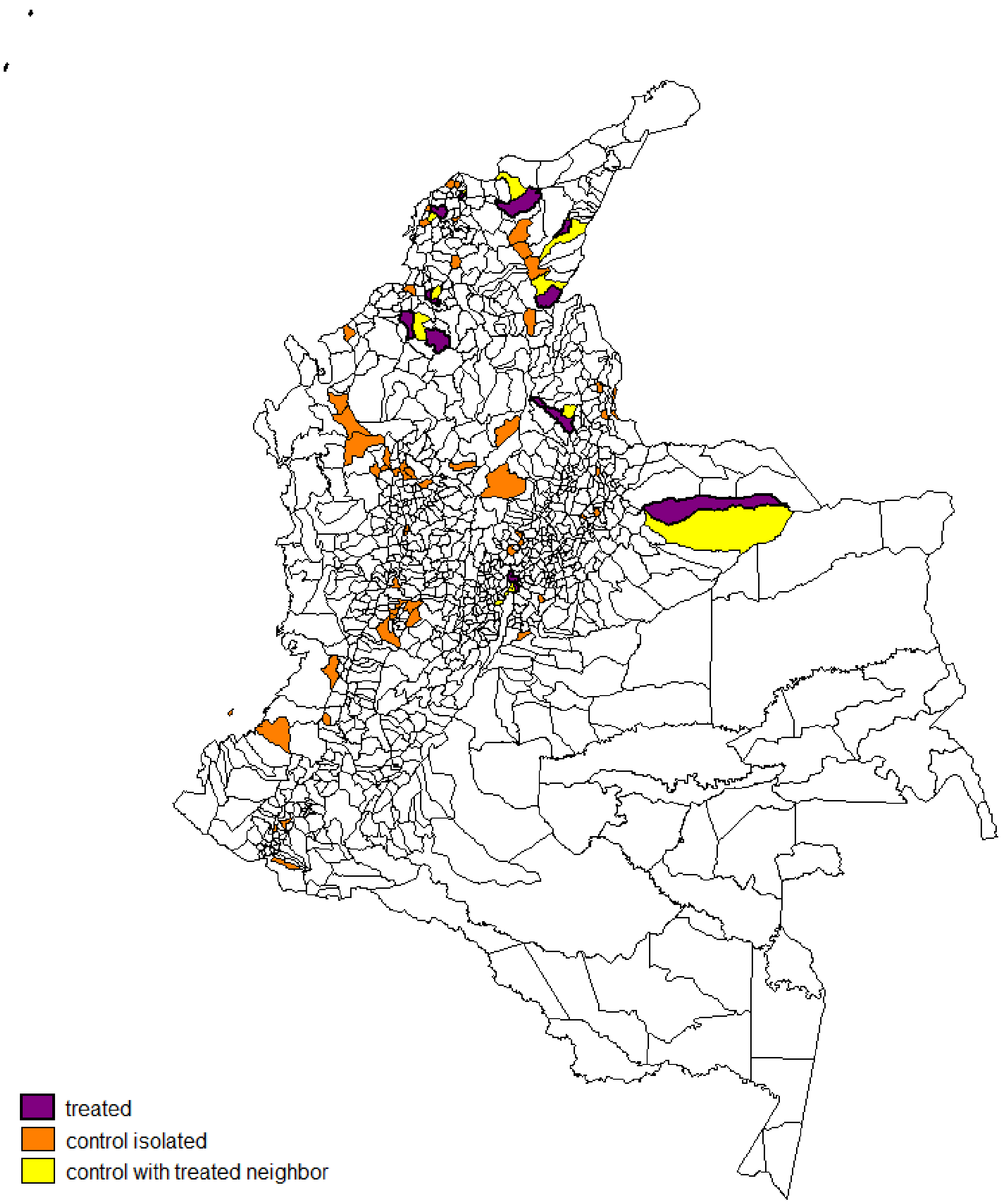


Figure 2.5: Familias en Acción: Cross-Village Externalities
Source: Departamento Nacional de Planeamiento, Colombia.
Notes: The map shows the location of all the control villages in the program, and it highlights the treated villages that had a feasible connection to their neighbor. The control villages that have a treated neighbor become part of the G_1 group; the isolated control villages are part of the G_2 group.

treated neighbor, and $D'_v = 0$ otherwise.

$$S_{iv} = \beta_0 + \beta_1 D'_v + \beta_2 N_{iv} + \beta_3 (N_{iv} * D'_v) + \sum_h \delta_h X_{ih} + \epsilon_{iv} \tag{2.5}$$

Table 2.4: 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.

The variable N_{iv} captures the effect of treated peers on their untreated peers. This variable is computed using two strategies: first, by including only the number of peers living in a treated village, where peers are defined by children of the same age group, who attend the same school year as individual i; and second, by weighting the number of peers with the average distance from the control to the treated village. This last option is implemented to determine whether having closer peers have a stronger impact on their untreated friends. Finally, the equation includes the vector X_{ih} that includes controls for different household and individual characteristics, time dummies for each year of the surveys, controls for the age group and schooling level, plus village fixed effects.

Computing the ATE for schooling decisions:

$$E[S_{iv}|D'_v = 1] - E[S_{iv}|D'_v = 0] = \beta_1 + \beta_3 E[N_{iv}|D'_v = 1]$$

Table B.4 shows the results of the estimation. Each column controls for different variables, and the results for the total sample demonstrate 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, Eq. (2.5) 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:

$$S_{iv} = \phi_0 + \phi_1 \bar{S_v} + \sum_h \delta_h X_{ih} + \tau_t + e_{iv} \tag{2.6}$$

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 B.5 shows the results of Eq. (2.6). 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 continuous. 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.

Figure B.4 summarizes the last results. The graph uses a nonparametric technique to smooth the graph. 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 Acción" (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 with 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.

Nevertheless, the importance of social interactions on school outcomes is a valid channel through which untreated individuals can benefit from the program, we cannot rule out the possibility of an extra channel: an increase in the infrastructure and quality improvement of the educational institutions in treated villages. In fact, since CCT programs imply behavioral conditions, the villages part of the program should have enough infrastructure available to cover the increasing demand. However, in some cases, such infrastructure does not exist, which in turns push organizers to increase their investment in such areas. Therefore, a higher investment might have an effect on the incentives people have in untreated areas to attend more regularly school. In spite of this, given the available information, it is not possible to assess whether this is happening. In fact, the original design of the program implied that, in terms of infrastructure, villages in treated and control villages are the same.

2.6 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 randomization of the program was not possible, when we analyze the within-village and family information the data showed significant differences between treated and control villages. Therefore, it was necessary a previous calibration of the data by matching techniques. The matching was done at the household level using a one-to-one with no replacement specification. For the cross-village analysis, however, the information available for the villages included in the analysis show no important differences in observable outcomes, so it was possible to use the data without a previous matching procedure.

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 a bit greater (4.4%) if the individuals have been receiving the cash grants for longer periods. This result varies in intensity when the analysis is done by different age groups and gender, where teenagers benefit the most (8% to 10%).

In spite of this positive result, when determining the effect on the labor decisions, it seems that individuals continue working as much as they did before: the treatment has had no effect on the probability of an individual to work.

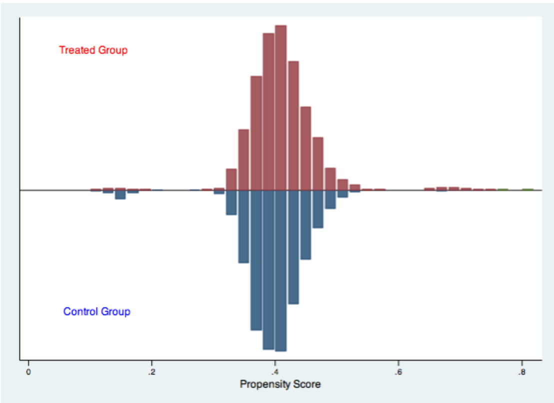
In terms of within-family externalities, we have analyzed the effect of having a treated relative in the same household. We have determined the effect of the program on the labor decision of adults. One of the concerns of any policy intervention intended to reduce poverty is to create the correct incentives. However, it could be the case that the cash grants reduce the individuals' incentives to search for a job, or, due the conditions of the program, older individuals reduce their working participation because they have to check the conditions are fulfilled. The results reveal that the cash grants have no impact on the labor decisions of the adults. Nevertheless, when analyzing the effect of the number of treated individuals in the household, the results show a significant negative effect on the labor participation 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 succeeds 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.

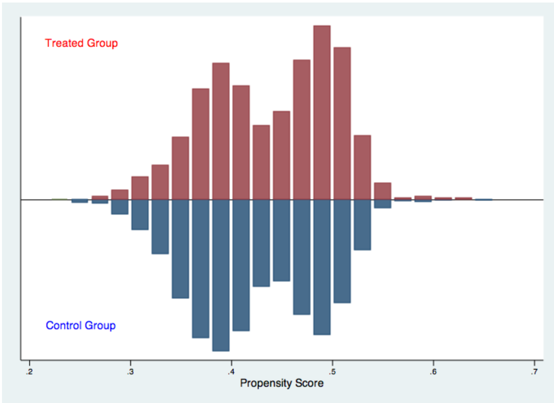
Appendix B

Appendix - Paper 2

B.1 Propensity Score Matching Groups



(a) Matching Data 1-year Treatment



(b) Matching Data 2-year Treatment

Figure B.1: Propensity Score Matching Groups

B.2 Model Estimation Results

Table B.1: Direct Impact of the Program: School Attendance

	Individuals 7-17 years old		Children: 7-12 years old				Young: 13-17 years old			
	1-Y Treatment	2-Y Treatment	Female 1	Female 2	Male 1	Male 2	Female 1	Female 2	Male 1	Male 2
treatment	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)
eligible-peers in 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)
male	-0.404$^+$	-0.468$^+$								
	(0.062)	(0.056)								
parents' schooling	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)
father works	-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)
mother works	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)
disability	-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)
# of family members	-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)
family's head illiterate	-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)
constant	-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)
age controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
schooling level	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
productive activity	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
chi2	2457.559	1865.300	315.819	152.296	285.238	282.608	302.617	239.732	309.916	391.033
N	13381.000	13864.000	3872.000	3937.000	4224.000	4293.000	2436.000	2613.000	2673.000	2879.000
Marginal Effects:										
treatment (γ)	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)

Dependent Var.= 1 if a person is attending school; 0, otherwise.
Significance levels: *: p<0.010 ; **: p<0.005 ; $^+$: p<0.001. Standard errors in parenthesis.

Table B.2: Direct Impact of the Program: Labor Supply

	Individuals 10-17 years old					
	1-Y Treatment	2-Y Treatment	Female 1	Female 2	Male 1	Male 2
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)
disability of family member	-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.

Table B.3: Within Family Externalities: Labor Supply

	Sample: 18 and 30 years old		Family's Head				Other members	
	1-Y Treatment	2-Y Treatment	Mother 1	Mother 2	Father 1	Father 2	Others 1	Others 2
treatment	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)
eligible 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)
eligible 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)
babies in the family	-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)
male	3.386^{+}	3.468^{+}					1.163^{+}	1.160^{+}
	(0.095)	(0.092)					(0.138)	(0.134)
mother education							-0.073	-0.036
							(0.050)	(0.046)
father education							0.020	0.007
							(0.043)	(0.041)
constant	-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)
education level	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
type of activity	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
chi2	1870.149	2145.089	138.292	124.528	114.993	208.058	149.445	150.279
N	17955.000	18842.000	6585.000	6935.000	8158.000	8604.000	2202.000	2291.000
Marginal Effects								
treatment 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)
eligible 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)

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.
Group 1: 1st. group to receive the treatment.
Group 2: 2nd. group to receive the treatment.

Table B.4: Cross Village Externalities: School Attendance. Logit Estimates

	All Sample				Girls	Boys	Girls	Boys
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
neighbor: treated village	0.145**	−0.156	0.065	0.459^{+}	0.791^{+}	−0.255**	1.090^{+}	−0.061
	(0.060)	(0.149)	(0.085)	(0.034)	(0.165)	(0.127)	(0.056)	(0.065)
number of peers in the village		0.017^{+}	0.010^{+}	0.014^{+}	0.010^{+}	0.017^{+}	0.011^{+}	0.017^{+}
		(0.003)	(0.002)	(0.003)	(0.003)	(0.005)	(0.003)	(0.004)
peers in neighbor village[1]		0.018^{+}	0.015^{+}		0.021**	0.013**		
		(0.005)	(0.003)		(0.009)	(0.006)		
peers in neighbor village/distance[2]				0.205^{+}			0.253^{+}	0.173**
				(0.044)			(0.074)	(0.086)
male			−0.161^{+}	−0.156^{+}				
			(0.036)	(0.038)				
father works			−0.066	−0.051	−0.021	−0.096	−0.020	−0.095
			(0.047)	(0.051)	(0.065)	(0.065)	(0.065)	(0.065)
mother 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)
parents 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)
father absent			−0.200^{+}	−0.167^{+}	−0.291^{+}	−0.053	−0.290^{+}	−0.053
			(0.062)	(0.057)	(0.070)	(0.082)	(0.069)	(0.083)
disability of family member			−0.232^{+}	−0.242^{+}	−0.286**	−0.200*	−0.286**	−0.200**
			(0.080)	(0.083)	(0.113)	(0.103)	(0.113)	(0.102)
family's 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)
number of family 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)
cons	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)
year controls	*No*	*No*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
school level controls	*No*	*No*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
age controls	*No*	*No*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
village controls	*No*	*No*	*No*	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Log-likelihood	−6529.148	−6305.114	−3452.655	−3336.558	−1461.068	−1822.531	−1462.018	−1822.413
N	15624.000	15624.000	14190.000	14190.000	6871.000	7201.000	6871.000	7201.000
Marginal Effects								
neighbor: treated village	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)
peers in neighbor village[1]		0.004^{+}	0.001^{+}		0.002^{+}	0.001**		
		(0.001)	(0.000)		(0.001)	(0.001)		
peers in neighbor village/distance[2]				0.018^{+}			0.017^{+}	0.018**
				(0.004)			(0.004)	(0.008)

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.
1. Number of peers living in the neighbor treated village.
2. Number of peers living in the neighbor treated village weighted by the distance between the two villages.

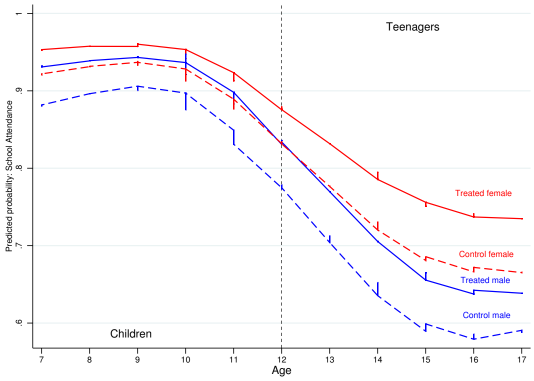
Table B.5: Cross Village Externalities: Instrumental Variables Estimates

	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 of family member	-0.034^{+}	-0.172^{+}
	(0.011)	(0.046)
family's head illiterate	-0.066^{+}	-0.329^{+}
	(0.009)	(0.038)
number of family 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

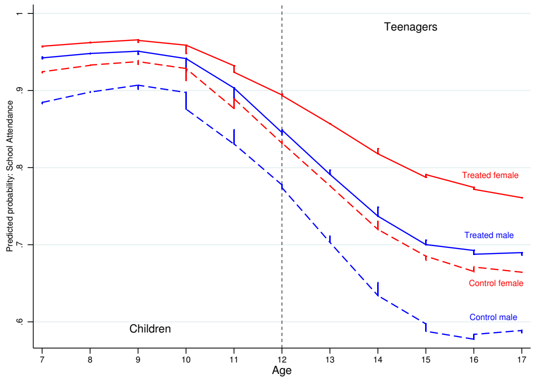
Dependent Var.= 1 if a child attends school; 0, otherwise.

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

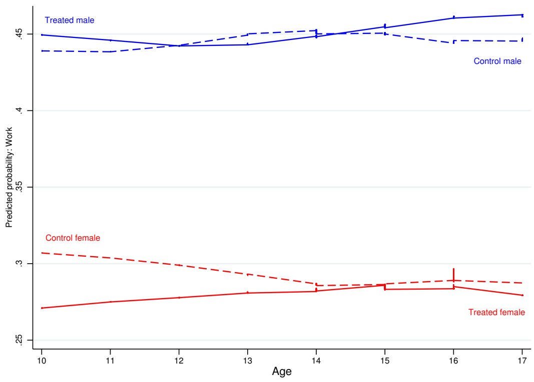
B.3 Spillover Effect: Graphical Results



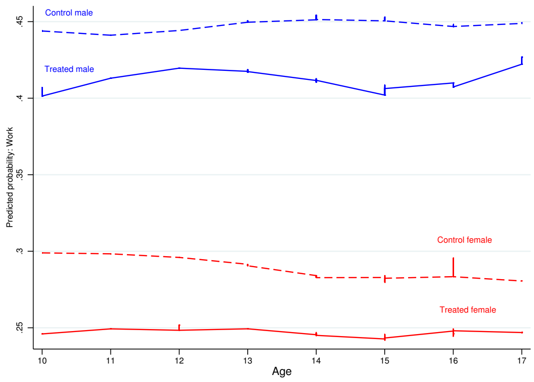
(a) School attendance: 1-year Treatment



(b) School attendance: 2-year Treatment

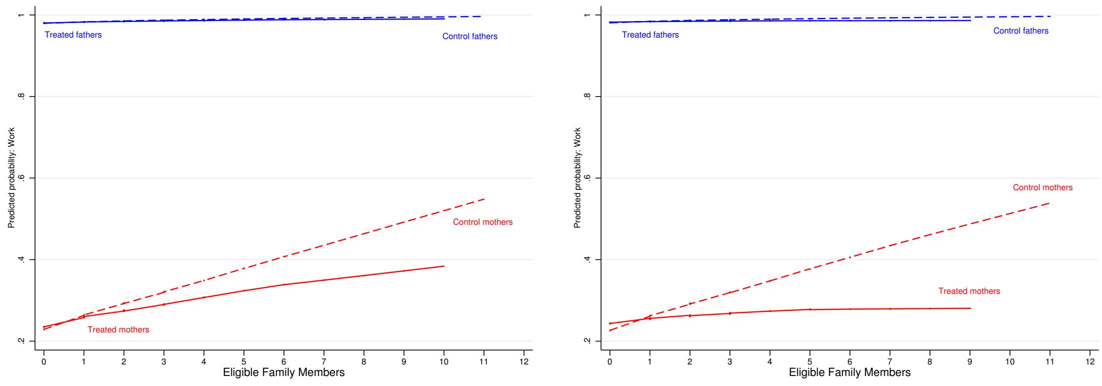


(c) Work: 1-year Treatment



(d) Work: 2-year Treatment

Figure B.2: Direct Effect: Individuals 7-17 years old



(a) 1-year Treatment (b) 2-year Treatment

Figure B.3: Within Family Externalities: Labor Supply

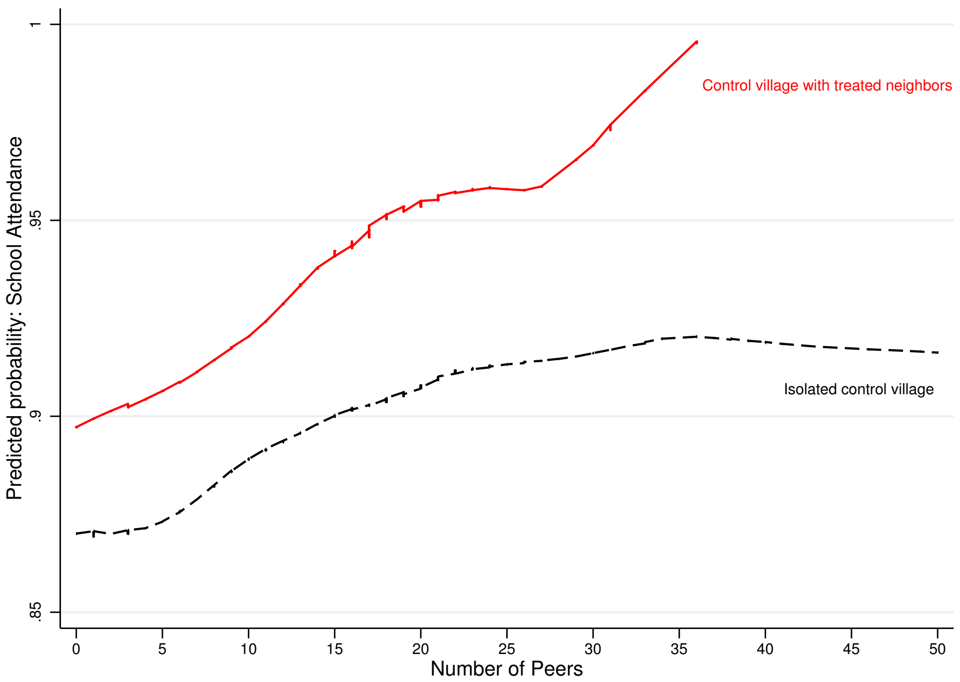


Figure B.4: Cross Village Externalities: School Attendance

Paper 3

Poverty Persistence & Rational Choices - The Limits of Public Incentives*

Poverty affects how individuals behave. Even when these individuals are rational, the environment they face push them to make decisions that are not efficient, but at the same time are the best they can. Using information of a Conditional Cash Transfer program, which its main goal is to reduce poverty by increasing the human capital of individuals, this paper estimates a duration model that determines the number of years a person decides to stay in school before dropping it to start working. Results suggest that the program, in spite of being quite successful at increasing the school attendance, did not have an effect on the dropping rates: individuals between 8 to 20 years old continue quitting school as much as before the program was implemented. We contrast these result by estimating the labor market opportunities poor individuals face when acquiring an extra year of school. Apparently, and since the market conditions do not seem to have improved, people maximize their earnings by just acquiring primary school. After that, an additional year of school has a higher marginal cost than its benefit.

*Paper presented in the "4th. Development Conference of Gretha-Gres", Bordeaux, France 2012.

3.1 Introduction

During the last decade, the number of people living under poverty has considerably reduced. However, in spite of this important achievement, still a big part of the world's population remain poor.[1] In fact, the dynamics of poverty show that nowadays it is more likely that people fall into poverty, and even more, people that left their poverty condition are very likely to go back to their initial situation at the minimum shock.[2]

In the literature it is possible to mention three branches that intent to explain poverty and its persistence. The first branch suggests that poverty is the result of an irrational behavior. The authors relate the inability to leave out of poverty mainly to "dysfunction characteristics" [Mead, 1992], where individuals choose their way of living because their level of satisfaction increases if their in-satisfaction is very high (i.e. people choose to have a low level of consumption, so if they get some, their utility increases exponentially). The driver behind this theory is the difficulty to understand why a person living in chronic poverty can present "irrational" characteristics: work few hours, drop school at an early age, do not save, or spend the few cents they have in unnecessary goods.

The second part of the literature stresses the idea of "poor but efficient" [Schultz, 1964]. The author claims that poor individuals have nothing different from any other individual. People living under poverty have bad lives, but there is nothing special about them. They live under difficult circumstances, but they do the best they can. Finally, modern development economics goes deeper in the economics of the poor and suggests that poverty does not only impose greater constraints to individuals, but it changes the decision-making process itself [Duflo, 2003]. This branch of analysis incorporates insights from "behavioral economics" to understand the economic decisions of the poor. For example, Mullainathan & Thaler [2000] claim that poor individuals have a limited ability of analysis, they do not always make choices that are in their best interest, and they are not purely self-interested. Following the last branch in the literature, many governments in developing economies have implemented antipoverty programs that intent to pull individuals out of poverty by investing in the human capital (health, nutrition and education) of children through monetary grants.[3] The main driver of such initiatives is

[1] Over three billion people (almost half of the world's population) live with less than 2.50USD per day, World Bank [2009]

[2] See, World Bank. Statistics and Indicators [2013].

[3] CCT have significantly increased between 1997 and 2008. While in 1997 there were only two countries with a nation wide programs, by the year 2008, this number increased to 29 countries [Fiszbein & Schady, 2009].

that by relaxing the family's constraints in exchange of positive behavior, individuals will be able to improve their health and education, which, on time, create better opportunities for his own, and their future generations.

The results show a great success of such initiatives in areas of health and nutrition [Legarde *et al.*, 2007], [Rawlings & Rubio, 2005], [Gertler, 2000], [Rivera *et al.*, 2004]. However, schooling outcomes seem to have some problems on time. In fact, in spite of having important rates of attendance [Schultz, 2000], [Schady & Araujo, 2006], [Cardoso & Souza, 2004], individuals continue dropping school once they finish primary school [Behrman *et al.*, 2000], [Reimers *et al.*, 2006]. By using experimental data of an anti-poverty program, we estimate the impact on schooling outcomes and contrast poor individuals' schooling decisions with their labor market opportunities. If targeted individuals continue quitting school it is because either, the opportunity cost they face is too high, or because the marginal benefit of an extra school year is not worth the time spent on it.

Finally, in an exercise of robustness check, we estimate a model of school attainment to contrast with the results of the duration model. If, in fact, individuals continue quitting school, in spite of the program's efforts, the schooling years should remain unchanged.

The document is organized as follows. The second part will describe the sample. Since the sample is part of a randomized experiment, the analysis will be done on comparable subgroups. In the third section, the theoretical framework of schooling decisions respect to returns to schooling is detailed. In the third part, the estimation methods are explained as well as the variables included in the equations. The final part of the documents shows the main results and also sketches the most important conclusions.

3.2 Background Information

Data used in the quantitative analysis correspond to Programa de Educación, Salud y Alimentación (PROGRESA). Serving approximately 25 million people, PROGRESA is Mexico's principal anti-poverty initiative. The program was launched by the government in 1997, and it awards cash grants to families living in poverty. The main goal of the program is to break the intergenerational transmission of poverty by increasing investment of treated families in the human capital of their children. The importance of PROGRESA is that its was conceived as a randomized experiment. In a first stage, and based in geographic localization, 506 villages that met inclusion criteria were chosen to be part of the program. From this villages, 330 were selected as treated and the remaining villages were part of the control group. Within each village the program applied a socio-economic survey that categorized families as poor and non-poor. Only the poor families living in treated villages receive the cash grants.[1]

The transferences are granted bimonthly conditioned upon specific behavior such as preventive health checkups and regular school attendance for children. The money is granted to mothers or to the person in charge of feeding and taking care of the children. Failing to carry out any of the conditions will immediately cancel the transfers. The form to check if the families are covering all the requirements is by controlling the attendance list of the schools and the doctors' records.

Table 3.1: PROGRESA Structure (%)

	Before		**After Implementation**					
	1997		**1998**		**1999**		**2000**	
Treatment[1]	**Yes**	**No**	**Yes**	**No**	**Yes**	**No**	**Yes**	**No**
poor	36.8	22.1	37.2	22.4	46.6	31.9	49.4	31.9
N	125449		130279		115889		130476	

Values are percentages respect to the total number of individuals.
Source: PROGRESA Evaluation Data

The program has survey evaluations every six months after the baseline survey in March 1998. By the year 2003, all the non-poor households became part of the program. Since this paper focuses on the effect of the PROGRESA on eligible individuals, the information of the non-poor is not included in the analysis. Table 3.1 describes the percentage of poor families per village that are part of the program. In the 506 villages, PROGRESA collects information of the household. It is possible to see that the percentage of families receiving the money increases year by year. In the initial survey (1997), this percentage was 36.8%, by the year 2000, families receiving the money were almost half of the villages' households: 49.4%.

3.3 Descriptive Statistics

A number of core questions about demographic composition of households and their socio-economic status were applied in each round of the survey. The surveys include information about family background, schooling, health and nutritional status, health

[1]The cash grants represent about 20% of the household's monthly income.

care utilization, consumption of food and non-food items, income, allocation of time, and productive activities.

Table 3.2: Background Characteristics per Village

	Treated	**Control**	**Diff.**[1]
baby	0.139	0.137	0.001 (0.005)
child	0.236	0.239	-0.003 (0.005)
young	0.149	0.149	0.000 (0.001)
adult	0.429	0.424	0.005 (0.006)
old	0.047	0.048	-0.001 (0.005)
gender: male	0.507	0.500	0.007 (0.004)
family head: father	0.888	0.889	-0.001 (0.009)
mother present	0.997	0.997	0.000 (0.001)
family members: number	5.268	5.352	-0.084 (0.103)
family member with disabilities[2]	0.057	0.055	-0.002 (0.007)
family with migrants[3]	0.193	0.179	0.014 (0.018)
running water	0.052	0.032	0.020 (0.012)
electricity	0.116	0.116	0.000 (0.010)

1. difference between the treated and control groups. Standard errors in parenthesis.
2. blind, mute, deaf, mentally and physically disabled.
3. percentage of families per village that reported to have a family member that migrate.

The importance of PROGRESA is that it was designed to have a random allocation of families. People living in treated villages have exactly the same background characteristics as the families living in the control villages. In this way, before, during and after the program's implementation it is possible to compare the situation between them and more important than that, it is possible to identify the effect on people's behavior.

Tables 3.2 and 3.3 details the baseline data (ENCASEH97) from the rural areas in Mexico. It has information on 125.449 individuals. The observations are divided between control and treated groups. As the numbers suggest, the randomization of the program was very efficient.[1] There are no significant differences between treated and control

[1]The t-test done on the mean differences does not have significant results.

groups. This means that families living in PROGRESA's villages are totally comparable with families living in the Control villages. In this way, once the money is granted, it is possible to evaluate what happened to individuals that receive the money and contrast their situation with people that did not get the money.

We can see that the villages have about 50% of individuals between 0 to 18 years old. In both groups, there are very few people older than 65 years old. The family head, in 90% of the families, is represented by a male. The households have, on average, 5 members. In general, migration is a characteristic of about 20% of the families per village. Generally, people leaving their home have a greater number of years in school. Nonetheless, before and after the program implementation, there are no differences between groups.

Regarding the health situation, 5% of households reported to have a relative that suffers from any type of disabilities. Finally, the table presents two indicators that identify the socio-economic conditions of the families: access to running water, only 3% to 5% of poor families have this service; electricity, 11% of poor families. The education conditions (refer to Table 3.3) show that the maximum years of education in the family are 8 years, which corresponds to complete primary school. Examining this indicator, it is possible to see that the average years of education for parents is around 4 years. In the same way, the actual school attendance is about 50%, indicator calculated for people between 5 and 18 years old who reported to be enrolled that year in school. For children, this indicator is a lot higher, close to 95%.

Working conditions show that villages have 46% of the individuals working. The hourly wage is quite low: only 4 pesos per hour, and it considerably varies depending on the type of work and on the schooling level.[1] People in the sample have very unstable jobs; while some of them work during an entire year, there exists a lot of people working day by day, per weeks or every two weeks. In spite of this, the average time reported of work per week is between 42 to 43 hours.

[1] Banco de Mexico. Historical data: 1USD:7.7918 Mexican pesos. Therefore 4 pesos are 0.50USD

Table 3.3: Schooling & Labor Conditions per Village

	Treated	**Control**	**Diff.**[1]
max. years of education[2]	8.261	8.189	0.071 (0.196)
father education[3]	4.678	4.575	0.102 (0.201)
mother education[3]	4.077	3.799	0.278 (0.205)
actual attendance[4]	0.549	0.553	0.004 (0.015)
working people[5]	0.464	0.452	0.014 (0.007)
weekly worked hours	42.128	43.031	0.903 (0.665)
hourly wage	3.883	3.900	0.018 (0.211)
Working people by age-group			
children	0.036	0.031	0.005 (0.005)
young	0.140	0.136	0.005 (0.007)
adult	0.785	0.800	0.015 (0.011)
old	0.065	0.068	0.003 (0.006)
Hourly wage & schooling level			
no school	3.275	3.020	0.255 (0.245)
preschool	3.546	2.859	0.687 (0.971)
primary	3.908	3.996	0.087 (0.234)
secondary	4.548	5.346	0.798 (0.856)
nb	9.407	16.535	7.128 (4.905)
preparatoria	4.546	4.741	0.195 (1.079)
graduate	8.023	10.356	2.328 (3.224)
Working Hours			
agriculture	42.622	41.695	0.927 (0.798)
non-agriculture	47.248	46.265	0.984 (1.398)
own business	41.898	41.915	0.016 (1.694)
family worker	38.203	38.841	0.638 (2.035)
other	43.047	43.899	0.851 (2.031)
Schooling Years[6]			
agriculture	5.515	5.509	0.006 (0.202)
non-agriculture	7.715	7.754	0.038 (0.402)
own business	5.179	5.755	0.576 (0.399)
family worker	6.315	6.125	0.191 (0.428)
other	4.214	4.071	0.143 (0.449)

1. Difference between the treated and control groups. Standard errors in parenthesis.
2. Maximum years of education reported in the family.
3. Number of years a person spent in school.
4. The total assistance is calculated respect to people between 5 and 25 years old.
5. % of people working between 8 and 65 years old.
6. 1: preschool; 2: primary; 3: secondary; 4: nb ; 5: preparatoria; 6: undergraduate; 7: graduate

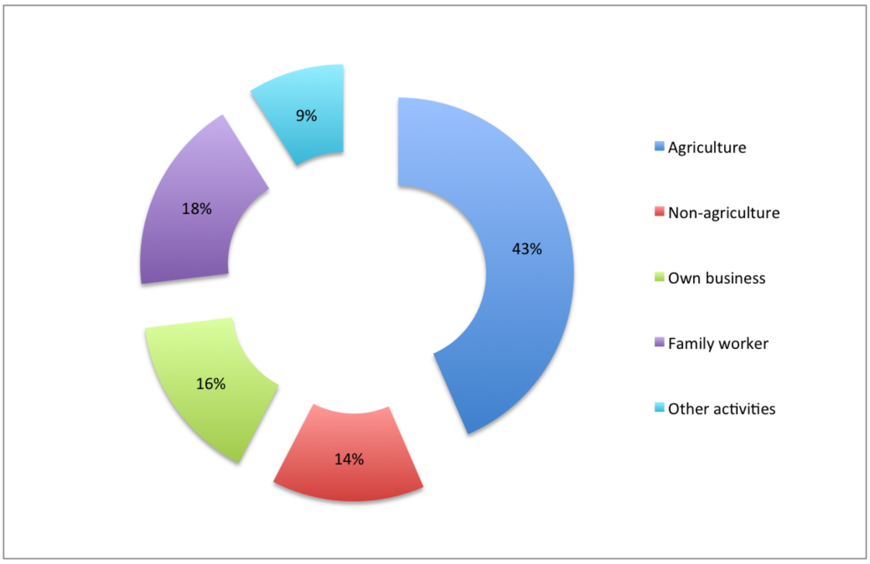


Figure 3.1: Working Population by Area

The working population is composed by 3% of children, 13% to 14% of people between 12 and 18 years old, 80% are adults, and there exists a 6% of individuals in the sample that work after 65 years old. Finally, if we check the difference of wages related to the acquired education, it is possible to see that a higher education is positive related to a higher wage. Nevertheless, the differences do not represent a big change from one level to another. In fact, only people in the "normative" level show to have an important increase in their income.[1] The rest of the people with different education levels, on average, earn 5 to 6 pesos per hour.

Regarding the type of work (see Fig. 3.1), almost half of the workers are located in the agricultural sector (42.9%), while the rest of the people are divided among the non-agricultural sector, their own business, family workers and other areas. In general, people who reported to have a payed activity have not attended more than 8 years of school. The years of schooling do not show important differences among sectors. While this indicator shows that agricultural workers attended 5 years of school, people woking in the non-agricultural sector have almost 9 years. In the rest of the sectors, the years in school does not pass the 7th. year.

[1]The "normative" education is people studying to become a teacher in primary school, or high school.

3.4 Econometric Application

The econometric part estimates two models. The first model uses a duration specification that tests whether people receiving social aid increase their schooling years. A second model estimates the returns to schooling in the sample to evaluate the labor market incentives for people with higher degrees.

The estimation includes information one year before the program's implementation (1997), and three years during the program's intervention: 1998, 1999 and 2000. Even thought the surveys evaluations were done every six months, this document includes data on a yearly basis because of seasonality reasons: people tend to leave their home to work in other localities during the first semester of the year (great part of the population works in the agricultural sector); while in the second semester, people returns to their family and therefore, individual observations seems to be very close from year to year.

The PROGRESA's evaluation surveys are available until year 2007. However, until the year 2000 people included in the treated and control groups, as well as the poor and non-poor subgroups are the same as the initial randomization. By the year 2003, all poor individuals were included in the treated group and therefore it is not possible to compare their situation with people who started with PROGRESA since 1997.

The goal of this paper is to check whether people change their behavior with respect to schooling not only by increasing their school attendance, but by increasing their schooling years. If the money grants, in fact, helped people to relax their budget constraint by making the opportunity cost of education lower, their behavioral change will significantly reduce their desertion rate. However, if people increase their enrollment rates to receive the money, but continue quitting school, the program is not achieving its main goal on time, and therefore the education level in the sample will continue to be very low.

Outcomes as education and health have important results on time. It is quite difficult to evaluate the results for these outcomes with few years of information. For this reason, this paper does not quantify the effect of education on the economic conditions, but determine people's behavior with respect to the cash grants. In this way, three years of information respect to the enrollment rate, as well as the quitting rate can perfectly reflect individual's decisions. The variables included in the analysis are next detailed:

- **Treatment:** binary variable which defines whether a person lives in a village where PROGRESA is applied.
- **Poor:** binary variable that identifies whether a person belongs to a family categorized as poor.

- **Age-groups:** divide people by different age groups. The division is done because conditions, characteristics, decisions differ from one group to other, specially for schooling decisions.
- **Gender:** control for male and female individuals is important. In general, schooling rates tend to differ from one group to the other.
- **Family head:** There exist important evidence that decisions in the family will significantly differ depending whether the family's head is the father or the mother.
- **People in family:** number of people living in the same house. In poor populations, due to cultural factors as well as scarcity of resources, a household is conformed by different families.
- **Migration:** at least in Mexico, migration is a strong characteristic. Most of the families have some relative living abroad.
- **Disabilities:** variable that identifies the households that have at least one person in the family with some kind of physical or mental deficiency.
- **Running water, electricity:** these two variables control for the poverty condition.
- **Years of education:** the number of years reported account from kindergarden and pre-primary school (1-3 years), primary school (4-9 years), secondary school (10-16 years), normative (Nb) and preparatory school (17-20 years), graduate (21-25 years) and postgraduate studies (26-28 years).
- **School attendance:** people have to report if they are attending school by that moment. However attendance does not report people achievement.
- **Woking conditions:** the survey ask individuals from 8 years about their working condition: type of work, worked hours and money received.

3.4.1 School Dropout

The paper estimates a duration model that tests whether people receiving PROGRESA increased their schooling years before deciding to dropout school. The model includes all the individuals between 8 to 20 years old that reported being working and not attending school at the moment of the interview. We limit the analysis to this age group for two reasons: first, we start from 8 years old because the survey evaluations ask about labor decisions from this age; and, second, the program directly targets individuals younger than 18 years old; since we include information for three years after the implementation, people between 18 to 20 years old might also show an effect of the program. In total, we have 7.486 individuals who are not studying and reported to be working. We chose the data on this way, to avoid problems with right-censoring data: people who, by the time of the survey, continued studying and were not working.

Define T to be the duration of schooling (years), or the time people stay in school before starting to work. The duration density function is given by $f(t)$, and the distribution function is $F(t)$. Therefore, the probability of exit (quit school) by time t is given by:

$$F(t) = P(T < t) = \int_{s=0}^{t} f(s)ds$$

and the probability of survival $S(t)$ in a state to at least time t is:

$$S(t) = P(T \geq t) = 1 - F(t)$$

The exit rate or hazard function λ represents the instantaneous exit rate from the state at time t. The hazard rate is the ratio of the duration density to the complement of the duration distribution function at time t:

$$\lambda(t) = \lim_{dt \to 0} \frac{P(t \leq T \leq t + dt | T \geq t)}{dt} = \frac{f(t)}{S(t)}$$

The potential pattern of duration dependence relies on the form of $\lambda(t)$. If people, in fact, changed their behavior with respect to school attendance, and this result is not just a short run response to the cash grants, when computing the survival function of the sample, treated individuals should have an important improvement compared to their control peers in the number of years they attended school before starting to work.

The Kaplan-Meier (see Fig. 3.2) function describes, in a simple way, the behavior of individuals respect to school and work . It involves computing the number of people who quit school at a certain point in time, divided by the number of people who were still in school. The area below the curve represents the mean average time that individuals in the sample stayed in school before starting to work. The graph shows the behavior of treated (red) and control (blue) individuals, and the effect before (dash) and after (solid) the implementation of PROGRESA. In fact, there are some differences on time, after the program's implementation, people stay longer in school; however, this result cannot be attributed to the program, since control and treatment groups have the same curves.

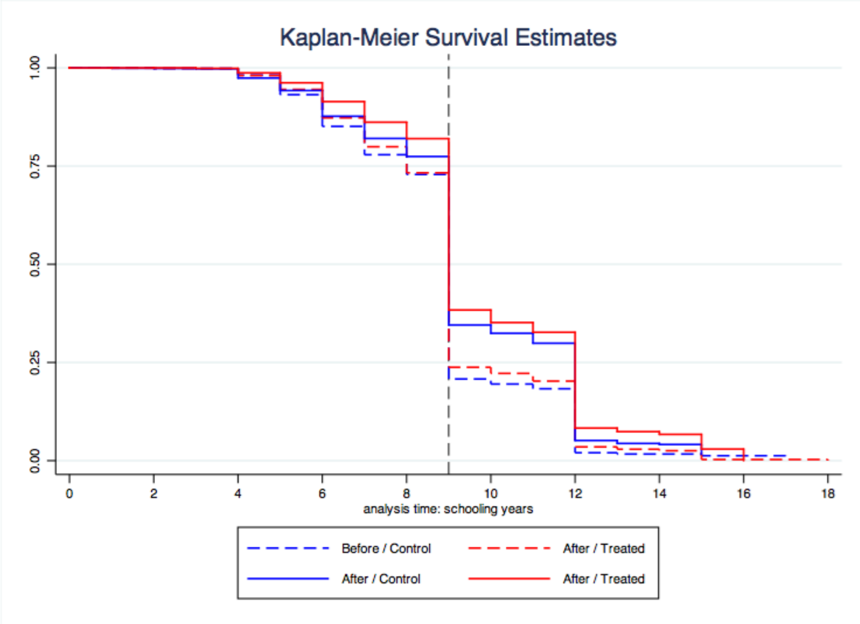


Figure 3.2: Survivor Function

It is possible to show the survival rates at different schooling years, and test the equality of survivor functions. Table 3.4 shows the percentage of individuals in the sample of analysis who stay in school before starting to work by treatment condition, and by timing of the program: the results are the same, there are no significant differences between groups. It is interesting to notice that while in primary school, the schooling rates are very high; however, by the end of primary school, the number of drops increases considerably, and by the beginning of high school, more than 70% of the sample dropped school after PROGRESA was implemented.

Finally, we estimate a difference-in-difference regression to determine the effect of the program on the duration variable Tit:

$$T_{it} = \alpha + \beta_1 D_v + \gamma \tau_t + \delta(D_v * \tau_t) + \sum_i \omega_i X_i + \epsilon_{it} \tag{3.1}$$

where D_v stands for the treatment condition of the village v, τ_t is a dummy variable that is equal to 1 if the observation is before the implementation of the program, and 0, otherwise. Finally, we control for a series of covariates X_i such as gender, parents' education, the number of members in the family, the working status of the mother and the father, whether the individual works in the agricultural sector, and whether individuals when

Table 3.4: Survivor Function

	Before PROGRESA		After PROGRESA	
Schooling Years	Treated	Control	Treated	Control
1	0.9991	1.0000	1.0000	0.9988
3	0.9977	0.9984	0.9983	0.9976
5	0.9295	0.9168	0.9496	0.9291
7	0.7531	0.7375	0.8217	0.7825
9	0.1561	0.1422	0.2646	0.2503
11	0.1271	0.1212	0.2096	0.2068
13	0.0097	0.0016	0.0079	0.0024
15	0.0005	0.0008	0.0004	0.0000
17	0.0005	0.0000	0.0000	.
	Test for Equality of Survivor Functions*			
events observed	2171	1238	2400	1678
chi2(1)	0.46		1.56	

* The test controlled by age groups.

dropping school work in the same activity as the family's head, age and year controls. To estimate the last equation, we use a log-logistic distribution.[1].

The results of Eq. (3.1) are presented in Table C.1. Besides the entire sample, we run different estimation by gender and age-groups. From sample to sample, the results do not change. In fact, in spite of having a positive effect once the program was implemented, the results are not significant. The same happens with the introduction of PROGRESA: the program does not have a significant effect on the survival years an individual decides to stay in school before starting to work. It is important to highlight the fact that the program cuts the money transferences if targeted individuals do not follow the required behavior. If people continue dropping school, they are actually rejecting the money grants.

These last results are a bit puzzling in light of the traditional economic analysis of schooling decisions[2]. How can people drop school and reject the extra money to start working? If, in fact, schooling has an important effect on the productivity and on the life earnings of individuals, having, on average, just primary school will not improve their

[1] Choosing the distribution in the estimation relies on the behavior of the duration variable. It is a continuous probability distribution for a non-negative random variable. It is mainly used for events whose rate increases initially and decreases later. To a more detailed analysis, see Appendix C

[2] See Appendix C1 for a summary of the theoretical background

economic situation. The possible answers are therefore that the money grants, in spite of representing an important part of the family's income, are still very small. Being part of the program helps, but does not solve all the urgent problems: the marginal cost of staying in school is much higher than its marginal benefit. Another option is that the labor market opportunities poor individuals face comprise low productivity activities. Therefore, acquiring more than primary school might be useless. The last option is that experience plays the most important role when deciding about a productive activity. It might be the case that individuals face better opportunities if they have experience rather than if they have more education.

Among the covariates it is important to mention that male show shorter schooling spells compared to women. Parent's education has a direct and positive effect on the duration variable: individuals will stay longer periods in school if their parents have higher education levels. An interesting variable is whether the mother works; if that is the case, in some of the samples in analysis, the time a person spend in school is significantly reduced. Finally, we control for whether a person drops school and performs the family's head activity: if that is the case, the schooling spells significantly decreases.

Using the predicted values of the last model, it is possible to estimate the average time people stay in school in the sample. Controlling by the treatment and other variables, it is possible to find that, on average, treated individuals and control individuals will stay almost 9 years in school, last year of primary school, before dropping to start working. The difference between the groups is quite small and therefore not significant.

One of the potential limitations of the last estimation model is that our sample includes all the individuals who entered the labor market and left school. We limited the analysis to this group because the main question of analysis is to know when individuals decide to quit in terms of schooling years. Nonetheless, it might be the case that the program did have an effect on individuals who continued studying and did not drop school, but who were not part of the sample of analysis. Therefore, to avoid this potential problem, we re-run the same analysis and model estimation using all individuals between 8 to 20 years old. The results are presented by Table C.2 and are the same as Table C.1, suggesting that the program did not have an effect on the schooling decisions of individuals. In spite of this, it is important to mention that this last model have too many right censored observations that might bias the results. For this reason, in a later section, we provide an additional robustness check for this hypothesis.

Table 3.5: Predicted Mean Schooling Years

Village	Treated	Control	Diff. [1]	N
all sample	8.752	8.741	-0.011	2134
	(0.015)	(0.018)	(0.024)	
female	8.918	8.893	-0.025	508
	(0.037)	(0.041)	(0.058)	
male	8.701	8.693	-0.008	1626
	(0.484)	(0.490)	(0.026)	
8≤Age≤15	8.495	8.531	-0.036	805
	(0.031)	(0.033)	(0.048)	
16≤Age≤17	8.924	8.930	-0.006	604
	(0.019)	(0.026)	(0.032)	
18≤Age≤20	8.892	8.820	-0.072*	725
	(0.023)	(0.019)	(0.039)	

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

1. Differences between treated and control groups. Standard errors in parenthesis.

3.4.2 Returns to Schooling

This paper explains people's behavior with respect to schooling decisions by analyzing the situation in the labor market. Specifically, we want to evaluate what are the options a person has if he/she acquires higher levels of education. As defined in the theory of returns to schooling, rational individuals balance their schooling decisions depending on the incentives they have in the labor market. If higher levels of schooling imply higher returns, rational individuals will stay longer periods in school. It is clear, however, that education is not only important for the potential earnings a person can make, but it also provides personal and spiritual growth by different means. Nonetheless, it is important to stress the fact that this analysis is done on people living under extreme poverty. In such case, non-monetary returns are neglected in the analysis: poor individuals are too constrained to think of schooling in a different way than just a mean for increasing their lifetime earnings.

Since the program was randomized in its design, it is possible to start by making simple mean differences between control and treated groups by year of implementation. Table 3.6 shows the main results, and it is possible to see that there are no significant changes. In fact, the percentage of people who reported to have a payed activity keeps unchanged year by year, as well as the mean hourly wage. People living in treated and controlled villages earn, on average, the same wage.

Deciding about the schooling years is a difficult choice because staying in school is quite costly and even worse, having primary or secondary school does not change their economic situation. Going further, this paper estimates a model of returns of schooling in the sample. Assuming returns to schooling are linear, it is possible to define the next equation:

$$lnw_{it} = \pi_0 + \pi_1 S_{it} + \pi_2 Z_{it} + \pi_3 D_v + \pi_4 \tau_t + \pi_5(\tau_t * D_v) + \pi_6 P_v + \sum_i \omega_i X_i + \xi_{it} \quad (3.2)$$

where lnw_{it} is the natural log of the hourly wage of individual i. The hourly wage depends on the schooling years (S_{it}) and the experience level ($Z_i t$). The latter variable is a dummy that controls for whether an individual performs the same productive activity as their parents. In fact, in most of the cases, poor individuals tend to learn and help in the activity developed by the family's head. In such a way that if the father works in agriculture, it is very likely that his children will continue with the same activity. If that is the case, individuals tend to learn the activity, and therefore their expertise is important once they can start working by their own.

The equation also controls for the introduction of PROGRESA (D_v), and the impact of the families receiving the money, ($\tau_t * D_v$). The idea of including these variables is to check whether the cash-grants had an effect in the labor market. One can argue that with additional monetary resources, people have less incentives to work, or one can also say that with the extra money, families have less incentives to send their kids to work since they are less constrained. In addition, to account for the economic situation, the equation includes the percentage of poor families in the village, P_v. Finally, we also include extra covariates to control for background characteristics.

Equation (3.2) has a clear problem of endogeneity of the schooling (S_{it}) variable. To solve this problem, this document uses two strategies to consistently estimate the last equation. The first strategy is to use as instrument the parents' education. In fact, one can see that children's schooling decisions are mainly determined by their parents' education. In this way, parents' education are closely related to children's education, but uncorrelated with their labor situation. The second estimation strategy is to use the average education of the individual's peer group. We assume that people's schooling decisions are closely related with their peers' decisions. As we will see later, both estimators give very close results.

The estimation is divided into two samples. The first one uses information about all

Table 3.6: Descriptive Evidence: Labor Market

Receive treatment	**Yes**	**No**	**Diff.**[1]
working people (1997)[2]	0.230	0.230	0.001 (0.006)
working people (1998)[2]	0.213	0.212	0.001 (0.004)
working people (1999)[2]	0.227	0.225	0.002 (0.003)
working people (2000)[2]	0.211	0.207	0.005 (0.004)
hourly wage (1997)	3.839	4.288	0.449 (0.672)
hourly wage (1998)	5.607	6.962	1.355 (2.245)
hourly wage (1999)	8.508	9.627	1.119 (3.488)
hourly wage (2000)	8.435	10.490	2.055 (1.473)

1. Difference between the treated and control groups. Standard errors in parenthesis.
2. Percentage of people who reported to have a payed activity.
3. Number of hours reported to work.

the individuals that reported to have a payed activity (people from 8 years old), and the second part uses a reduced sample of individuals between 18 and 45 years old. The idea to use two samples is that in the first case we might be biasing the results downwards since we assume all working people are not studying; which is not necessarily true since many people, specially children, study and work at the same time. The second sample includes all the individuals that could have had, at least, the option to finish high school. In this sample we limit the individuals to the age of 45 because most of the productive activities in the sample require strong and healthy people. The results are presented in Table C.3. The first column shows a small but significant effect of schooling years on the hourly wage (0.5%); however, it does not account for the double causality problem, so the coefficients can show inflated results. When using instrumental variables, we find that both techniques show quite close results: an additional year of schooling increases by 2.1%, in the first case, and 1.21% in the second case the hourly wage. These results are very small and, in both cases, not significant.

The treatment has a larger impact compared to the schooling years, but still it is not significant. The dummy variable that accounts for the time the program has been implemented shows important and significant results. In fact, once the program was implemented it seems the hourly wage increases between 10% and 12%. This can be the result of the inflation rate. However, the interaction term coefficient that shows the direct effect of the cash grants on the targeted group has no significant effect.

The estimation of returns to schooling includes two additional variables that try to capture the productive structure of the villages. The first one accounts for the economic situation and it uses the percentage of poor families living in the same locality. As we can see, the results are highly significant and have a strong negative effect on the hourly wage. In fact, an increase of poor density in the village, decreases the hourly wage in about 40%: poorer villages face very bad labor market conditions.

The second variable accounts for the "experience" in the productive activities. We assume that individuals have a higher level of expertise if they develop the same activity as the parents: the learning time is lower, and in most of the cases they start working in the activity since they are kids. In this way, the experience variable is a dummy that is equal to 1 if a person is working in the same economic area as the father and/or mother, and 0 otherwise. In all the specifications this result is significant and negatively related to the hourly wage. We can explain this result by saying that people who pursue family's activity will tend to drop school earlier since their parents are teaching them what they need to know in order to start working.

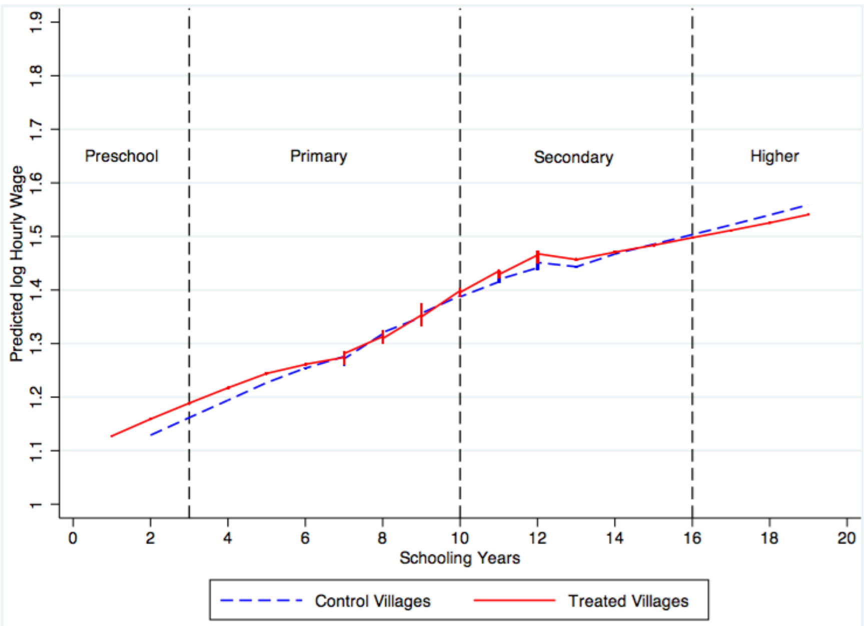


Figure 3.3: Labor Market

Among the control variables, it is possible to see that gender plays an important role: being a man importantly increase the hourly wage in our sample. This result is explained by the fact that most of the economic activities that are develop in the sample are related to physical strength plus the fact that households are composed by several people. In that case, at least for newly born children, the mother has to stay at home. Migration and the effect of other people receiving the money have no significant effect on the hourly wage. Finally, to get a better picture of what is happening in the labor market, we run a non-parametric regression using the predicted values of the estimation. The results are described in Fig. 3.3. The first thing to notice is that the situation for individuals in treated and control villages is not different. In spite of the implementation of PROGRESA, people receiving the cash-grants do not face a better situation in the labor market. We would have expected that the intervention of the program in health and school conditions of people, would have improved their personal conditions to participate in the labor market; however this seems not to be the case.

Even though there is a positive relation between hourly wage and schooling years, the change in the returns to school for an additional year is quite low. It is important to notice that the average hourly wage in the sample is extremely low. The fact that people live under the subsistence level, plus the fact that there exists plenty of individuals with the same characteristics, age, experience and education, makes that the job replacement in these type of markets be really high. This situation makes that people start working with a very low salary and work as many hours as possible.

This situation can be the result of two problems. The first one is that due to the cash grants, families face a substitution effect. By receiving the money grants, people have less incentives to search for a job, and therefore, stay longer periods unemployed. If that would be the case, people in the sample would show a decrease in the working hours, which is not the case. Furthermore, this effect is characteristic at high wage levels where people value more their leisure time than the money they can make by working extra hours. It is important to notice that the money treated families receive cannot buy much; the grants represent around 20% of the average earned by people, but the wage still is much lower than the minimum required for a living. Therefore it does not seem that relevant to think about a trade off in the labor market, and even more when accounting for the hours a person work: in general people work more than 8 hours per day, and in most of the cases, people work weekends.

The second possibility, and the most plausible one, is that the productive activities in poor populations are, in general, transmitted generation by generation. In fact, most of the children, evaluated during the 3 years of implementation, see their future exactly as their parents do now. In that case, they would need as much education as their parents have in order to continue with the same activity. Moreover, if children would like to have a different productive activity, the possibilities they have do not represent an important change: most of them offer the same wage and require the same level of education.

3.5 Robustness Check

3.5.1 School Attainment

One of the main concerns of the duration model presented in this document is the problem with right-censored observations. As explained in the econometric section, we estimated the survival time a person stayed in school before starting to work. We limited our analysis to all the individuals in the age group targeted by PROGRESA, who reported to be working and not studying at the time of the survey. Therefore, we exclude all the individuals who we do not have information about their duration variable. In other words, we have a lot of individuals who were still in school and were not working, so they were not included in the sample of analysis. The right censoring might bring some problems; for this reason, as a robustness check, this document estimates the matriculation rate and school attainment of all the sample. If the program did not change the dropping decisions of individuals, the school attainment must keep unchanged.

It is important to differentiate between matriculation rate and school attainment. While the first one accounts for all the individuals currently attending school, the second rate accounts for schooling achievement. It is important to mention that PROGRESA directly focuses on school attendance; however, if the goal of the program is to increase the human capital of individuals, school attainment is assumed a clear objective. An interesting fact of the data is that the schooling attendance and matriculation rate in the first years of school is close to the optimum. At least at the beginning, almost all children go to school. The turning point is when children decide to continue to high school; from there after, people appear to drop school quicker.

It is possible to start the analysis by making simple mean comparisons to study the effect of the program on the targeted population. Table 3.7 shows the sample mean of the outcomes of interest. In the first part of the table, it is possible to see that before the program implementation (year: 1997), the mean differences between treated and control localities are not different from zero. On the contrary, after 1997, the results for "school attendance" significantly improved for treated individuals. The schooling years also show an increment; however, the results are less strong.

Table 3.7: Descriptive Evidence: Schooling Decisions

	Treated	**Control**	**Diff.**[1]
matriculation rate in 1997	62.3%	60.6%	0.017 (0.010)
matriculation rate in 1998	66.6%	63.2%	0.033*** (0.009)
matriculation rate in 1999	76.8%	72.7%	0.041*** (0.008)
matriculation rate in 2000	70.5%	68.2%	0.022*** (0.008)
average schooling years in 1997	6.244	6.139	0.105 (0.063)
average schooling years in 1998	6.650	6.523	0.127* (0.064)
average schooling years in 1999	7.296	7.210	0.085* (0.061)
average schooling years in 2000	7.822	7.712	0.111* (0.061)

[1.] Difference between the treated and control groups. Standard errors in parenthesis.
Notes: The schooling years go from 1 to 28 years.

The schooling years are estimated using data at the individual level. Equation (3.3) is a model of schooling decisions, where A_{it} is a count variable that describes the number of years a person i attended school. A_{it} has a Poisson distribution that takes integer values, $a = 1, 2, 3, ..., 28$[1].

$$A_{it} = \gamma_0 + \gamma_1 D_v + \gamma_2 \tau_t + \gamma_3(\tau_t * D_v) + \sum_i \theta_i X_i + \mu_{it} \tag{3.3}$$

D_v is the treatment variable that defines whether an individual lives in a PROGRESA village, or in a control one; τ is a dummy-time variable that accounts for the time the program has been implemented. The interaction term, $(\tau_t * D_v)$, estimates the direct impact of the program in the schooling years in the treated villages. X_i is a vector of control variables that includes: the number of people living in the household, whether the father was present in the family, gender, parents' maximum years of education, mi-

[1]In a Poisson distribution the mean and variable can be shown to be $E(S) = var(S) = \mu$. Appendix C presents the histogram (see Fig. C.2) per estimated groups in order to check if this condition holds.

gration, and year effects. In this equation, we allow for clustering errors at the village level.

When applying the model of schooling decisions to the sample, it is important to be aware of the structure of the data. As mentioned before, the estimation includes information on four different years: one before the program's implementation and three after. It is also important to account for the differences among localities and age groups: we cannot compare decisions among kids with those made by teenagers and adults. Finally, the model accounts for the possibility of clustering errors by village. The results reported in Table C.4 present the estimated marginal effects of Eq. (3.3).

From the results, it is possible to see that living in a treated village has a positive but not significant effect on the school attainment of individuals. Our interest mainly relies on the coefficient of the interaction term ($\tau_t * D_v$), which identifies the actual impact of the program on people receiving the money.

Comparing the behavior of individuals in treated villages with the control villages, it is possible to see that, the schooling years in the two groups, in spite of the cash grants, are not different: treated and controlled groups have the same schooling years. This result means that PROGRESA was quite successful to increase the attendance rate with the money grants; however, this results is just a short-run effect since people continue dropping school as much as people that are not part of the program.

Finally, it is important to mention some interesting results among the included controls. For example, men has, in most of the cases, a higher matriculation rate: about 8%. For the schooling years, this value is much lower and not significant. The number of people living in the same household has mixed results: it depends on the age-group analyzed. An interesting results is the one presented by families that as their head have a woman. In fact, if the father is absent, it increases in about 4% the number of years a person attends school. This result, however, changes if we evaluate the regression in different age-groups. For individuals between 5 and 15 years, the direction is the same, but for individuals between 15 to 20 years, this result is negative. This last result can be explained by the fact that if the father is absent, young individuals have a higher urgency to start working to help the family.

3.6 Conclusions

A defining characteristic of chronically poor people is that they remain in poverty over a long period. This can mean that poverty is transmitted from one generation to another, with poor parents having poor children, who are more likely to become poor adults themselves. This intergenerational transmission of poverty can be the long term effect of poor nutrition, inadequate education and health care, few assets or a lack of opportunities that can help to break this cycle. How to make that change has no straightforward answer. In fact, it is not only necessary more education or healthier people to break the cycle, but also to change people's incentives in the markets so that future generations can, at least, increase their earnings compared to current workers.

Poor individuals, as any other individuals, optimize their decisions balancing their costs and opportunities. The main difference is the time frame they face. In fact a person who has all the "survival" conditions solved can make rational decision on a long-run frame. On the contrary, a poor individual, who has very tight constraints, can hardly think about the distant future, so they make decisions on the short-run. The strict conditions, under which poor individuals live, make that the most illogical option in normal conditions, becomes the most rational.

This paper shows that people living in absolute poverty make rational decision. Given their situation and evaluating their possibilities, people choose the optimum number of years of education they require. The problem is, however, that these decisions will not pull them out of poverty. In fact, even people are bribe to continue studying, the unchanged conditions make optimal to quit school at a determined stage. This situation maintains people undereducated, and working in the same underproductive activities; behavior that, on time, will be transmitted to their kids.

Following Duflo (2003), we can conclude that what is needed is a theory of how poverty influences decision-making, not only by affecting the constraints, but by changing the decision-making process itself. Education, by itself, constitutes an important variable to reduce poverty; however, it is not a sufficient condition. Since it implies a huge cost, in terms of time for people who have to work to survive, it cannot be taken only as an easy decision. If it is not accompanied by an improvement in the incentives people face, education will not change people's poverty conditions.

Appendix C

Appendix - Paper 3

C.1 A Theoretical Framework in Returns to Schooling

The causal link between education and labor market is of general interest in theoretical and applied documents. In this context, and following the study made by [Card, 1999], we can summarize the main points of a model of endogenous schooling. The returns to schooling can be illustrated in the framework of a model built on [Becker, 1967]. The model describes individuals that have to decide the optimal years of schooling vs. labor market opportunities: the decision maker balances the benefits of higher schooling against its costs. In general, it is assumed that individuals maximize the discounted present value of earnings vs. schooling costs. This approach is appropriate if people can borrow at a fixed interest rate, and if they are indifferent between attending school or working. However, heterogenous individuals may have different preferences for schooling relative to work, result that may lead to differences in the optimal level of schooling.

Assume individuals have an infinite planning horizon and they accrue a flow of utility in period t that depends on consumption $c(t)$ and on whether they are in schools working part time, or out of school working full time. The utility while they are in school is given by:

$$u(c(t) - \phi(t))$$

where $u(.)$ is a increasing concave function and $\phi(t)$ is a convex function that reflects the relative disutility of school vs. work for the t^{th} year of schooling. The individual faces a discount rate ρ. In this way, the value function that each individual face is given by two expressions. The first given by the utility while in school, and the second part after school.

$$V(S, c(t)) = \int_0^S (u(c(t) - \phi(t))e^{-\rho t}dt + \int_S^\infty u(c(t))e^{-\rho t}dt$$

subject to the constraint:

$$\int_0^\infty c(t)e^{-Rt}dt = \int_0^S (p(t) - T(t))e^{-Rt}dt + \int_S^\infty y(S,t)e^{-Rt}dt$$

where $y(S,t)$ is the real earnings of an individual who has completed S years of schooling. The model also accounts for individuals who have a part time job and earn $p(t)$ when studying, and the tuition cost are $T(t)$. Finally, individuals can borrow freely at a fixed interest rate R.

Solving the individuals' model, the solution with respect to schooling (S) is given by the marginal benefit and the marginal cost of the S^{th} unit of schooling:

$$\Omega_S(S, c(t), \lambda) = \lambda e^{-RS}[\underbrace{\int_0^\infty \frac{\partial y(S, S+\tau)}{\partial S} d\tau e^{-R\tau}}_{MgB} - \underbrace{[y(S,S) - p(S) + T(S) + \frac{1}{\lambda}e^{-(\rho-R)S}\phi(S)]]}_{MgC}$$

A necessary and sufficient condition for an optimal schooling choice is that $MgC(S) = MgB(S)$. Following Mincer [1974], we assume that log earnings are additively separable in education and years of experience:

$$y(S,t) = f(S)h(t-S)$$

where $h(0) = 1$ and the MgB of the S^{th} unit of schooling, evaluated at $t = S + \tau$:

$$MgB = f'(S)\int_0^\infty h(\tau)e^{-R\tau}d\tau = f'(S)H(R)$$

$H(R)$ is a decreasing function of the interest rate. In this way, if earnings are fixed after the completion of schooling, then $H(R) = \frac{1}{R}$. If earnings follow a concave lifecycle profile, then $H(R) = \frac{1}{R-g}$[1]. Under separability, the MgB and MgC of additional schooling is:

$$\frac{f'(S)}{f(S)} = \frac{1}{H(R)}\{1 + \frac{[T(S) - p(S)]}{f(S)} + \frac{1}{\lambda}e^{-(\rho-R)S}\frac{\phi(S)}{f(S)}\}$$

In the last equation, the left hand side is the proportional increase in earnings associated with the S^{th} unit of schooling. The right hand side corresponds to the marginal cost of the additional unit of schooling. Assuming there are no specific preferences between school and work ($\phi(S) = 0$), no tuition cost $T(S)$, and no part time job $p(S)$, the equation is reduced to:

$$\frac{f'(S)}{f(S)} = \frac{1}{H(R)}$$

In the left hand side the equation expresses the marginal benefit (MgB) of an additional year of schooling, while in the right hand side, we have the the marginal cost (MgC). It is possible to assume that the MgB is decreasing as the schooling time increases. On the contrary, the MgC is an increasing function of the schooling years.

$$MgB = b_i - k_1 S$$
$$MgC = r_i + k_2 S$$

A sufficient condition to get the optimal schooling choice scheme is that $MgB = MgC$, being $k_1 + k_2 = k$:

$$S_i = \frac{b_i - r_i}{k}$$

[1] g is a constant growth rate.

C.2 Log-logistic Distribution

The time path of schooling years is not linear (see Fig. C.1). Individuals acquire additional years of schooling as long as the marginal benefit of an additional year is greater than its marginal cost. Since the returns to schooling are much lower in poor populations, the time pattern is shifted downwards: the maximum average schooling years in the sample is around 10 years (beginning of high school). As the figure suggests, people, at the beginning, increases their schooling years on time; however, at higher levels, the schooling years decreases.

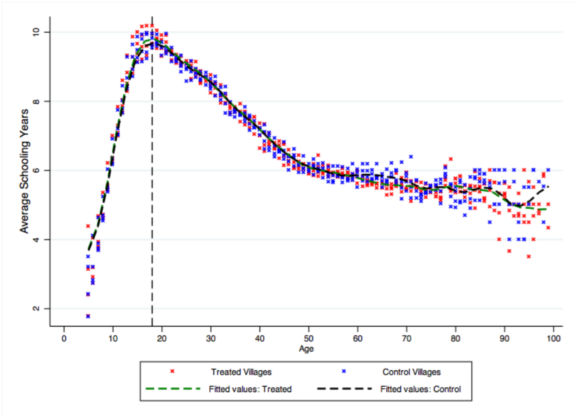


Figure C.1: Schooling Years

C.3 Schooling Years Distribution

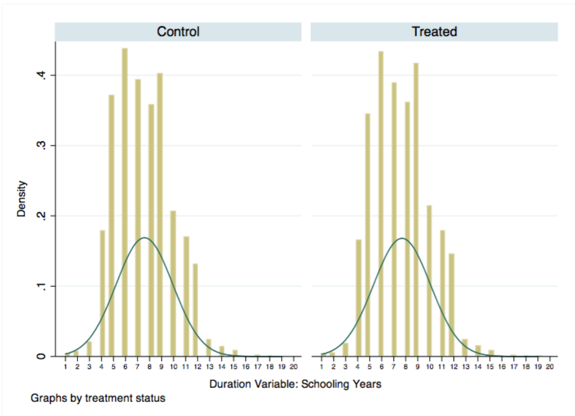


Figure C.2: Histogram: Schooling Years

C.4 Model Estimation Results

Table C.1: Duration Model: Years of Schooling: Individuals that entered the labor market

	All Sample	Male	Female	8≤Age≤12	13≤Age≤15	16≤Age≤18
after implementation	0.066	0.008	0.087	0.436	0.002	0.049
	(0.04)	(0.07)	(0.05)	(0.24)	(0.04)	(0.08)
treatment	0.004	-0.018	0.011	0.002	0.019	-0.005
	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
after*treatment	0.012	-0.023	0.029	-0.194	0.123	-0.071
	(0.09)	(0.12)	(0.12)	(0.14)	(0.08)	(0.27)
male	-0.012			-0.030*	0.006	-0.001
	(0.01)			(0.01)	(0.02)	(0.02)
parents' education	0.018+	0.020+	0.019+	0.012**	0.019+	0.026+
	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.01)
family members	-0.002	0.001	-0.005	-0.005	-0.003	0.001
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
mother works	-0.021	-0.065*	0.002	-0.044	0.002	-0.014
	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
father works	0.011	-0.015	0.028	0.044	-0.035	0.008
	(0.02)	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)
activity: agriculture	0.015	-0.008	0.029	-0.008	0.059**	0.007
	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
activity: family's head	-0.054+	-0.025	-0.065+	-0.020	-0.087+	-0.069**
	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
cons	2.088+	2.085+	2.072+	2.142+	2.124+	2.023+
	(0.04)	(0.07)	(0.05)	(0.06)	(0.07)	(0.07)
age controls	Yes	Yes	Yes	Yes	Yes	Yes
year controls	Yes	Yes	Yes	Yes	Yes	Yes
ln_gam						
cons	-2.102+	-2.183+	-2.087+	-2.261+	-2.115+	-1.966+
	(0.04)	(0.07)	(0.04)	(0.06)	(0.05)	(0.04)
chi2	304.510	.	1179.099	177.052	43.458	26.855
N	2134.000	508.000	1626.000	805.000	604.000	725.000

Dependent var.= number of years a person attended school before start working.

* $p < 0.05$, ** $p < 0.01$, + $p < 0.001$. Standard errors in parenthesis.

Table C.2: Duration Model: Years of Schooling: Individuals between 8 and 20 years old

	All Sample	Male	Female	8≤Age≤12	13≤Age≤15	16≤Age≤18
after implementation	0.053	0.058	0.066	0.051	0.052	0.051
	(0.04)	(0.08)	(0.05)	(0.06)	(0.05)	(0.08)
treatment	0.005	-0.006	0.009	0.007	0.015	-0.007
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
after implementation*treatment	0.018	0.010	0.017	-0.012	0.048	0.090
	(0.10)	(0.12)	(0.14)	(0.12)	(0.11)	(0.26)
male	-0.005			-0.012	0.013	-0.010
	(0.01)			(0.01)	(0.02)	(0.02)
parents' education	0.021^{+}	0.020^{+}	0.022^{+}	0.017^{+}	0.022^{+}	0.026^{+}
	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)
family members	-0.004	0.001	-0.007^{**}	-0.009^{**}	-0.003	-0.001
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
mother works	0.012	-0.026	0.035	0.014	0.035	-0.018
	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)
father works	0.002	-0.021	0.018	0.016	-0.048	0.011
	(0.02)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)
activity: agriculture	0.002	-0.022	0.017	-0.022	0.055^{**}	0.001
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
activity: same as family's head	-0.049^{+}	-0.030	-0.060^{+}	-0.029^{*}	-0.074^{+}	-0.064^{**}
	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
cons	2.124^{+}	2.127^{+}	2.109^{+}	2.175^{+}	2.119^{+}	2.075^{+}
	(0.04)	(0.06)	(0.04)	(0.05)	(0.06)	(0.07)
age controls	Yes	Yes	Yes	Yes	Yes	Yes
year controls	Yes	Yes	Yes	Yes	Yes	Yes
ln_gam						
cons	-2.037^{+}	-2.111^{+}	-2.019^{+}	-2.149^{+}	-2.050^{+}	-1.924^{+}
	(0.03)	(0.06)	(0.03)	(0.05)	(0.05)	(0.04)
chi2	69.185	21.190	62.018	36.590	42.730	23.404
N	2868.000	721.000	2147.000	1450.000	661.000	757.000

Dependent var.= number of years a person attended school before start working.

* $p < 0.05$, ** $p < 0.01$, $^{+}$ $p < 0.001$. Standard errors in parenthesis.

Table C.3: Returns to Schooling

	All Sample			18 $\leq$Age$\leq$45	
	OLS	IV-I[1]	IV-II[2]	IV-I[1]	IV-II[2]
schooling years	0.005*	0.021	0.012	0.020	0.010
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
treatment	0.014	0.009	0.012	0.029	0.034
	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
after implementation	0.000	0.121^{+}	0.124^{+}	0.106^{+}	0.110^{+}
	(0.00)	(0.02)	(0.02)	(0.03)	(0.02)
after implementation*treatment	-0.017	-0.015	-0.016	-0.016	-0.018
	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)
%of poor neighbors	-0.403^{+}	-0.391^{+}	-0.398^{+}	-0.422^{+}	-0.431^{+}
	(0.05)	(0.05)	(0.05)	(0.06)	(0.06)
family's head activity	-0.024*	-0.022*	-0.024*	-0.041^{+}	-0.04^{+}
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
father absent	0.005	0.008	0.007	-0.005	-0.008
	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)
male	0.110^{+}	0.111^{+}	0.110^{+}	0.084^{+}	0.083^{+}
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
migrants in family	0.026	0.027	0.026	0.023	0.023
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
cons	1.291^{+}	0.954^{+}	1.019^{+}	1.210^{+}	1.300^{+}
	(0.05)	(0.10)	(0.07)	(0.10)	(0.07)
age-groups	Yes	Yes	Yes	No	No
year effects	Yes	Yes	Yes	Yes	Yes
state effects	Yes	Yes	Yes	Yes	Yes
R^2	0.397	0.390	0.396	0.373	0.381
F	87.80				
χ^2		1856.92	1853.85	1319.92	1341.42
N	11702	11702	11702	7407	7407.000

* $p < 0.05$, ** $p < 0.01$, $^{+}$ $p < 0.001$

[1.] IV-I uses as instrument the parents' education level.

[2.] IV-II uses the average education level of the peer group

Table C.4: School Attainment

	(I)	(II)	(III)	(IV)	5<Age≤10	10<Age≤15	15 <Age≤20
treatment	0.019	0.012	0.010	0.010	0.007	0.014	0.020
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
after implementation		0.175^{+}	0.175^{+}	0.068^{+}	0.644^{+}	0.010	0.015**
		(0.01)	(0.01)	(0.01)	(0.05)	(0.01)	(0.01)
after*treatment		0.009	0.010	0.013	-0.004	0.003	-0.005
		(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
people in the family			0.006^{+}	0.00^{+}	0.003	0.003^{+}	-0.009^{+}
			(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
father absent			0.044^{+}	0.041^{+}	0.047^{+}	0.019*	-0.028**
			(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
male			0.006	0.007	0.004	-0.011**	-0.005
			(0.00)	(0.00)	(0.01)	(0.00)	(0.00)
max. parents' education			0.001	0.001	-0.002^{+}	0.003^{+}	0.016^{+}
			(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
migrants in family			0.038^{+}	0.038^{+}	-0.003	0.012**	-0.000
			(0.01)	(0.01)	(0.01)	(0.00)	(0.00)
cons	1.865^{+}	1.727^{+}	1.667^{+}	1.67^{+}	0.460^{+}	1.452^{+}	2.212^{+}
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)
year effects	No	No	No	Yes	Yes	Yes	Yes
χ^2	2.8	1850.7	1884.1	3908	4648.3	3015.1	804.2
AIC	457600	454292	391531	388732	25264	177273	116267
BIC	457619	454330	391614	388835	25342	177369	116356
N	95208	95208	82024	82024	9091	45642	25144

* $p < 0.05$, ** $p < 0.01$, $^{+}$ $p < 0.001$

Part III

Conclusions and Outlook

Chapter 4

Evaluation, Outlook and Conclusions

Poverty is still a characteristic of a great part of the world's population. The problem is so big, that only thinking about it is overwhelming and a pessimistic judgement rapidly invades our thoughts. Trying to find a macro recipe to overcome the problem is not feasible. For this reason, focusing at the micro level to solve one problem at a time, is the best or only option scientists have to take real action.

This thesis analyzes the case of one of the most successful antipoverty initiatives implemented in developing countries. I use microeconometric techniques to evaluate the direct impact, the presence of externalities, and the potential limitations of CCT programs. CCT programs target families living under poverty by combining monetary incentives with conditioned behavior. The conditions include regular school attendance, frequent health checkups and nutrition monitoring for the beneficiaries. The main goal of these programs is to increase investment in the human capital of children.

The research is divided into three papers that analyze the case of two CCT programs implemented in Mexico and in Colombia targeting poor families in rural areas. I chose these two samples because, in structure and design, they are very similar, and because they are nationwide initiatives that had consistent and regular follow-ups.

The first paper studies the case of PROGRESA, a CCT program implemented in Mexico in 1997. It started as a randomized experiment, but nowadays it covers more than 25 million people. I analyze the direct impact and spillover effect of the Program on health and related outcomes using two yearly evaluation surveys.

The results suggest that the program has significant positive effects on the direct beneficiaries, but it also has an impact on the family's members living in treated households. In the same way, the presence of spillovers shows a significant effect on the health status of individuals, who in spite of not being part of the program, interact with treated individuals.

An interesting finding that requires a deeper analysis is that the results suggest that, in some cases, the impact on untreated individuals seems to be greater than the impact on direct beneficiaries of the program. This could be explained by the presence of externalities, where untreated individuals also benefit indirectly from the program, so they can easily catch up with treated individuals.

This happens specially when measuring health outcomes because by treating a contagious disease, for example, the treatment will not only decrease the rate of infection among the treated, but also among the non-treated. This result shows the importance to account for the presence of externalities in the framework of program evaluation. Otherwise, focusing only on the beneficiary group might underestimates the total effect.

The second paper, using the same strategy, but different estimation techniques, determines the direct impact and generated externalities of a CCT program implemented in Colombia in 2001. The program, known as “Familias en Acción”, was implemented as the main antipoverty initiative targeting families living in poverty. It works using the same structure of PROGRESA, by granting money with specific behavioral conditions to the beneficiaries.

The paper focuses on the effect of the program on the school attendance and labor participation of targeted beneficiaries, but it also evaluates the potential presence of within-family externalities, as well as cross village externalities. In addition, it analyzes the results in the short and medium term by contrasting the results between villages that receive the program first, and villages that entered the program later. In contrast to the PROGRESA, “Familias en Accion” does not have a randomized structure; therefore, to evaluate the program, I had to run some previous matching algorithms to make treated and control groups comparable.

The results suggest that targeted individuals significantly increased their school attendance due to the program. Nevertheless, the impact on the labor participation seems to be unchanged: individuals continue working as much as before the program was implemented. This result raises the question about the long-term impact of the program. If individuals, in the age of studying, allocate important part of their time to work, the quality of what they are doing in school is uncertain. It could be that individuals attend school to fulfill the requirements of the program, but are not really taking advantage of being in school. In spite of this, when comparing the results over time, it seems that for people who receive the program for longer periods the program shows a positive, but not significant improvement in terms of labor participation.

In terms of within-family externalities, the paper estimates the impact on labor participation for young adults and adults living in treated household. One of the main concerns of antipoverty programs implementing money grants is the way incentives work. It could be that, due to a substitution effect, people receiving the money replace their working time with leisure. Nevertheless, it turns out that the program did not have an important impact on the labor participation of young adults, or adult individuals. In spite of this, when accounting for the effect of having treated children in the household, the results show a negative impact on the labor participation of the mothers.

This last result suggests that the imposed conditionalities to the family force adult individuals, especially mothers, to take responsibility of certain activities related to the program, such as taking children to school or to the doctor. Since these activities are mandatory in order to receive money, most women will not be able to commit in productive activities. If women have to stay at home, or have to undertake more house-related activities, their future does not seem very promising: a later insertion in the labor market will be very difficult. However, if we analyze this problematic from the perspective of children, the extra time parents allocate building the human capital of their children will have an important positive impact in the long run.

In terms of cross-village externalities, the paper exploits the geographic localization of the villages and estimates the presence of spillovers in the school attendance of children. The estimation took all the control villages that had contact with treated villages, and compared the effects with isolated villages. The results show that the school attendance of children living in control villages increases when the village has a treated village as a neighbor. This result is robust to different estimation techniques and also shows the importance of a deep analysis of the presence of externalities. Since the control group, by different channels, also benefits from the program, the estimation of the effect on the treated should be cautious: a simple comparison between treated and control groups will undermine the real effect of the program.

The third paper uses again the case of PROGRESA, but this time analyzes school outcomes that are expected to have an impact in the long run. This paper only focuses on the beneficiaries, and it implements a difference-in-difference specification. If CCT programs are intended to increase the human capital of children, the conditions regarding school attendance, health, and nutrition, must show results in their school attainment. By this I mean that a higher attendance, accompanied by a better health status, implies an improvement in the final educational outcomes of children. However, this seems not to be the case.

In fact, the results of a duration model estimating the time individuals decide to stay in school before dropping out to start working, are unchanged by the treatment. This result resonates with the literature, which in very few cases find significant positive results. If we contrast this result with a returns to schooling model, it is possible to justify this behavior by the fact that people have no incentives to continue studying. In fact, most of the people in the sample work in agricultural activities, so what they need to know, is learned by experience, and at primary school. Secondary school does not give them a significant input, but instead represents a huge opportunity cost.

It is important to mention that these results apply only for rural populations. In fact, analysis done in urban areas shows that individuals have great incentives to increase their school attendance, and finish secondary school. This happens because the opportunity costs in rural areas and urban areas are significantly different. Nevertheless, we have focused the analysis on this group since the incidence of poverty in Latin America is mainly related to people living in rural areas.

The general conclusion of this research is that incentives work, and they work as long as people want them. Therefore, if the intention is to reduce poverty, the first thing to do is to understand how poor individuals think, and decide to better design programs targeting specific problems. Poverty clearly changes the way people behave, and not because they are different from any other people, but because the environment they face is so constrained that sometimes inefficient decisions, are the only possibility.

Conditional Cash Transfers (CCT) programs, as well as many other social interventions have legitimate and commendable intentions. By now, most results appear significant and positive. Nonetheless, the limitations are clear. In terms of health, for example, most of the detected illnesses are easily preventable and the treatment is not costly; however, problems such as self-medication, besides the bad quality of health providers, are still latent. Additionally, one of the main causes of the many detected illnesses is a bad nutrition. Therefore, the programs' objectives should be redesigned and focused on how to get people to eat healthier, not only when they are children, but during their entire lives.

In terms of education, the programs have had a great impact on school attendances; however, school attainment is still an issue. In addition, an important part of the problem is not that individuals do not want to go to school, but the quality of the schooling services themselves. In many areas, for example, there exists plenty of schools but not enough teachers, or the other way around. In addition, the quality of the education is something that is not taken into account in the programs' designs. Sometimes, the curricula of the educational programs mainly covers formal education, and no practical assignments.

Finally, the main objective of these antipoverty initiatives is to break the intergenerational transmission of poverty. To do so, the programs plan specific actions on health, nutrition and education to build the human capital of younger generations. Nevertheless, even when individuals improve their health, and increase their human capital, there are no better opportunities in the market: labor perspectives may remain unchanged. Therefore, initiatives such as CCT need to be complemented with other interventions such as workfare or employment programs, as well as social pensions.

In any case, CCT programs have been working for only 15 years. There is still too little information, and many things to learn and improve. By now, however, in spite of the clear limitations, such antipoverty initiatives show important improvements in poor people's life.

4.0.1 Future Work

The present work has showed that policy interventions using different types of incentives might have an effect beyond the targeted group. The walked road has raised several questions that could be the starting point for future research in the topic.

First, it would be interesting to determine the different channels from which the spillovers appeared. Are these spillovers generated due to market transactions, or due to the presence of social interactions? The identification of these channels would make more efficient the application and enforcement of policies applied to poor populations.

Second, since CCT programs mainly target children, the required behavior in exchange of monetary grants involves the active participation of other family's members, who, in most of the cases, is the mother. It would be interesting to study the long run effect in terms of women empowerment. If women have to take care of children, this will have an impact on their labor market insertion, which, in turn, might also have an effect not only on her future, but on the future of her children.

Third, following the same argument, if the future of children depends on their mother, then policies should try to target more girls than boys. With a more complete data set than includes several years of observations, it would be interesting to estimate what is the long run effect of an antipoverty program on the families where the family's head is a woman, compared to the families where the family's head is a man.

Fourth, data plays a very important role in the study of this topic. Available and reliable information that covers long periods of time will be very helpful in the estimation of long term effects, specially when analyzing the results in the socio-economic conditions of families.

Finally, in more theoretical terms, it would be very valuable to elaborate a more structured theory about how poor individuals make decisions. Is it really the case that people, under certain conditions, make decisions using a different time framework, where the optimal option is not always the more efficient?

Acronyms

CCT Conditional Cash Transfers

PROGRESA Programa de Educación, Salud y Alimentación

ZINB Zero-Inflated Negative Binomial

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