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The Indirect Effects of Conditional Cash Transfer Programs: An Empirical Analysis of Familias En Accion

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THE INDIRECT EFFECTS OF CONDITIONAL CASH TRANSFER PROGRAMS: AN EMPIRICAL ANALYSIS OF FAMILIAS EN ACCION.

BY

MONICA OSPINA

A Dissertation Submitted in Partial Fulfillment
of the Requirements for the Degree
of
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ACCEPTANCE

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

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ABSTRACT

THE INDIRECT EFFECTS OF CONDITIONAL CASH TRANSFER PROGRAMS: AN EMPIRICAL ANALYSIS OF FAMILIAS EN ACCION

By

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Conditional cash transfer (CCT) programs have become the most important social policy in Latin America, and their influence has spread to countries around the world. A number of studies provide strong evidence of the positive impacts of these programs on the main targeted outcomes, education and health, and have proved successful in other outcomes such as nutrition, household income, and child labor. As we expect CCT programs to remain a permanent aspect of social policy for the foreseeable future, demand for evidence of the indirect effects of CCT programs has grown beyond the initial emphasis of these programs. My research pays particular attention to these relevant but unintended outcomes, which have been discussed less extensively in the literature.

Familias en Accion (FA), a CCT program in Colombia, started operating in 2002 and has benefited approximately 1,500,000 households since its beginning. The results of the program's evaluation survey, representative of poor rural households in Colombia, are a very good source or investigating not only the unintended effects of the program but

also the microeconomic behavior of poor households and social policy issues in the country. Using a panel dataset from FA, I address three empirical policy questions: (i) to what extent is consumption of beneficiary households better insured against income shocks? (ii) has the program displaced child labor as a risk-coping instrument?, and (iii) are there any incentive effects of the cash transfers and the associated conditionalities on the labor supply of adults in recipient households?

Each of my research questions is addressed separately; however, the results, taken together, can be informative in understanding the safety net value of the program and their potentialities to reduce poverty in the long term. I find that the program serves as an instrument for consumption smoothing. In particular, FA is effective in protecting food consumption, but not nonfood consumption, and it reduces consumption fluctuations in response to idiosyncratic shocks but not to covariate shocks. Results also reveal that FA works as insurance for the schooling of the poor but is not able to completely displace child labor. Finally, the results also show that beneficiary mothers are devoting more time to household chores and that girls and female adult labor are complementary. Male labor supply has increased while boys have increased leisure time as a response to the program.

Chapter 1 . CCT PROGRAMS FOR CONSUMPTION INSURANCE: EVIDENCE FROM COLOMBIA

Introduction

Poor households in developing countries live with high levels of risk and limited access to formal financial systems for credit and insurance. To secure their levels of consumption, or smooth consumption, households have traditionally engaged in different ex-post risk coping strategies; i.e., depletion of assets, increase of labor supply, informal borrowing, or transfers from relatives. Also, risk-averse households can take ex-ante actions to mitigate the effects of negative income shocks; i.e., income smoothing. However, neither of these alternatives allows poor households to achieve an optimal allocation of risk across time, and most of these strategies are costly in terms of long-term poverty and vulnerability. In particular, ex-post consumption smoothing strategies might result in households' decreased capital accumulation, and the income-smoothing mechanism might result in reduced investments in productive assets. Thus, the inability of households to cope with risk is a channel through which they can get into a poverty trap. For these reasons, the research on risk coping behavior and consumption smoothing arrangements of poor communities in developing countries is a crucial issue in the formulation of policies aimed to reduce poverty.

The purpose of this paper is twofold. First, we analyze the degree of consumption insurance of poor households in Colombia in relation to fluctuations in their incomes due to idiosyncratic and community shocks.[1] Second, we evaluate the effects that a

[1] Idiosyncratic shocks affect only particular households while covariate shocks affect a community as a whole.

conditional cash transfer program (CCTs), Familias en Acción (FA), has had on protecting households from the negative effects of shocks. By doing this, we hope to contribute to the literature of consumption smoothing in developing countries as well as to provide new evidence of the role of CCTs as risk management instruments. A good understanding of how and which public interventions provide effective insurance is crucial for policy design.

Economics literature has broadly studied how individuals smooth consumption in response to income shocks. Two main hypotheses have dominated the literature. On one hand, the full risk-sharing hypothesis assumes that consumption is fully insured against idiosyncratic income shocks but not against community income shocks. On the other hand, the permanent income hypothesis (PIH) assumes that, under complete credit markets, self-insurance through borrowing and saving may allow inter-temporal consumption smoothing against idiosyncratic and covariate shocks. Although both hypotheses have been rejected repeatedly (e.g., Townsend 1994; Ravallion and Chaudhuri 1997; Deaton 1992; Skoufias 2003), empirical evidence has shown that consumption reacts too little to permanent income shocks to be consistent with the economic theory (Campbell & Deaton, 1989; Attanasio & Pavoni, 2006). Because these models are extreme characterizations of individual and market behavior, recent literature has addressed the issue of whether partial consumption insurance is available to agents. This paper, in addition to following the traditional approach of testing the hypotheses of complete consumption insurance, estimates partial insurance parameters from the data following the model of partial insurance proposed by Blundell et al. (2008).

In addition to identifying the relationship between consumption smoothing and income shocks, we give special attention to how public interventions—CCTs, in particular—can play a significant role in reducing consumption vulnerability of poor households. According to Morduch (1999), CCTs guarantee that a minimum of insurance is received in order to compensate for under-provision of safety-net services in poor areas. There are several ways in which we can expect CCT programs to reduce the risk of vulnerability: They can (1) reduce income fluctuations because they increase income irrespective of shocks and thus have the same insurance properties as permanent income; (2) displace non-desirable coping strategies, such as high-interest loans, child labor, or depletion of productive assets; (3) create a regulatory and institutional framework to scale up services through informal safety nets; and (4) counteract the government's lack of ability to respond, whether at the central or local level (Cox & Jimenez, 1992).

FA provides subsidies to families on the conditions that all household members receive periodic health checks and that all children are enrolled in and attend school regularly. Given the importance of the program at a national level, a rigorous impact evaluation design has been followed since the very early stages of the program. This has allowed for the collection of repeated observations of beneficiary households surveyed before and after the implementation of the program, as well as the collection of similar data from comparable households that have not been covered by the program. Thus, this panel dataset provides an excellent opportunity for measuring consumption insurance and reveals possible roles of public interventions as risk management instruments. This study has some advantages over other similar studies because of the quasi-experimental design of the program it studies and because of the comprehensive data collected from the program's evaluation survey. First, the balanced panel dataset has detailed information on consumption, income, and shocks for a representative sample of poor households living in small villages in Colombia. Most of the datasets used in earlier studies to evaluate consumption smoothing report either income or consumption, not both. For example, in order to estimate partial insurance parameters for the United States, Blundell et al. (2008) have to infer consumption statistically, since consumption and income data are not available for the same households in a single dataset.

Second, while some studies use changes in income as measures of shocks (Skoufias, 2003; Townsend, 2004), others use dummy variables for the occurrence of idiosyncratic shocks in a given period of time (Cochane, 1991; Mace, 1991). Although income has been criticized as a right hand side variable since it can be endogenous in the consumption equation (Cochane, 1991), if we are able to control for that endogeneity, income variance at household and community levels are very informative about the degree of consumption insurance of poor households. Furthermore, as frequency and intensity of shock events are difficult to capture in occurrence shocks data, a better understanding of the vulnerability to shocks is obtained when we are able to complement these results using income variance and shock events as measures of the risk faced by these households. The dataset used in this analysis uses both income variance and shock events to estimate consumption insurance parameters. Finally, as we have data for treatment and control households before the program was implemented, we are able to estimate an unbiased effect of the program on consumption smoothing, controlling for any pretreatment differences, and for time variant differences at the municipality level.

This paper makes three important contributions to the existing literature. First, it adds to the empirical literature on consumption insurance by providing evidence of the ability of poor households in Colombia to insure consumption against idiosyncratic and covariate shocks. Prior evidence of consumption smoothing has been limited to results from a particular dataset from India[2] and a few other samples collected mainly in Asia and Africa (Baez, 2006). Latin America, a region with a massive proportion of people living in poverty who are subject to income shocks, is clearly underrepresented in this literature, in large part due to the lack of suitable information for investigating risk and insurance of poor households. Second, it contributes to the social program evaluation literature by going beyond assessing the impact of the program on its main objectives to analyze the consequences of participation in other dimensions, such as the degree of informal risk sharing. Third, it is the first paper, to our knowledge, that estimates consumption insurance parameters under both the full risk-sharing model and a partial insurance model.

Based on all specifications used in this research, we support estimations from the partial insurance model as it allows for self-insurance instruments other than savings. We observe a high, but not complete, level of consumption smoothing among poor households in small villages in Colombia, with food consumption's being better insured than nonfood consumption. In addition, results suggest that FA has been effective as a risk management instrument protecting food consumption when households are faced by income shocks and has not displaced risk pooling among households in the same communities. These findings provide strong indications that households engage in risk

[2] A pool of cross-sectional data for the period 1975–1984 from the International Crops Research Institute of the Semi-Arid Tropics (ICRISAT).

management strategies aimed at insulating, at least partially, consumption changes from income changes. For instance, our results suggest that the introduction of this program has enforced risk coping instruments such as the use of savings and assets, and has displaced internal transfers. If FA has in fact crowded-out or enforced existing informal risk coping strategies and how the final well being of beneficiary households has been affected are issues not addressed on this paper. Finally, we conclude that FA, despite not being a consumption insurance program, helps treated families to smooth consumption. Results are robust to different specifications.

The rest of the paper is organized as follows. The next section provides an overview of the program and a description of the evaluation sample used for the empirical analysis. The subsequent section examines risks faced by households in rural Colombia and describes the data used for the empirical analysis. Following is a section that presents basic predictions of the full risk-sharing model and the influential findings on risk coping behavior and consumption smoothing arrangements in developing economies. The next section contains the empirical model and results for the full insurance model. The two subsequent sections present the model used in this paper to estimate partial insurance parameters based on both Blundell et al.'s (2008) and this study's estimations, respectively. The following section presents an analysis of risk coping strategies used by households to buffer adverse income shocks, and the final section reports the conclusions and makes suggestions for future research.

Familias en Acción

The program Familias en Acción is a welfare program run by the Colombian government to foster the accumulation of human capital in rural Colombia. It is similar to other CCT programs, such as Progresa, in Mexico (now called Oportunidades); Red de Proteccion Social, in Nicaragua; and Bolsa Familia, in Brazil, that have been implemented in middle-income countries during the last decade in an effort to break the intergenerational transmission of poverty. The FA program is aimed primarily at improving the education, health, and nutrition of poor families. The nutrition component consists of a basic monetary supplement that is given to all beneficiary families with children under seven years of age. The health component consists of vaccinations and growth and development checks for children, as well as courses on nutrition, hygiene, and contraception for their mothers. Participation in the health component is a precondition for receiving the benefits of the nutritional component. All children between 7 and 18 years old are eligible for the educational component. To receive the grant, they must attend classes during at least 85% of the school days in each school month as well as during the whole academic year. The size of the grant increases for secondary education and is equal for girls and boys. The amount of the subsidy on a monthly basis at the time of the baseline survey was 14,000 Colombian pesos (COP) or (US$6) for each child attending primary school and COP$28,000 or (US$12) for each child attending secondary school in 2005. The nutritional supplement[3] is paid to families with children aged between 0 and 6 years. The amount is COP$46,500 or (US$20) per family per

[3] This subsidy is an alternative to participation in a pre-existing program called Hogares Comunitarios. Beneficiaries cannot participate in both programs with the same children. However, families with children both under and over the age of 6 can choose to send the young children to a Hogar Comunitario and to participate in FA with the older children.

month. The average transfer received per household is COP$61,500, which represents approximately 25% of average household income of beneficiary households. In general, all the transfers are received by the female head of the household every two months.

Familias en Acción determined household eligibility in two stages: first by identifying target communities and then by choosing low-income households within those communities. Selection criteria for target communities were based on the following conditions. The town must: (i) have fewer than 100,000 inhabitants and not be a departmental capital, (ii) have sufficient education and health infrastructures, (iii) have a bank, and (iv) have a municipality administrative office with relatively up-to-date welfare lists and other official documents deemed important. A subset of 622 of the 1,060 Colombian municipalities qualified for the program. Eligible households were those registered at SISBEN[4] level 1 at the end of December 1999, with children under 17 years old, living in the target municipalities. SISBEN 1 households account for roughly the lowest quintile of Colombia's household income distribution (Attanasio, 2004).

The program started operating in the latter half of 2002.[5] It has benefited approximately 1,500,000 households since its beginning, and the cost has ascended to the sum of 300 thousands of millions of Colombian pesos annually (US$150 million). The cost of the program corresponds to the 0.5% of the Colombian GDP and represents approximately 10% of educational expenditures in the country.

The Evaluation Sample

For evaluation purposes, it was decided to construct a representative stratified sample of treatment municipalities and to choose control municipalities among those that

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[4] SISBEN, Sistema Unificado de Beneficiarios, is a six-level poverty indicator used in Colombia to target welfare programs and for the pricing of utilities.
[5] In a few municipalities the program started as early as the end of 2001.

were excluded from the program but that belonged within the same strata. The strata were determined by region and by an index of infrastructure based on health and education. The control towns were chosen within the same stratum to be as similar as possible to each of the treatment towns, in terms of population, area, and quality of life index. Most of the control municipalities were towns with basic school and health infrastructures but without banks or, in the few cases chosen to match relatively large municipalities, just over 100,000 inhabitants. As a consequence, control towns are broadly comparable to treatment towns (Attanasio, 2004). In the end, the evaluation sample was made up of 122 municipalities, 57 of which were treatment and 65 of which were controls.

For each municipality, approximately 100 eligible households were included in the evaluation sample. The total evaluation sample consists of 11,462 households interviewed between June and October 2002 (baseline survey), 10,742 households interviewed between July and November 2003 (first wave), and 9,566 households interviewed between November 2005 and April 2006 (second wave). The attrition rate between the three rounds was approximately 16%.[6] Most of the observations lost were households which children´ age exceeded the required age or households that move from their location and were no possible to find again. Compliance was very high,[7] more than 97% of the eligible households participate in the program, so for the analysis we include in the sample all observations from treatment municipalities.

The final longitudinal data used in this study include information from 6,519 repeated households, after excluding households that received payments before the

[6] According to Attanasio (2007) attrition between baseline survey and the second follow up survey is not statistically different between treatment and control households. Therefore we assume that lost of observations is random.

[7] The main reason for no compliance was lack of the required documents fro registration. None of the households reported lack of interest of participation in the program.

baseline survey and households located in control municipalities that received payments during the second survey.[8] At the household level, the sample consists of families that are potential beneficiaries of the program—that is, households with children from the poorest sector of society. Data are collected at both the household and the individual level. The available data provide a rich set of variables that allows us to measure consumption of durables and non-durables, family composition, household socio-demographic structure, labor supply, nutritional status of children, education, household assets, income, and different shocks to income, for both rural and urban households.

Empirical Evidence on Risk and Consumption

Shocks

The variables used to identify the various shocks experienced by households are obtained from direct questions in the evaluation survey. In each of the three survey rounds, the household was asked whether during the last year it had experienced any of the following shocks: crop loss or job loss, death of a household member, illness of any household member, violent attack or displacement, or weather shock.[9] We include an additional shock, unemployment of the household head, which takes a value of one if the household head was looking for a job for more than three months during the year previous to the survey. In that way, we expect to capture a severe income shock.

For the sample of households in treatment and control municipalities, the prevalence of different types of shocks at the household level during each of the cross-section surveys are reported in Table 1. As we observe, there is no statistical difference

[8] A total of 13 municipalities of the control sample were converted to treatment municipalities in 2005, before the second wave of the evaluation survey.
[9] Fire, floods, or other catastrophic events.

between treatment and control households for all of the shocks, except for illness during the first round. In order to control for potential endogeneity of this shock, we distinguish between illnesses of children, which can be very endogenous, and illnesses of the household head and other adults, which should be less endogenous. Participation in the program could decrease the vulnerability to disease shocks of children, as the program imposes regular visits to health centers as a condition for receiving part of the transfers (Attanasio, 2004). We find that illness of the household head is not statistically different between treatment and control municipalities, suggesting that it is an exogenous shock, while illness of children and spouse are correlated with program participation and so might be endogenous. For the purposes of this research we use exclusively illness of household head as a measure of shock.

Data show that the exposure of households to crop loss and unemployment of household head is very high: over 10% of households had at least one crop loss and over 5% had at least one member unemployed for more than 3 months during the year previous to the interview. Around 11% of the households reported having the household head ill for more than two weeks at least once over the year prior to the survey. Death of any household member, being a victim of violence, and weather shocks are less frequent but can be very harmful to poor families because they result not only in loss of income but also in increased household expenditures.

Table 1. Frequency of Idiosyncratic Shocks

	Crop loss	Unemployment, HH head	Death, HH member	Weather	Violence
Baseline					
Control	11.39%	5.20%	1.81%	1.55%	1.16%
Treatment	9.58%	5.14%	2.01%	0.95%	1.02%
T-test (p value)	0.483	0.777	0.709	0.615	0.808
1st wave					
Control	12.66%	5.47%	1.81%	1.06%	0.95%
Treatment	13.50%	4.93%	2.54%	1.25%	1.48%
T-test (p value)	0.545	0.323	0.085	0.913	0.136
2nd wave					
Control	12.25%	5.87%	2.09%	5.95%	1.50%
Treatment	13.67%	5.61%	2.39%	5.57%	1.89%
T-test (p value)	0.563	0.193	0.598	0.469	0.536
Baseline					
Control	9.37%	12.40%	9.56%	8.67%	
Treatment	11.65%	13.96%	11.43%	7.95%	
T-test (p value)	0.136	0.636	0.582	0.636	
1nd wave					
Control	10.16%	12.66%	9.05%	7.61%	
Treatment	9.97%	11.28%	8.18%	6.51%	
T-test (p value)	0.024	0.645	0.067	0.045	
2nd wave					
Control	9.61%	10.11%	7.32%	3.92%	
Treatment	10.00%	10.60%	7.08%	3.33%	
T-test (p value)	0.223	0.574	0.293	0.569	

Notes: T-test of difference in household means computed clustering at the municipality level.

In order to capture the covariate nature of weather shocks, we use the proportion of households within a municipality reporting to have suffered this shock (de Janvry et al., 2006). Community violence is obtained from other sources and measures the number of terrorist attacks that municipalities had during the year before the interview.[10] Mean statistics and differences among treatment and control municipalities are presented in Table 2. As we can observe, there are not pre-treatment and post-treatment statistical differences in the occurrence of these covariate shocks between treatment and control municipalities.

Table 2. Frequency of Covariate Shocks

	Survey	**Treatment**	**Control**	**T-test (p value)**
Weather	Baseline	0.82	0.80	0.9725
	1st wave	0.96	1.06	0.8631
	2nd wave	1.35	1.74	0.1639
Violence	Baseline	0.82	0.87	0.8744
	1st wave	0.75	1.25	0.2075
	2nd wave	0.89	1.61	0.3476
Observations		2804	3715	

Notes: Numbers indicates the average proportion of households on each municipality that have suffered weather and violence shocks. T-test of difference in means among communities in the sample.

Consumption

The evaluation survey of FA contains detailed information on food and nonfood expenditures in all three rounds: baseline, first wave, and second wave. In food expenditures, there is information on the amount of money spent by households in buying

[10] These data have been collected by Interconexion Electrica SA (ISA) since 1998. ISA is the biggest power line operator in Colombia, which has been the target of recurrent terrorist attacks.

fruits and vegetables, cereals and grains, meats and animal products, and other food products, like soft drinks, alcoholic beverages, coffee, tea, etc. In the nonfood expenditures category, there is information on the money spent on clothing, health products and services, house maintenance products, school and educational goods, transportation, utilities, and other nonfood expenditures, like cigarettes, social events, and toys. Expenditures on durables, such as furniture, and luxury items are excluded from our expenditure measures as they not represent a regular expenditure of the household.

Depending on the commodity, good, or service, the survey asked the head of household about the expenditures made during the week, month, semester, or year prior to the date of the survey. In order to construct the measures of household consumption used in this paper, we converted all expenditures into a household's monthly expenditures and then added them up across the corresponding categories: total consumption, food consumption, and nonfood consumption. We also deflated the measures using the National Consumer Price Index of Colombia and turned them into adult-equivalent[11] pesos at constant 2002 prices. In-kind food consumption[12] is included in our measures using town-level prices observed for households buying similar commodities.

Table 3 shows that households spend around COP$8,000 per adult equivalent per month on total consumption, and that 70% of these expenditures are on food. There are no pretreatment differences in consumption between treatment and control households. Attanasio et al. (2005) have shown the effectiveness of the FA program to increase food consumption in both rural and urban areas. They estimate a 15% increase in average consumption levels one year after the baseline survey. They also find that shares in food

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[11] Household members older than 12 years old are counted as 1 person; household members younger than 12 years are counted as 0.5 person.

[12] Commodities consumed but not purchased (i.e. produced or received as pay or as a gift.

and nonfood consumption are not affected by the program but that it has created redistributive effects in favor of children through expenditure on children's clothing and on education. They also found that the program has not significantly affected consumption of adult goods, such as alcohol and tobacco or adults' clothing.

Table 3. Consumption at Baseline

	Total	**Food**	**Nonfood**
Treatment	83067.72	60222.32	22845.4
Control	82711.8	60305.42	22406.38
T-test	0.3678	0.2239	0.1385

Notes: Consumption measures are per adult equivalent deflated to 2002 price level in Colombian pesos. T-test of difference in means computed clustering at the municipality level.

Control Variables

Table 4 provides the means and standard deviations of the main variables used in the analysis for the sample of households in the treatment and control municipalities for all three surveys. All of the variables used in all of the regressions are at the household level. Monthly household income is constructed by adding reported labor income, self-employment, pensions, interest, rents, and government transfers, including FA potential transfer.[13] Income transfers and remittances received from neighbors, friends, and relatives are excluded from total income, as these sources of income are likely to reflect ex-post adjustments to shocks. *Income* is expressed in adult equivalent measures and deflated to 2002 prices. *Agriculture* indicates the household head was occupied in agricultural activities. *Members economically active* indicates the number of persons in

[13] Potential FA transfer was estimated for all beneficiary households according the number of beneficiary children in the household.

the household older than 12 who were working or looking for job at the moment of the survey. *Education variables* indicates the last level of education by the head and partner of the household.[14] *Urban* is a dummy variable that takes a value of one for households located in urban areas and zero, otherwise. *Household composition variables* represents the proportion of household members by age.

[14] Education categories are: 1, none; 2, incomplete elementary; 3, complete elementary; 4, incomplete secondary; 5, complete secondary; 6, college; 7, graduate.

Table 4. Summary Statistics of Main Variables for all Survey Rounds

	Treatment		Control	
Variable	Mean	Std. dev.	Mean	Std. dev.
Income	59742	47636	53954	57921
Labor Income	28390	41619	29128	50064
HH head age	45.182	12.540	46.727	12.676
Wife age	39.889	9.959	40.997	10.177
HH head education	2.885	1.430	2.974	1.495
Spouse education	3.083	1.198	3.159	1.290
Female HH head[a]	0.216	0.412	0.182	0.386
Own house[a]	0.271	0.444	0.245	0.430
Urban[a]	0.461	0.499	0.547	0.498
Agriculture[a]	0.107	0.161	0.096	0.150
Number of HH members 0–6	0.875	1.003	0.695	0.927
Number of HH members 7–12	1.391	1.026	1.428	1.005
Number of HH members 13–17	1.293	0.898	1.238	0.905
Number of HH members	4.669	1.784	4.789	1.784
Members economically active	1.885	1.187	2.024	1.223
Observations	2804		3715	

Notes: Averages based on three rounds. Income measures are per adult equivalent deflated to 2002 price level in Colombian pesos.[a] Mean values of dummy variables represent percentage of households that meet each of the conditions of the variables.

Full Risk Sharing and the Permanent Income Hypothesis

The most relevant risk coping strategies theorized in the literature are the full risk-sharing hypothesis and the permanent income hypothesis (PIH). The full risk-sharing hypothesis implies that, once aggregate shocks are accounted for, the growth rate of consumption would be independent of any idiosyncratic shock affecting the income available to the household (Bardhan & Udry, 1999). That is, the only risk that any household faces is the risk faced by the municipality as a whole. The alternative mechanism to coping with income shocks is the permanent income hypothesis, which states that a household with no opportunity for cross-sectional risk pooling, but with unlimited access to a credit market and separable preferences of consumption and labor, makes savings or lending decisions so that the effects of shocks are spread out between current and future consumption (Bardham & Udry, 1999). According to the hypothesis, individuals tend to smooth consumption when facing transitory income fluctuations. In practice, these hypotheses are not very relevant to most of the rural households in developing countries, given the inexistence of complete credit markets.

Although both hypotheses have been repeatedly rejected in studies using micro-data, empirical evidence has shown that consumption reacts too little to income shocks to be consistent with the theory. Townsend (1994) and Ravallion and Chaudhuri (1997) test the hypothesis in the ICRISAT Indian villages and reject it, although they find a substantial amount of risk sharing. Deaton (1992) and Grimard (1997) test the hypothesis of perfect risk sharing within villages and ethnic groups in Côte d'Ivoire and find little evidence of any risk pooling at the municipality level and somewhat stronger evidence within ethnic groups. Udry (1994) also rejects the hypothesis for northern Nigerian villages. Skoufias (2003) examined the extent to which Russian households were able to protect their consumption from fluctuations in their income using longitudinal data from 1994 to 2000. The study found that consumption was only partially protected from idiosyncratic shocks to income; with food consumption's being better protected than nonfood consumption expenditures.

Evidence from developed countries has also rejected the hypothesis of full risk insurance (Mace, 1991; Cochrane, 1991). Cochrane (1991), using data on household food consumption from the Panel Study of Income Dynamics (PSID) for the period 1980–1983, regressed changes in consumption onto different measures of idiosyncratic shocks. His results rejected the full insurance hypothesis for some but not all of the different shocks. Similarly, Mace (1991) tested consumption insurance with panel data from the U.S. Consumer Expenditure Survey (CEX). She could not reject the full insurance hypothesis when evaluating changes in consumption against changes in income, but she did reject full insurance when using growth rates. Finally, using household panel data from Bangladesh, Ethiopia, Mali, Mexico, and Russia, Skoufias and Quisumbing (2003) examined the extent to which households are able through formal and/or informal arrangements to insure their consumption from specific economic shocks and fluctuations in their real income. The study showed that adjustments in nonfood consumption appeared to act as a mechanism for partially insuring ex-post the consumption of food from the effects of income changes.

These findings raise the question of how households achieve some level of consumption smoothing given their limited access to financial markets. It seems that poor households engage in self-insurance strategies and mechanisms to secure their level of consumption once they face negative shocks. The most common self-insurance mechanisms for uninsured households are taking loans from the informal financial sector (Udry, 1994), selling assets (Deaton, 1992; Rosenzweig & Wolpin, 1993), increasing household labor supply (Kochar, 1998), or sending children to work in order to supplement income (Jacoby & Skoufias, 1997). Townsend (1994) showed that even extremely poor villages in rural India may have self-insurance strategies that allow them to come close to an optimal allocation of risk bearing. While these actions enable households to spread the effects of income shocks over time, they might have adverse consequences in the long run in terms of poverty and future vulnerability of households.[15]

According to Baez (2006), the work to date on the extent of consumption smoothing in rural areas allows us to draw three important conclusions. First, most if not all of the empirical work has mainly rejected the full risk-sharing model. Second, and regardless of that rejection, a large amount of consumption smoothing is taking place. Rural households are not purely consuming what they earn, although the poorest have less scope to do so. And third, considering some market failures that obstruct formal insurance in rural villages, informal mechanisms seem to play a significant role in protecting their consumption.

As these conclusions have been widely accepted, recent literature has gone beyond the complete market model and has proposed and encouraged “the construction and testing of market models with partial insurance” (cited in Blundell et al., 2008; Deaton & Paxson, 1992). Also, literature has centered on alternative informal instruments to bear risk, estimating the extent of consumption insurance over and above self-insurance, including the role of public interventions. In this paper we address both issues. First, we investigate how well-known public interventions in developing countries—

[15] For example, there is evidence that the use of children as part of the household labor pool compromises human capital and productivity of those children, raising the risk of poverty for the next generation. Also, if assets that are used to buffer consumption from income fluctuations are themselves used in the production process, then there can be important effects on future income from even temporary shocks to current income.

CCTs—can play a significant role in reducing consumption vulnerability of poor households. Second, we estimate the degree of consumption insurance under the full risk-sharing model and under a partial insurance model recently proposed by Blundell et al. (2008).

Public interventions can play a significant role in strengthening or displacing the informal insurance mechanisms already in place. The following examples illustrate some of the effects of public intervention on consumption insurance. In South Africa, Jensen (2002) compares the difference in the level of remittances received by pensioned and non-pensioned workers, after the increase in pension levels, relative to the difference before the increase. Findings based on the crowding-out effect differ across both groups. In Mexico, public cash transfer programs have not displaced informal mechanisms within the scheme of risk-sharing mechanisms (Skoufias, 2003); the evidence, however, is not clear for Northern Thai villages, where the effects of public intervention vary across distinct private transfers and informal mechanisms (Townsend, 1995). Finally, in the case of Mexico, García-Verdú (2002) analyzes a model of informal insurance and also finds that there is no crowding-out effect between cash transfers and informal safety nets.

To date, no structural model has been estimated to address the issue of partial insurance directly. Blundell et al. (2008) address the issue of whether partial consumption insurance is available to agents and estimate the degree of insurance over and above self-insurance through savings. They do so by contrasting shifts in the distribution of income growth with shifts in the distribution of consumption growth and then analyze how these two measures correlate over time. We follow this methodology to estimate the parameters of partial insurance for transitory and permanent shocks. Section 6 presents the model proposed by Blundell et al. (2008), which is used in this paper for the estimation of partial insurance parameters.

Empirical Evidence of Consumption Insurance under the Full Risk-sharing Model

In this section we consider the model of Pareto efficient risk pooling within a community to estimate the extent of risk sharing in poor households in Colombia and to test the effect of FA as an instrument for consumption smoothing. One way of testing the hypothesis of complete risk sharing within a community is to examine whether the growth rate of household consumption is independent of the growth rate in household income after controlling for aggregate shocks. We employ the following specification commonly encountered in the literature (e.g., Cochrane, 1991; Mace, 1991; Townsend, 1994; Ravallion & Chaudhuri, 1997). [16]

$$C_{ivt} = \alpha_0 + \alpha_1 S_{ivt} + \alpha_2 FA_{ht} * S_{it} + \alpha_3 FA_{ht} + \beta X_{ivt} + \delta_i + \gamma_v + \mu_t + \tau_{vt} + \varepsilon_{ivt} \quad (1)$$

where $\mathrm{C_{ivt}}$ refers to adult equivalent consumption per capita of household *i* in municipality *v* at time *t*; $\mathrm{S_{ivt}}$ represents idiosyncratic shocks; FA is a dummy for households that participate in the program; $\mathrm{X_{ivt}}$ is a set of socioeconomic and demographic characteristics of the household that takes into account the composition of the household by age, sex, and education level of household head; and $\delta_i, \gamma_v, \mu_t, \tau_{vt}$ and ε_{ivt} represent household, municipality, time, municipality-time fixed-effects, and the idiosyncratic error term, respectively.

Theory predicts that, under complete risk sharing, $\alpha_1 = 0$, and provides an estimate of the extent to which idiosyncratic income shocks play a significant role in

[16] Similar specifications are defined in terms of consumption and income growth and include a set of binary variables *D* identifying each community separately by survey round (round and community interaction terms) in order to control for covariate shocks.

explaining the household-specific consumption changes. For the purposes of the empirical analysis, the insurance group is defined to be the full set of households within a municipality.[17] Since our sample is representative of poor households in small towns in Colombia, and credit and insurance markets don't function at all in these towns,[18] the identification of the insurance group is adequate. In addition, we should expect that insurance arrangements are easier to organize and enforce in small and poor communities.

To test the effect of FA on consumption smoothing of beneficiary households, equation (1) include FA_{ht}, which is a binary variable equal to 1 for households in treatment municipalities for the first and second follow-up surveys, and 0 for households in control municipalities in all three surveys and for treatment municipalities at baseline. In this equation, the coefficient α_2 is the difference in the vulnerability to risk between beneficiary and control households in the program that have been hit by the same shock. A negative estimate of α_2 implies that FA has decreased vulnerability to risk in the treatment communities. An insignificant estimate of α_2 suggests that there are no significant differences in the level of consumption insurance between control and treatment households. The coefficient α_3 reflects the effect of the program on consumption for households that have been hit by each of the income shocks considered.

Since the program was not randomly assigned among participants and control households, we can expect program participation being endogenous on the consumption equation. However, we found balance between the covariates for each sub-sample[19] so we assume that program participation is not correlated with the unobservable

[17] On average, there are 50 households in each municipality.
[18] Less than 5% of the households have credit or a savings account.
19 Each sub-sample corresponds to treatment and control households hit by the same shock.

characteristics on the consumption equation. In addition, using fixed effects regressions we are able to control for unobservable time invariant characteristics. Alternatively, we used matching methods to find control households comparable to treatment households. Results from matching are very similar to results without matching for crop loss and illness of the household head, and matching was not possible for death and unemployment shock events. Therefore, we show results from fixed effect regressions.

We consider different definitions of consumption and different types of idiosyncratic shocks to estimate fixed effects regressions. As dependent variables we use food consumption, nonfood consumption, and total consumption. The idiosyncratic shocks considered are: (i) death of a household member, (ii) illness of the household head, (iii) crop loss or job loss, and (iv) unemployment of household head. The household surveys asked each household whether it has suffered any of these shocks during the year prior to the date of the interview. Hence, each household was allowed to declare whether it was affected by a shock or not.

Fixed effects estimates of equation (1) presented in Table 5 include estimations for one type of shock at a time, and households hit by one shock, two shocks, and three or more shocks.[20] As discussed above, these estimates are obtained under the assumption that the insurance group consists of all households in a municipality and include municipality-year fixed effects. All regressions control for household composition by age, sex, and the following household characteristics: age and dummies for level of education of the household head, female household head, number of household members

[20] Regression is also estimated using all shocks together. Results are not different from estimations using one shock at a time indicating there is no correlation among shocks.

active in the labor market, dummy if the house is owned as a measure of assets, dummy for households working in agriculture as a proxy of vulnerability to shocks, and municipality characteristics such as population, number of schools and health centers, dummy for households located in urban regions as well as in different economics regions in the country.

Table 5. Impact of Idiosyncratic Shocks on Consumption: fixed effects estimation

	Food consumption	Nonfood consumption	Total consumption
Crop loss	-0.083***	-0.026	-0.068**
	(0.02)	(0.03)	(0.02)
***Treatment**	0.060*	0.069	0.056*
	(0.02)	(0.06)	(0.02)
Unemployment	-0.155***	-0.285***	-0.236***
	(0.02)	(0.04)	(0.04)
***Treatment**	0.077	0.005	0.063
	(0.07)	(0.10)	(0.07)
Death	0.000	0.226***	0.144**
	(0.05)	(0.04)	(0.05)
***Treatment**	-0.186*	-0.182**	-0.198**
	(0.08)	(0.09)	(0.07)
Illness, HH **head**	-0.032	-0.13*	0.028
	(0.02)	(0.04)	(0.02)
***Treatment**	-0.010	0.089	0.007
	(0.03)	(0.06)	(0.04)

	Food consumption	Nonfood consumption	Total consumption
One shock	-0.046**	-0.112***	-0.065***
	(0.02)	(0.03)	(0.02)
***Treatment**	0.031*	0.061*	0.038*
	(0.01)	(0.04)	(0.02)
Two shocks	-0.046**	-0.135***	-0.095***
	(0.02)	(0.03)	(0.02)
***Treatment**	0.021*	0.028	0.058
	(0.01)	(0.04)	(0.09)
Three shocks	-0.040**	-0.149***	-0.105***
	(0.02)	(0.03)	(0.02)
***Treatment**	0.010*	0.061	0.049
	(0.01)	(0.04)	(0. 13)

Notes: The measure of consumption is its adult equivalent value in units of 2002 pesos. Estimations are marginal effects of the control variables of interest, ie. Shock events. Robust standard errors, clustered at the municipality level, are in parentheses. Additional repressors included but not reported: household age and education, household composition by age and sex, number of household members active in the labor market, if the house is owned, if household members work in agriculture, and municipality characteristics such as population, number of schools and health centers, if urban. Total number of observations is 6519.Municipality-year effects included. Each individual coefficient is statistically significant at the *10%, **5%, or ***1% level.

Considering shocks one at a time, it is evident that the null hypothesis of perfect risk sharing is rejected for crop loss, unemployment and illness of the household head. Crop loss will reduce per capita food consumption by 8% and total consumption by 6%, while unemployment will reduce food consumption by 15% and nonfood consumption by

28%.[21] Illness of the household head reduces nonfood consumption by 13% and doesn´t have a significant effect on food consumption. Death of a household member increase nonfood and total consumption with respect to households that have no shocks, so there is no evidence of consumption smoothing for this shock. The increase in nonfood consumption is explained by the fact that these shocks usually increase funeral expenditures.[22]

The role of FA as an instrument for consumption insurance is also evaluated. Being a beneficiary of the FA program would protect the household's food consumption when it experiences a crop loss but not unemployment of the household head. That is, while control households reduce food consumption by 8% when they have a crop loss, treatment households reduce food consumption by 2%. It is interesting to see that treatment households are no better insured against unemployment than control households as the estimated coefficient is not statistically different from zero. Negative estimations of death shocks for treatment households indicate that, while control households increase non food consumption after these shocks, treatment households are better able to buffer them. One explanation is that treatment households might have available less costly ex-ante self-insurance strategies than control households. For example, it is possible that the FA cash transfer works also as an income-smoothing mechanism for treatment households.

[21] The high coefficients of job loss could be a consequence of the potential endogeneity of this variable in the consumption equation. It could be expected that unemployment is correlated with unobservable characteristics of the household to explain consumption.
[22] Nonfood consumption is the sum of health, clothing, and miscellaneous expenditures.

Finally, we measure the effect of having one shock and having more than one shock at a time.[23] Having a negative shock reduces food consumption by 4% and non food consumption by 11%, and the program partially protects food and non food consumption. Having two or more shocks reduces food by 4% and non food consumption by 14% and the program FA partially protects food consumption but non food consumption. There are not big differences in consumption changes between having two or more shocks.

Covariate Shocks

In order to capture the covariate nature of weather and violence shocks, we use the proportion of households within a municipality reporting to have suffered each shock as environmental and violence shock variables. Also, we use an alternative measure for violence: the number of terrorist attacks that municipalities have had during the year before the interview.

To examine the degree of consumption smoothing of individual households with respect to covariate risk, we remove the municipality-year fixed effects from the estimation to calculate the following equation:

$$C_{ivt} = \alpha_0 + \alpha_1^C S_{ivt} + \alpha_2^C FA_{vt} * S_{ivt} + \alpha_3^C FA_{vt} + \beta X_{ivt} + \delta_i + \gamma_v + \mu_t + \varepsilon_{ivt} \qquad (2)$$

The model of full risk sharing predicts that local risk-sharing arrangements permit households to efficiently pool the idiosyncratic variation within communities, but they can do little to help households deal with covariate risk. .. Therefore, we should expect $\alpha_1^C = 1$, under a Pareto efficient model, or an estimate of $0 < \alpha_1^C < 1$ if households are

[23] We use a set of dummy variables that takes the value of one for households that have one shock, two shocks and three or more shocks during the same year.

able to smooth at least some part of community shocks by formal or informal insurance mechanisms.

As before, we consider the following dependent variables: food consumption, nonfood consumption, and total consumption. The covariate shocks considered are: (i) violence and (ii) weather shocks. For estimation, we use fixed effects regression with robust standard errors clustering at a municipality level. All regressions control for the same exogenous variables included in equation (1).

Results are presented in Table 6. As we observe, violence does not affect consumption. This is reasonable if we assume that most of the terrorist attacks are targeted at institutions such as banks, police stations, government offices, or to the army and not to civilians. Weather shocks seem to have a very small effect on consumption, decreasing nonfood consumption by 0.91% in control communities and by only 0.71% in treatment communities. Results are opposite to economic predictions, under which we should expect a positive and significant effect from covariate shocks on consumption, with estimations close to one. However, these results can be explained by the fact that they are not permanent but transitory shocks. In fact, Colombia had no severe long-term weather shocks during 2002–2005.

Table 6. Impact of Covariate Shocks on Consumption: fixed effects regression

	Food consumption	**Nonfood consumption**	**Total consumption**
Violence	-0.023	-0.009	-0.011
	(0.01)	(0.01)	(0.01)
***Treatment**	0.006	0.005	0.001
	(0.01)	(0.02)	(0.01)
Weather shocks	0.000	-0.009***	-0.004
	(0.00)	(0.00)	(0.00)
***Treatment**	0.005*	0.002*	0.003
	(0.00)	(0.00)	(0.00)

Notes: The measure of consumption is its adult equivalent value in units of 2002 pesos. Estimations are marginal effects of the control variables of interest, ie. community shock events. Robust standard errors, clustered at the municipality level, are in parentheses. Additional repressors included but not reported: household age and education, household composition by age and sex, number of household members active in the labor market, if the house is owned, if household members work in agriculture, and municipality characteristics such as population, number of schools and health centers, if urban. Municipality-year effects included. Total number of observations is 6519.Each individual coefficient is statistically significant at the *10%, **5%, or ***1% level.

Consumption Smoothing Against Idiosyncratic Income Change

Most of the empirical studies (Skoufias, 2003; Townsend, 1994; Ravallion & Chaudhuri, 1997) have tested the hypothesis of full risk sharing using changes on household income as a measure of shocks. Using income growth instead of negative shocks dummy variables has the advantage that income has the same time frame as consumption. In the section above, the reference period of consumption (the month before the survey) can differ from the period of shocks (year prior to the survey).

We estimate equation (3) using a fixed effects regression and DID matching regression in order to control for potential endogeneity of program participation on the consumption equation.[24] In this specification we use consumption growth per adult equivalent in constant values as a dependent variable and income growth per adult equivalent in constant values as independent variables. Since declared income might be endogenous in our specifications, we use lagged income as instrumental variables of income.[25] Municipality-time fixed effects are replaced by a set of binary variables D identifying each community separately by survey round (round and community interaction terms). Including the community/round interaction dummies have the purpose of controlling for aggregate shocks insured at the community level.

$$\Delta C_{ivt} = \alpha_0 + \alpha_1 \Delta Y_{ivt} + \alpha_2 FA_{vt} * \Delta Y_{it} + \alpha_3 FA_{vt} + \beta X_{ivt} + \sum_{tv} \delta_{tv} D_{tv} + \varepsilon_{ivt} \quad (3)$$

Results from this specification will reveal the average degree of consumption insurance in the community to any change in the household's income. As before, under full risk sharing we expect $\alpha_1 = 0$, but if α_1 is positive and significant, it provides an estimate of the partial correlation between income and consumption growth in control municipalities. If FA helps beneficiary households to cope with income shocks, we should expect a significantly negative estimate of α_2, and the sum $\alpha_1 + \alpha_2$ will provide an estimate of the partial correlation between income and consumption growth in the treatment municipalities. The measure of consumption insurance adopted under this specification can be interpreted as a partial insurance parameter, where lower estimated

[24] Using DID matching gave us an advantage over the small number of studies that have tried to identify the impact of cash transfer programs on consumption insurance. As Skoufias (2003) remarks, "the absence of any reliable consumption data in treatment and control villages before the implementation of *Progresa* prevent one from applying the difference-in-differences estimator for the evaluation of the impact of PROGRESA on consumption insurance" (pp.638).
[25] We include income from 1999 to 2001 as instrumental variables of observed income in 2002.

values of α_1 suggest a high degree of consumption insurance and thus a lower vulnerability of consumption to income shocks (Amin et al., 2003).

In order to correct any pretreatment differences remaining from the quasi-experimental design used to select the sample of treatment and control municipalities, we also use a difference in difference matching estimator[26] (also called conditional matching) to estimate the effects of the program in consumption for households with income shocks. . Matching involves pairing treatment and comparison units that are similar in terms of their observable characteristics, and a DID estimator compares the conditional before/after outcomes of participants with those of nonparticipants, allowing for unobservable but temporally invariant differences in outcomes between participants and nonparticipants. Thus, the DID matching estimator extends the conventional DID estimator by defining outcomes conditional on the propensity score and using nonparametric matching methods to construct the differences. DID matching is superior to DID as it does not impose linear functional form restrictions in estimating the conditional expectation of the outcome variable (Smith & Todd, 2005).

For matching, we use non-parametric kernel propensity score matching with replacement to find the best counterfactual sample, and then estimate the difference in difference equation. We use the Imben´s variance matrix to estimate the statistically significance of the estimated ATT. Finally, we restrict the analysis to individuals in the common support[27] in order to minimize any bias due to extrapolation within the parametric specification and reweight the observations according to the weighting

[26] DID matching was first suggested by Heckman et al. (1998a). It extends the conventional DID estimator by defining outcomes conditional on the propensity score and using semiparametric methods to construct the differences.
[27] The region over which treated individuals have a counterpart in the group of controls

function of the matching estimator. We also estimate the bias-corrected matching estimator proposed by Imbens (2004) which adjusts the difference within the matches for the differences in their covariate values. Finally, since our treatment households are those eligible on the program, our estimations represent intent to treat effect of the program on the treated (ITT). However, we expect this is very close to the average treatment effect of the program (ATT) as non-compliance was mainly due to lack of required documents of the households.

Estimations of equation (3) using fixed effects regression are reported in Table 7 and in Table 8 using DiD Matching regression. Results show that, when we measure shocks as changes in income, matching is required in order to control for potential differences between treatment and control households on consumption. The estimates presented in Table 7 column (1) show that a 10% drop in real income is accompanied by a 1.8% drop in household total consumption, a slightly lower (1.7%) decrease in food consumption, and a higher (2%) drop in nonfood consumption. However, the insignificant coefficients of the interaction of income changes with the dummy variable identifying beneficiary households of FA suggest that there are no significant differences in the level of consumption insurance between control and treatment households. The effect of the program is better identified using matching methods. Results in Table 8 show that, controlling for pretreatment differences, FA partially insures food consumption but not nonfood consumption. Unbiased estimates of the impact of the program on consumption insurance improve our previous estimates and are robust with estimations using dummy variables for idiosyncratic shocks.

The same regression was estimated using percentage change in labor income as an explanatory variable. We should expect a higher degree of consumption insurance with respect to changes in labor income than with respect to changes in total income since labor income is already insured.[28] In fact, we observe in Table 7 and Table 8 that consumption insurance is higher for labor income than for total income. The estimates for food consumption indicate that a 10% decrease in labor income will reduce total consumption by 0.9%, with no differences between food and nonfood consumption. Estimations using matching methods are very similar to estimations using fixed effect regression.

Table 7. Fixed Effects Regression: Impact of Household Income Changes in Household Consumption

	Food consumption	**Nonfood consumption**	**Total consumption**
	b/se	*b/se*	*b/se*
Δ(Ln total income)	0.172***	0.201***	0.188***
	(0.02)	(0.02)	(0.01)
***Treatment**	-0.016	-0.042	-0.027
	(0.02)	(0.03)	(0.02)
Δ(Ln labor income)	0.097***	0.090***	0.095***
	(0.01)	(0.02)	(0.01)
***Treatment**	-0.016	0.019	0.000
	(0.02)	(0.02)	(0.02)

Notes: The measure of consumption is its adult equivalent value in units of 2002 pesos. Estimations are the marginal effects of being a FA beneficiary and having an income shock. Robust standard errors, clustered

[28] Labor contracts are income-smoothing mechanisms that might reduce risk and the effects of negative income shocks.

at the municipality level, are in parentheses. Additional repressors included but not reported: household age and education, household composition by age and sex, number of household members active in the labor market, if the house is owned, if household members work in agriculture, and municipality characteristics such as population, number of schools and health centers, if urban. Municipality-year effects included. Total number of observations is 6519.Each individual coefficient is statistically significant at the *10%, **5%, or ***1% level

Table 8. DID Matching Estimations: Impact of Household Income Changes in Household Consumption Controlling for Pretreatment Effects

	Food consumption	**Nonfood consumption**	**Total consumption**
	b/se	*b/se*	*b/se*
Δ(Ln total income)	0.237***	0.257***	0.234***
	(0.02)	(0.02)	(0.01)
***Treatment**	-0.139***	-0.008	-0.154
	(0.03)	(0.03)	(0.05)
Δ(Average Ln labor income)	**0.094*****	**0.093*****	**0.095*****
	(0.01)	(0.02)	(0.01)
***Treatment**	-0.031	-0.021	0.000
	(0.02)	(0.02)	(0.02)

Notes: The measure of consumption is its adult equivalent value in units of 2002 pesos. Estimations are the ATT on consumption of being a FA beneficiary controlling for household income shocks. Robust standard errors, clustered at the municipality level, are in parentheses. Additional repressors included but not reported: household age and education, household composition by age and sex, number of household members active in the labor market, if the house is owned, if household members work in agriculture, and municipality characteristics such as population, number of schools and health centers, if urban. Municipality-year effects included. Total number of observations is 6519.Each individual coefficient is statistically significant at the *10%, **5%, or ***1% level.

Risk Pooling at the Community Level

Finally, we investigate whether risk sharing is in fact taking place among households within the same insurance community by eliminating the municipality-year fixed effects from equation (3) and including the average income growth of each municipality as a right hand side variable, as suggested by Deaton (1997) and Ravallion and Chaudhuri (1997).

$$\Delta C_{ivt} = \alpha_0 + \gamma_1 \Delta\overline{Y_{vt}} + \gamma_2 FA * \Delta\overline{Y_{vt}} + \beta X_{ivt} + \varepsilon_{ivt} \quad (4)$$

Under this model, the growth rate in household consumption is determined by the growth rate in household income as well as the growth rate in average community income. According to the hypothesis of full risk sharing, $\gamma_1 = 1$, so individual consumption is not protected from aggregate income shocks. Under imperfect risk sharing, evidence that the growth rate in average community income has a significant role in the growth rate of household consumption is consistent with the hypothesis that some risk sharing is taking place within the communities. We test also if the degree of insurance provided by the community is affected by the presence of the FA program. A positive coefficient would be interpreted as the program's increasing risk pooling in the community; the opposite would be concluded for a negative coefficient. For estimation we control for household income changes and for other household and municipality characteristics.

Table 9. Impact of Average Community Income Changes in Household Consumption

	Food consumption	Nonfood consumption
Fixed effects regression	*b/se*	*b/se*
Δ(Average Ln income, municipality)	0.630*	0.386
	(0.27)	(0.59)
***Treatment**	-0.608	-0.929
	(0.69)	(1.12)
ATT: DiD matching estimates		
Δ(Average Ln income, municipality)	0.560**	0.386
	(0.27)	(0.59)
***Treatment**	0.039	-0.008
	(0.03)	(0.03)

Notes: The measure of consumption and income is its adult equivalent value in units of 2002 pesos. Robust standard errors, clustered at the municipality level, are in parentheses. Additional repressors included but not reported: household age and education, household composition by age and sex, number of household members active in the labor market, if the house is owned, if household members work in agriculture, and municipality characteristics such as population, number of schools and health centers, if urban. Municipality-year effects included. Total number of observations is 6519.Each individual coefficient is statistically significant at the *10%, **5%, or ***1% level

The estimated coefficients of the growth rate in average community income, the parameters γ_1 and γ_2, are reported in Table 9. The estimates provide evidence in favor of community risk sharing in food consumption but not in nonfood consumption. Also, no significant differences are found regarding the effect of mean community growth rate between treatment and control households. As changes in average community income reflect covariate income shocks, results show that households are less insured to covariate income shocks than to idiosyncratic income shocks, 37% and 80%, respectively.

Nonetheless, these results are opposed to our findings above when using measures of weather and violence shocks. As we said before, it is possible that our measures of community shocks are not the best or that they reflect community transitory shocks instead of permanent shocks. In order to check our results, we estimate the partial insurance model proposed by Blundell et al. (2006) and calculate the partial insurance parameters for permanent and transitory shocks using the same data. Results are very similar to our estimations above. Details of the methodology and results are presented in the next section.

Partial Insurance Model

Based on Blundell, Pistaferri, and Preston (2008), we estimate the degree of partial insurance for transitory and permanent shocks for different households' characteristics: (i) household head education level, (ii) urban and rural households, (iii) single parent and biparental households, and (iv) FA beneficiary and control households. In this model, partial insurance is defined as smoothing mechanisms—other than personal savings and borrowings—to smooth consumption changes when incomes are shifted by permanent or transitory shocks. These mechanisms could help us understand the lower volatility of consumption in relation to the volatility of income and introduce a method to measure the impact of different-smoothing tools (Casado, 2009). This model has less assumptions and it is more complete than the full insurance model in the sense that it examines the roles of asymmetric information, moral hazard, and heterogeneity and shows how the complete markets model must be amended to include some forms of imperfect insurance.

This analysis of partial insurance requires the study of income and consumption process and its relationship to transitory and permanent income shocks. In this model, the relationship between income shocks and consumption depends on the degree of persistence of income, and we expect to uncover less insurance for more persistent shocks. Blundell and Preston (1998) derive the conditions under which the growth of variance and covariance of income and consumption can be used to separately identify the growth in variance of permanent and transitory income shocks. Blundell, Pistaferri, and Preston (2008) describe the transmission of income inequality into consumption inequality and derive the transitory and permanent partial insurance parameters.

It is supposed that income has the following equation:

$$\log Y_{i,t} = Z'_{i,t}\varphi_t + P_{i,t} + v_{i,t}$$

where Y is real income and Z a set of control variables (such as education of the household head and number of household members, among others). $P_{i,t}$ is the permanent income component, and $v_{i,t}$ stands for transitory income.

Assuming a random walk for $P_{i,t}$ ($P_{i,t} = P_{i,t-1} + \zeta_{i,t}$) and a martingale process MA(q) for $v_{i,t}$ ($v_{i,t} = \sum_{j=0}^{q} \theta_j \varepsilon_{i,t-j}$), the difference of the unpredicted income can be written as:

$$\Delta y_{i,t} = \zeta_{i,t} + \Delta v_{i,t} \quad \text{where} \quad \Delta\, y_{i,t} = \Delta LnY_{i,t} - \Delta Z'_{i,t}\varphi_t$$

The Euler equation with CRRA preferences and complete credit markets is:

$$C_{i,t-1}^{\beta-1} = \frac{1 + r_{t-1}}{1+\delta} e^{\Delta Z'_{i,t}\varphi_t} E_{t-1} C_{i,t}^{\beta-1}$$

Computing the mapping from the income shocks $\zeta_{i,t}$ and $\varepsilon_{i,t}$ to the optimal consumption growth following estimations by Blundell et al. (2008), and assuming that personal saving is the only mechanism available to smooth consumption, we obtain the consumption growth equation:

$$\Delta c_{i,t} = \phi_{i,t}\zeta_{i,t} + \psi_{i,t}\varepsilon_{i,t} + \xi_{i,t}$$

where ϕ is the loading factor of permanent shocks and ψ of transitory shocks and where ξ represents innovations in consumption independent of those from income. The moments required to compute the partial insurance parameters were estimated using diagonally weighted minimum distance (DWMD).

Following Meghir and Pistaferri (2004, cited in Blundell et al., 2008) we identify the parameters of interest ψ and ϕ for transitory and permanent shocks in income. Following this approach, ψ and ϕ can be understood as the instrumental variable estimation of Δc_t on Δy_t using $(\Delta y_{t-1} + \Delta y_t + \Delta y_{t+1})$ and $E(\Delta c_t \Delta y_{t+1})$ as instruments, respectively.

$$\psi = \frac{E(\Delta c_t \Delta y_{t+1})}{E(\Delta y_t \Delta y_{t+1})} \quad \text{and} \quad \phi = \frac{E(\Delta c_t(\Delta y_{t-1} + \Delta y_t + \Delta y_{t+1})}{E(\Delta y_t(\Delta y_{t-1} + \Delta y_t + \Delta y_{t+1})}$$

Where transitory insurance parameter ψ is computed measuring the relation between income and lagged consumption, it must be correlated through the transitory component $E(\Delta c_t \Delta y_{t+1}) = \sigma_\varepsilon^2$. Similarly, we compute the covariance between current consumption and current income growth $E(\Delta c_t \Delta y_t)$, removing the contribution of the transitory component to compute the permanent income shock effect $E(\Delta c_t(\Delta y_{t-1} + \Delta y_t + \Delta y_{t+1}) = \sigma_\zeta^2$. Finally, the variance of the component σ_ξ^2 is computed like the variance of consumption growth, removing the contribution of permanent and transitory income shocks.

In order to instrument our income variable, we use retrospective data on income captured in the baseline survey for the years 1999, 2000, and 2001. We also infer income for 2004 from an income equation controlling for household and individual characteristics.

In the above representation, the case of full insurance would be $\phi = \psi = 0$, where neither transitory nor permanent shocks in income would affect consumption. The case of no insurance would be $\phi = \psi = 1$. Parameter estimations between zero and one identify the degree of transmission of income shocks into consumption. If coefficients are closer to zero, the degree of insurance will be higher. These partial insurance parameters include self-insurance (precautionary saving) and other insurance devices, but we cannot identify each insurance component by itself.

Empirical Evidence of Consumption Insurance under a Partial Insurance Model

In this section we present estimations of the partial insurance parameters for transitory and permanent income shocks. Using panel data on income and consumption, we are able to estimate the degree of partial insurance for transitory and permanent shocks for FA beneficiary and not beneficiary households.

The FA dataset doesn't show evidence of a MA(q) process for transitory shocks, so we assume they are uncorrelated ($v_{i,t} = \varepsilon_{i,t}$). Diagonally weighted minimum distance (DWMD) was used to estimate parameters because it allows for heteroskedasticity, unlike equally weighted minimum distance (EWMD). Also, we assume that insurance parameters are constant over time.

Table 10. Partial Insurance Parameters

		Food consumption		**Nonfood consumption**	
Criteria	Groups	Permanent shocks (Φ)	Transitory shocks (Ψ)	Permanent shocks (Φ)	Transitory shocks (Ψ)
Complete sample		0.42	0.18	0.50	0.17
Program FA	Control group	0.44	0.14	0.51	0.17
	Treatment group	0.37	0.16	0.51	0.16

Notes: The measure of consumption and income is its adult equivalent value, in units of 2002 pesos.

Estimations of transitory and permanent partial insurance parameters are presented in Table 10. For the full sample, a 10% permanent income shock induces a 4.2% permanent change in food consumption and a 5% change in nonfood consumption. Simultaneously, a 10% transitory income shock induces significant 1.8% transitory and permanent changes in consumption. We find higher degrees of insurance to transitory shocks than to permanent shocks. Food consumption seems to insure better from permanent shocks than nonfood consumption. The insurance coefficient of the transitory shocks is not statistically different between food and nonfood consumption, which indicates that total consumption is not fully insured against transitory shocks but that the degree of insurance is high.

The insurance against permanent shocks for the treatment group is higher than for the control group for food consumption but not for nonfood consumption. That is, as in our previous results, we observe that the program protects food consumption but not nonfood consumption of beneficiary households. The insurance capacity to permanent shocks of FA households is almost 74%, while the insurance capacity of control household is 58%.In conclusion, we observe that the partial insurance parameters for permanent and transitory shocks are very similar to the estimations of equation (3) using matching methods. Although both estimations are not directly comparable as the assumptions and methodology are different, we find similar results about the degree of consumption insurance of poor households in Colombia and the role of FA to protect consumption during income shocks.

Risk Coping Strategies and the Role of FA

Results have shown that households in rural Colombia are able to partially spread the effects of income shocks over time and that this is partially due to risk-sharing arrangements across households at the community level at any one point in time. However, we also observed that covariance between nonfood consumption and income is still pretty low and that risk pooling has been effective in smoothing only food consumption but not other consumption. Therefore, households may be adopting a variety of self-insurance strategies to spread the effects of income shocks over time. For example, they may use their savings (Paxson, 1992); take out loans from the informal financial sector (Udry, 1994); sell assets (Deaton, 1992; Rosenzweig & Wolpin, 1993); adjust their labor supply (Kochar, 1998) including sending their children to work instead of school in order to supplement income (Jacoby & Skoufias, 1997); or rely on transfers and remittances from friends and neighbors (Rosenzweig, 1988; Besley, 1995; Morduch, 1999).

In this section we examine whether the incidence of different shocks is associated with increased likelihood of using the following coping instruments: (i) increasing expenditures, (ii) using savings, (iii) incurring debts, (iv) receiving transfers from friends or relatives (v), selling assets, or (vi) increasing the labor supply of household members. Households were asked at baseline and in the first survey how they responded to these shocks. Households could select more than one instrument.

Although answers to these questions could differ from their behavioral responses, they give a glimpse of how households cope with income shocks and how FA alters these responses. We estimate the following probit model separately for each of the six coping instruments mentioned above:

$$Prob(Y_{it} = 1) = \alpha + \beta S_{it} + FA(\alpha_1 + \beta_1 S_{it}) + \emptyset X_{it} \qquad (5)$$

where *Y* equals one when the household declares it used each specific instrument to cope with shocks and where *S* is a vector of dummy variables denoting the incidence of any of the following shocks: (i) death of a household member, (ii) illness of a household member, (iii) crop loss, (iv) natural disaster, or (v) violence. *X* is a vector of household and municipality characteristics, such as the age and sex of household head and spouse, whether the household is headed by a female, the education level of the household head and spouse, binary variables for owning the house where they live, if the household works on cropping or harvesting, and age composition of the household. Municipality variables include a dummy variable for household beneficiaries of FA, for the regions of the country, for urban areas, and for the survey round. Finally, the coefficients of interest are β and β_1, where β denotes whether the incidence of a shock increases the likelihood that the dependent variable Y equals 1 and the extent to which the incidence of the same shock entails a stronger or opposing reaction in the households benefited by FA ($\beta + \beta_1$).

Table 11 presents the marginal effects of the different shock variables on the probability of adopting a specific response. Results show that, controlling for household characteristics and for any income shocks, beneficiary households seem to rely more on savings and less on transfers to smooth consumption. Crop loss is handled by reducing expenditures and receiving transfers from friends and relatives. However, it is notable that treatment households seem to reduce the likelihood of using transfers from friends and relatives as a risk coping instrument when they are hit by these shocks and increase the likelihood of using assets as a risk coping instrument.

Weather and death shocks are more likely to result in a household's receiving help from relatives, while illness shocks force households to incur new loans, probably in the informal sector at very high interest rates. However, the program has no differential effect on these self-insurance arrangements.

Results from this section suggest that the FA program might be crowding out some self-insurance instruments such as internal transfers, while reinforcing the use of savings. Attanasio and Rios-Rull (1999) have shown that, in a model of risk sharing under limited commitment, the introduction of a government insurance scheme can crowd out preexisting informal risk-sharing arrangements, resulting in a decrease in welfare for the beneficiaries. Therefore, further research should explore how different coping instruments have in fact been displaced or reinforced by FA.

Table 11. Probability of using the following Risk Coping Strategies for Idiosyncratic Income Shocks and the crowding-out effect of FA

	Reduce expenditures	Use savings	Sell assets	Internal transfers	Credit	Increase labor supply
	b/se	b/se	b/se	b/se	b/se	b/se
Treatment	-0.194	0.131*	-0.055	-0.250**	-0.030	-0.010
	(0.11)	(0.04)	(0.09)	(0.09)	(0.09)	(0.09)
Death	-0.150	0.235*	0.107	0.231*	0.026	-0.328*
	(0.12)	(0.10)	(0.11)	(0.11)	(0.13)	(0.15)
***trtmt**	0.000	-0.043	-0.192	0.166	0.115	0.348
	(0.17)	(0.17)	(0.16)	(0.16)	(0.20)	(0.21)
Illness	-0.043	0.138	-0.012	0.128	0.304***	-0.126
	(0.09)	(0.09)	(0.07)	(0.07)	(0.05)	(0.09)
***trtmt**	0.043	0.005	-0.059	0.150	0.004	0.083
	(0.12)	(0.12)	(0.10)	(0.09)	(0.08)	(0.11)
Crop loss	0.429***	-0.060	-0.138*	0.265***	0.009	0.100
	(0.08)	(0.06)	(0.06)	(0.06)	(0.05)	(0.07)
***trtmt**	-0.018	0.084	0.144*	-0.189*	0.045	0.091
	(0.13)	(0.09)	(0.05)	(0.09)	(0.09)	(0.10)
Weather	-0.055	0.046	-0.085	0.365*	-0.124	0.125
	(0.18)	(0.14)	(0.13)	(0.18)	(0.17)	(0.16)
***trtmt**	-0.051	-0.709*	0.105	-0.133	-0.325	-0.239
	(0.23)	(0.30)	(0.19)	(0.23)	(0.22)	(0.23)
Violence	-0.252*	-0.142	0.120	-0.056	-0.422*	-0.011
	(0.12)	(0.17)	(0.15)	(0.12)	(0.17)	(0.14)
***trtmt**	0.278	0.282	-0.113	0.048	0.110	-0.048
	(0.17)	(0.22)	(0.27)	(0.19)	(0.22)	(0.17)
N	5528	5522	5518	5516	5525	5519

Notes: Marginal effects of using self-insurance instruments when households have income shocks. Each column represents a separate regression. Robust standard errors, clustered at the municipality level, are in parentheses. Additional repressors included but not reported. Each individual coefficient is statistically significant at the *10%, **5%, or ***1% level.

Although the results reveal that the program affects the role of transfers as a risk coping instrument for treatment households, we don´t observe statistical differences between treatment and control samples on transfers to and from households, in kind, or in cash, over the three rounds of the survey. Average transfers before and after the program is presented in Table 12. As stated above, evidence here is not validated with household behavior. Further research should address the crowding out effect of FA on self-insurance instruments since the evidence provided here is not sufficient.

Table 12. Transfers in Money Received by Households

	Treatment	**Control**	**T-test (p-value)**
Baseline	347957.3	341160	0.1157
1st wave	368110.4	355374.4	0.4467
2nd wave	361998.8	355946.9	0.8422

Notes: The measures of transfers are per household in units of 2002 pesos. T-test of difference in means computed clustering at the municipality level.

Conclusion

Under our several specifications above, we reject the hypothesis of complete consumption insurance, although we observe a high level of consumption smoothing among poor households in small villages in Colombia. Results show that (i) the growth rate of consumption is related to the growth rate of income, but certainly less so than what one would expect under the alternative hypothesis of a complete lack of risk-sharing tools, suggesting that insurance is incomplete even for idiosyncratic shocks; (ii) food consumption is better insured than nonfood consumption; (iii) risk-pooling mechanisms at the community level insure food consumption but not nonfood consumption;; and (iv) household consumption growth is much more respondent to changes in aggregate municipality consumption than to changes in household income. Overall results are robust to different specifications.

This study has also analyzed the impact of a CCT program on the ability of households to smooth consumption when faced with negative shocks. Results suggest that a CCT programs, specifically FA, is effective as a risk-management instrument. Overall, beneficiary households of FA appear to have lower absolute changes in consumption than control households when subject to idiosyncratic shocks, and there is no effect of the program on risk pooling within communities. Results show that beneficiary households of the program are able to protect food consumption from shocks such as crop loss and also to safeguard nonfood consumption when faced with the death of a household member. The program has not been effective in insuring unemployment and illness shocks. In sum, Familias en Acción, despite not being a consumption insurance program, helps treated families to smooth consumption, working as a social insurance program. .

Widely known theories of risk coping strategies in the literature, like the Pareto full risk sharing hypothesis and the permanent income hypothesis, are clearly rejected by the data, giving support to partial insurance models. Estimation of the partial insurance parameters for transitory and permanent shocks reinforces some of our findings under the complete markets assumption. On average, households are able to self-insure consumption against transitory shocks by approximately 83% and against permanent shocks by about 45%. FA works as insurance for permanent shocks for food consumption but not for nonfood consumption. Insurance parameters under the partial insurance model show a higher degree of insurance than under the full risk sharing model as it allows for self-insurance other than own savings.

Results raise questions about the precise mechanism by which poor households in Colombia cope with risk. That is, households do not rely exclusively on risk-sharing arrangements; instead, they appear to complement informal risk-sharing strategies with self-insurance strategies. Thus, the next step in this research project is to test how costly self-insurance strategies have been displaced by the program. More precisely, we will investigate whether the conditional aspect of the program prevents parents from using their children as risk coping instruments in response to shocks.

Chapter 2 . THE EFFECTS OF INCOME SHOCKS ON CHILD LABOR AS AN INSURANCE MECHANISM FOR SCHOOLING

Introduction

This work relates to important literature on consumption smoothing, credit constraints, and the role of public interventions on a country's safety net. Poor people in developing countries are highly exposed to idiosyncratic shocks that cause loss of income or unexpected expenditures of the household. However, it has been extensively documented (Morduch, 1994, and others) that poor households succeed in smoothing their consumption profiles, even with limited access to credit; hence they are likely to resort to mechanisms other than borrowing to cope with income shocks. This paper examines one such mechanism, child labor. If households use child labor to cope with shocks, the costs of uninsured shocks can be quite high in terms of human capital accumulation and poverty. Therefore, public interventions that alleviate the need for households to use their children as risk-coping instruments might play an important role on the safety net of the uninsured (de Janvry, Finan, Sadoulet, & Vakis, 2006).

Conditional cash transfer programs (CCTs) have been proven to be effective in increasing educational achievements and reducing child labor (Attanasio, Fitzsimons, Gomez, Lopez, Meghir, & Santiago, 2006; Schultz, 2004). A growing body of evidence has also shown that CCTs can play an important safety net role, protecting household consumption during an income shock and helping to mitigate the negative effects of a shock on children's human capital investments (de Janvry, Finan, Sadoulet, & Vakis 2006; Maluccio 2005 and others). In a previous chapter of this dissertation, we found that consumption smoothing of poor households in Colombia is relatively high,[29] and that households are adopting a variety of self-insurance strategies to spread the effects of income shocks over time. We also found that the CCT program Familias en Accion (FA) might be crowding out some self-insurance instruments, such as internal transfers, while reinforcing the use of savings. Using declared households' responses, we found no evidence of increasing household labor supply to buffer income shocks. In this paper, we go further, testing whether child labor is used as a risk-coping instrument and whether FA can crowd out this specific behavior, resulting in increased welfare of those involved.

Particularly, this paper investigates the effects of adverse shocks on household decisions concerning school attendance and child labor in rural Colombia and whether FA protects children's school enrollment of households under risk. Based on theoretical foundations (Jacoby & Skoufias, 1997), we expect that households would underinvest in the human capital of their children under imperfect credit markets and exposure to shocks. Also, we might expect CCT programs to protect child human capital from being used as a risk-coping instrument. If the effect of the condition on school attendance, acting as a price effect, is much larger than the income effect of the shock, then CCTs can be an efficient way of providing risk coping while protecting children's education. Therefore, the effect of shocks, access to credit, and CCTs on child labor and schooling decisions is ambiguous. This is what we address in this paper.

The empirical models in this study estimate equations for children's time at school and at work and for participation in school full time, work full time, and combined school and work. For this we use the FA evaluation survey, which includes longitudinal

[29] We found that poor households are able to insure at least 80% of food consumption when they face adverse income shocks.

detailed data on children's time use and adverse shocks along with individual, household, and community characteristics for beneficiary and control households of the program. Because FA was not randomly assigned among communities,[30] we use difference-in-difference (DiD) matching methods to control for endogeneity on program participation and unobservable time invariant effects.

In this paper new ground is explored by explicitly incorporating the interactive effects of shocks, credit access, and CCT treatment on school and child labor hours. Previous studies have limited their analysis to estimate the effect of shocks and the mitigating effect of CCT only on schooling or child labor (de Janvry et al. 2006; Maluccio 2005 and others), not considering that children can work and study simultaneously. Given the condition on school attendance together with the fact that time at school and at work are not perfect substitutes, more relevant effects should be found on children that perform both activities. Knowing how hours on each activity are affected by shocks and how the program mitigates these effects is very important to understand the overall well-being of children.

The results of this research suggest that (i) poor rural households use child labor to cope with income shocks, but at the expense of leisure or studying time of children, not at the expense of school enrollment; (ii) shocks have heterogeneous effects on children by age and sex; (iii) the relationship between economic shocks and school enrollment is ambiguous, as different types of shocks are likely the product of heterogeneous impacts; (iv) credit-constrained households have a higher probability of using child labor as a risk-

[30] In fact the program was controlled randomly assigned. That is, control municipalities were selected to be as similar as possible to the randomly selected treatment municipalities.

coping instrument; and (v) the effect of shocks should be estimated at intensive and extensive margins to account for complementarities of schooling and working activities.

With respect to the safety net value of FA, we find that the program consistently reduces the number of hours of child labor and increases the number of study hours, but not school attendance, for households under shocks. Alternatively, we found that the program provides strong incentives for children to combine work and school when households are exposed to shocks. The results of this paper reveal that CCT programs can work as insurance for the schooling of the poor but are not able to completely displace costly risk-coping strategies. However, the final well-being of children is still in question.

The remainder of this paper is as follows. The next section provides an overview of the program and a description of the evaluation sample used for the empirical analysis. Section 3 reviews previous studies on the effects of CCT programs and income shocks on school enrollment and child labor and describes a theoretical model that predicts the impacts of shocks on school and child labor. Section 4 describes the data used in this study and provides empirical evidence on child labor, the occurrence of shocks, and control variables including the access to credit among poor households in Colombia. Section 5 gives the methodology and econometric models for school attendance and work decisions in the presence of shocks. Results are presented in section 6, and section 7 concludes.

Familias en Acción

The program Familias en Acción is a welfare program run by the Colombian government to foster the accumulation of human capital in rural Colombia. It is similar to other CCT programs, such as Progresa, in Mexico (now called Oportunidades); Red de

Proteccion Social, in Nicaragua; and Bolsa Familia, in Brazil, that have been implemented in middle-income countries during the last decade in an effort to break the intergenerational transmission of poverty. The FA program is aimed primarily at improving the education, health, and nutrition of poor families. The nutrition component consists of a basic monetary supplement that is given to all beneficiary families with children under seven years of age. The health component consists of vaccinations and growth and development checks for children, as well as courses on nutrition, hygiene, and contraception for their mothers. Participation in the health component is a precondition for receiving the benefits of the nutritional component. All children between 7 and 18 years old are eligible for the educational component. To receive the grant, they must attend classes during at least 85% of the school days in each school month as well as during the whole academic year. The size of the grant increases for secondary education and is equal for girls and boys. The amount of the subsidy on a monthly basis at the time of the baseline survey was 14,000 Colombian pesos (COP) or (US$6) for each child attending primary school and COP$28,000 or (US$12) for each child attending secondary school in 2005. The nutritional supplement[31] is paid to families with children aged between 0 and 6 years. The amount is COP$46,500 or (US$20) per family per month. The average transfer received per household is COP$61,500, which represents approximately 25% of average household income of beneficiary households. In general, all the transfers are received by the female head of the household every two months.

[31] This subsidy is an alternative to participation in a pre-existing program called Hogares Comunitarios. Beneficiaries cannot participate in both programs with the same children. However, families with children both under and over the age of 6 can choose to send the young children to a Hogar Comunitario and to participate in FA with the older children.

Familias en Acción determined household eligibility in two stages: first by identifying target communities and then by choosing low-income households within those communities. Selection criteria for target communities were based on the following conditions. The town must: (i) have fewer than 100,000 inhabitants and not be a departmental capital, (ii) have sufficient education and health infrastructures, (iii) have a bank, and (iv) have a municipality administrative office with relatively up-to-date welfare lists and other official documents deemed important. A subset of 622 of the 1,060 Colombian municipalities qualified for the program. Eligible households were those registered at SISBEN[32] level 1 at the end of December 1999, with children under 17 years old, living in the target municipalities. SISBEN 1 households account for roughly the lowest quintile of Colombia's household income distribution (Attanasio, 2004).

The program started operating in the latter half of 2002.[33] It has benefited approximately 1,500,000 households since its beginning, and the cost has ascended to the sum of 300 thousands of millions of Colombian pesos annually (US$150 million). The cost of the program corresponds to the 0.5% of the Colombian GDP and represents approximately 10% of educational expenditures in the country.

The Evaluation Sample

For evaluation purposes, it was decided to construct a representative stratified sample of treatment municipalities and to choose control municipalities among those that were excluded from the program but that belonged within the same strata. The strata were determined by region and by an index of infrastructure based on health and education. The control towns were chosen within the same stratum to be as similar as possible to

[32] SISBEN, Sistema Unificado de Beneficiarios, is a six-level poverty indicator used in Colombia to target welfare programs and for the pricing of utilities.
[33] In a few municipalities the program started as early as the end of 2001.

each of the treatment towns, in terms of population, area, and quality of life index. Most of the control municipalities were towns with basic school and health infrastructures but without banks or, in the few cases chosen to match relatively large municipalities, just over 100,000 inhabitants. As a consequence, control towns are broadly comparable to treatment towns (Attanasio, 2004). In the end, the evaluation sample was made up of 122 municipalities, 57 of which were treatment and 65 of which were controls.

For each municipality, approximately 100 eligible households were included in the evaluation sample. The total evaluation sample consists of 11,462 households interviewed between June and October 2002 (baseline survey), 10,742 households interviewed between July and November 2003 (first wave), and 9,566 households interviewed between November 2005 and April 2006 (second wave). The attrition rate between the three rounds was approximately 16%.[34] Most of the observations lost were households which children´ age exceeded the required age or households that move from their location and were no possible to find again. The final longitudinal data used in this study include information from 6,519 repeated households, after excluding households that received payments before the baseline survey and households located in control municipalities that received payments during the second survey.[35] As compliance was very high,[36] more than 97%, we include in the sample all observations from treatment municipalities.

[34] According to Attanasio (2007) attrition between baseline survey and the second follow up survey is not statistically different between treatment and control households. Therefore we assume that lost of observations is random.
[35] A total of 13 municipalities of the control sample were converted to treatment municipalities in 2005, before the second wave of the evaluation survey.
[36] Reasons for no compliance were due mainly to lack of the required documents and not to lack of interest of participation in the program.

At the household level, the sample consists of families that are potential beneficiaries of the program—that is, households with children from the poorest sector of society. Data are collected at both the household and the individual level. The available data provide a rich set of variables that allows us to measure consumption of durables and non-durables, family composition, household socio-demographic structure, labor supply, nutritional status of children, education, household assets, income, and different shocks to income, for both rural and urban households.

Literature Review and Conceptual Framework

A number of studies have measured the impact of uninsured shocks on school attendance and child labor outcomes, suggesting that parents may be forced to draw on their children as labor when other strategies are not available (Beegle, Dehejia, & Gatti, 2006; Duryea, Lam, & Levison, 2003; Guarcello, Mealli, & Rosati, 2003; Jacoby & Skoufias, 1997). Duryea et al. (2003), using data from Brazil, showed that unemployment shocks significantly increase the probability that a child enters the labor force and decreases the probability that the child advances in school. Guarcello et al. (2003) observed a similar response for households in Guatemala. They showed that a negative shock substantially increases the probability that a child works, or works and studies simultaneously, and that access to credit and medical insurance provides risk-coping instruments that help protect children from dropping out of school. Jacoby and Skoufias (1997) developed a model of human capital accumulation under uncertainty with complete and incomplete markets. Using data from International Crops Research Institute for the Semi-Arid Tropics (ICRISAT) set on a panel of Indian rural households, they examined whether fluctuations in family income affect school attendance in the face of financial market incompleteness. They found that child school attendance does decline when poor households are hit by a shock and that school attendance is more responsive to aggregate than idiosyncratic shocks. Finally, Beegle et al. (2006), using data from a household panel survey in Tanzania, investigated the extent to which transitory income shocks lead to increases in child labor and found that household asset holdings mitigate the effects of these shocks.

During the last decade, many have analyzed the role of CCT programs on children's school enrollment and work decisions. Schultz (2004) showed that the Progresa program in Mexico has a positive effect on schooling and helps to reduce child work, particularly for boys, while girls are able to combine school and domestic work. An analysis of the program Bolsa Escola in Brazil found that it has a big impact on increasing school enrollment but it has no influence on child labor (Bourguignon, Ferreira, & Leite, 2003; Cardoso and Souza, 2004). In most cases, there is evidence that the effect of a CCT is much stronger on increasing school enrollment than reducing child labor. However, these studies ignore intensity of work activity, which is clearly very important from a welfare perspective.

Attanasio et al. (2006) provided estimates of the effect of the FA conditional transfer on education and work choices, at both extensive and intensive margins. At the extensive margin they found that FA has a positive effect on school enrollment, especially in older children, a negative effect on domestic work for young children, and a neutral effect on income-generating work. At intensive margin they found the same results, but the magnitudes of the impacts are very different across groups. For instance, the effects on schooling are most pronounced for younger children, yet the increased time at school is not wholly substituted by reduced time at work but by domestic work. Time spent at income-generating activities does not change significantly after the program for any of the groups.

Two works in the conditional cash transfer literature by de Janvry et al. (2006) and Maluccio (2005) evaluated the relationship between economic shocks and the impacts of CCTs on school enrollment. Researchers de Janvry et al. (2006) found that Progresa fully protects children's schooling from the shocks of unemployment and illness of the household head, but natural disasters in the community but does not prevent children from working more when their households are hit by a shock. Maluccio (2005) showed that the Nicaraguan Red de Protección Social protected household's total food expenses and children's school attendance against the effect of the Central America coffee crisis in 2000–2001. Overall, the literature suggests that shocks reduce school enrollment and increase child labor, while CCTs have the reverse effect.

The connection between negative income shocks and decreased schooling when there are credit constraints has been established in a theoretical model by Jacoby and Skoufias (1997). The empirical equations used in this research are based on predictions and formulations of this model. Jacoby and Skoufias' model assumes that households maximize a utility function defined over current consumption and future children's consumption. Parents supply labor inelastically, and their returns are used to finance current consumption. Children's time can be used either to further increase current consumption through work, to accumulate human capital, or for leisure. Human capital determines children's future consumption. The household can change the intertemporal allocation of consumption by changing the children's labor supply. The presence of credit rationing restricts the budget of the household and, if binding, will inefficiently generate a low level of investment in human capital. Moreover, household income net of children's contribution is not certain but rather is subject to shocks. If capital markets were complete, the realization of such shocks would not affect children's labor supply, as they would be insured (Guarcello et al., 2003).

This model (Jacoby & Skoufias, 1997) predicts four possible outcomes for children's activities: three corner solutions and one internal solution. A child can attend school full time, work full time, do neither, or combine work and school. The decision of a household concerning the activities of its children will be guided by an unobservable utility as a function of a set of household characteristics including household expected or permanent income net of children's contribution, a set of proxies for the rate of returns to child work and for cost and returns to schooling, and a set of variables relating to credit rationing, access to public or private insurance mechanisms, and realized shocks (Guarcello et al., 2003). The model suggests a linear relationship between shocks and school enrollment. This result is used to motivate an econometric estimation structure with linear relationships between shocks, schooling, and program impacts, as presented in section 5.

Child Labor, Shocks, and the Access to Credit Control Variable

The data of this study come from the FA evaluation survey, which was designed especially for the purposes of evaluating the impact of the program. The survey collected information on individuals and households located in treatment and control municipalities between 2002 and 2005: the baseline survey was in 2002 (before implementation of the program), the first round was in 2003, and the second round was in 2005. The balanced panel dataset has information of a total of 30,985 individuals, 13,737 children under 18 years old, and 6,519 households.[37]

This dataset has several features that make it particularly appropriate for the empirical analysis in this research. First, the detailed household survey has ample individual and household characteristics, including information on time use of all household members aged ten and older. This includes time spent working for wages at household businesses and non-household business and working without wages doing household chores. The survey also includes information on different household shocks as well as measures of access to credit in each of the interviews. As a consequence, the survey allowed the creation of a valuable balanced panel dataset to model household behavior under risk.

Child Labor

In Colombia, an estimated 2.5 million children (10.4% of the total population of children) are forced to work to support their families. Only 60% of all the children in Colombia leave school with a primary school diploma, and 87% of school-age children were enrolled in school in 2005. On average, child laborers work about seven to eight hours per day. Their wages are pitifully low, and most of them receive no health or unemployment benefits (Bernal & Cardenas, 2005).

School starts at the age of 5 in Colombia, and no significant amount of child labor is found below the age of 7. The basic cycle of education requires 9 years of study, and

[37] Households receiving payments from FA during the baseline survey were excluded, as were households without complete information on all three surveys.

other 3 years are needed to finish high school. Thus, children generally finish the basic cycle at 14 years old and complete school at 17 years old. Although legislation allows children to work legally from the age of 14, for the purposes of this study, child labor is defined as the work of children in the age range between 10 and 17 years old.

We use the time allocation data of FA survey to construct binary indicators of participation in work and school. We consider child labor as a child spending more than one hour working in economic activities or non-paid work, including household chores and work in family business.[38] Participation in school takes the value of one if the child is enrolled in school. We also measure hours of work and hours of study and include time doing homework or other school activities. We focus on four age/sex sub-samples: girls and boys 10–13 years old and 14–17 years old.

Table 13 gives detailed information on children's activities in our sample. Seventy-six percent of the children aged 10–17 years were engaged in work during the baseline survey. Most were employed in family businesses or household chores (72%). A significant proportion—64%—were reportedly working and attending school, while 19% reported attending school only and 12% reported working only. Also, we observed that between baseline and follow-up surveys participation in schooling increased and participation in child labor decreased, for both treatment and control municipalities, while percentage of children that work and study simultaneously remained more or less the same. This suggests that the children who only work are moving to school, which is the main objective of the program, but that the program doesn't prevent children from working and studying simultaneously. Table 14 shows that child labor is very

[38] The concept of child labor by International Labor Organization (ILO) standards is not restricted to only economic activities.

heterogeneous according to age and sex and urban vs. rural households. In general, child labor is most common in rural areas, in boys, and in older children of both sexes.

Table 13. Children's types of activity, participation percentages

	Baseline			Average of First and Second Rounds		
	Control	**Treatment**	**Total**	**Control**	**Treatment**	**Total**
Work only	13.22	11.15	12.37	7.04	5.92	6.58
School only	20.82	17.29	19.38	28.27	21.45	25.5
School and work	61.4	68.65	64.36	61.16	70.68	65.03
None	4.57	2.9	3.89	3.53	1.95	2.89
Observations	7,485	5,120	12,605	7,485	5,120	12,605

Table 14. Child labor by group, participation percentages

	Baseline	First Rounds
Urban	9.9	8.0
Rural	19.4	16.1
Boys	17.4	13
Girls	7.9	7.8
Ages 7–9	3.9	3.1
Ages 10–11	10.7	6.7
Ages 12–14	16.6	12.6
Ages 15–17	27.6	25.7
Observations	12605	12605

Table 15 shows the average number of hours children work and attend school. In the pooled data, children worked an average of 4 hours per day and studied an average of 4 hours per day at baseline. Hours reported for school activities increased in the following surveys by 1.5 hours, and time at work decreased by only 1/4 of an hour. Mean school hours were lower for treatment municipalities at baseline and increased after program participation. The inverse relation is observed for work hours, which decreased after treatment. It is noticeable that time at school increases for both treatment and control households, while time at work remains constant for control households. This evidence suggests that the increased time at school could be not a result of the program but of other observed or unobserved effects.

Table 15. Children's time use, number of hours by sex/age groups

	Baseline			**Average of First and Second Rounds**		
	Total	**Treatment**	**Control**	**Total**	**Treatment**	**Control**
Work	4.01	4.36	3.72	3.74	3.73	3.75
Boys 10–13	3.55	3.92	3.20	3.30	3.27	3.32
Girls 10–13	3.17	3.56	2.84	2.90	2.91	2.90
Boys 14–17	5.05	5.33	4.83	5.12	5.18	5.07
Girls 14–17	4.03	4.45	3.68	3.87	3.84	3.89
School	4.00	3.34	4.46	5.44	5.51	5.39
Boys 10–13	3.49	2.99	3.84	4.92	5.12	4.78
Girls 10–13	3.45	2.94	3.81	5.00	5.02	5.00
Boys 14–17	5.18	3.96	5.96	6.57	6.65	6.51
Girls 14–17	5.59	4.72	6.18	6.98	6.92	7.02
Observations	21,605	5,120	7,485	21,605	5,120	7,485

Shocks

The variables used to identify the various shocks experienced by households in Colombia were obtained from direct questions in the evaluation surveys. In each of the three survey rounds, the households were asked whether during the last year the household experienced the following shocks: crop or job loss, severe illness of the household head, death of a household member, bankruptcy, a weather shock,[39] or violent attack or displacement. The prevalence of different types of shocks at the household level during each of the cross-section surveys, for the sample of households in treatment and control municipalities, is reported in Table 16. In total, 50% of the households had at least one shock between baseline and follow-up surveys. No statistical difference was observed between treatment and control households for any of the shocks.

Data shows that exposure of the households to crop or job loss is very high—more than 11% of households had at least one crop or job loss. Around 12% of the households reported having the household head ill for more than two weeks at least once over the year prior to the survey. Death of a household member, weather shock, and a household being a victim of violence are less frequent. However, we consider them in the analysis as they can be very harmful to poor families because they not only result in loss of income but also in increased household expenditure.

[39] Fire, flood, or other catastrophic weather events.

Table 16. Frequency of shocks on households by treatment and control groups

	Crop Loss or Job Loss	Illness Household Head	Bankruptcy	Death Household Member	Weather	Violence or Displacement
			Baseline			
Control	11.39%	12.40%	2.20%	1.81%	1.55%	1.16%
Treatment	9.58%	13.96%	2.14%	2.01%	0.95%	1.02%
T-test (p value)	0.483	0.636	0.777	0.709	0.615	0.808
			First Round			
Control	12.66%	12.66%	2.47%	1.81%	1.06%	0.95%
Treatment	13.50%	11.28%	2.93%	2.54%	1.25%	1.48%
T-test (p value)	0.545	0.645	0.323	0.085	0.913	0.136
			Second Round			
Control	12.25%	10.11%	2.87%	2.09%	5.95%	1.50%
Treatment	13.67%	10.60%	2.61%	2.39%	5.57%	1.89%
T-test (p value)	0.563	0.574	0.193	0.598	0.469	0.536

Note: Percentage of households hit by a shock during a year previous to the survey. T-test of difference in household means computed clustering at the municipality level.

Control Variables

Table 17 compares mean values of observable characteristics for treatment and control municipalities. The variables included are education, sex, and ages of the child and the head of the household; age composition of the household; number of private and public schools, health centers, and banks in each municipality; and the geographical region where the municipality is located. Other variables are included in order to control for access to some form of insurance: dummies for households that have credit or savings in a bank, if household own a house or land as a measure of collateral, and have health insurance for all family members, dummy variables for less-insured households such as households whose main occupation is agriculture, and a dummy for households headed by a single parent.

Access to credit is a very important variable in the theoretical model used in this research. As a measure of access to credit we use the possession of assets and possession of credit. The possession of assets provides an indication of both the well-being of the household and, by serving as collateral, its ability to borrow money. About 64% of the treatment households are home-owners and about 15 percent of the sample report holding land assets distinct from the land on which they live. Land ownership is much more common in rural areas, as is home ownership. On the other hand, the actual use of credit is very limited, even in municipalities with the presence of a bank. there are not differences in use of credit between treatment and control municipalities prior to the program. Only 5% of the households got loans to buy a house or land, and only 2% got loans from financial institutions for other purposes. However, there were significantly more households with actual debts in treatment municipalities than in control municipalities in the following surveys. In regard to savings in credit institutions, only 2% of households had savings, and there were not statistical significant differences among treatment and control municipalities.

Table 17. Summary statistics of main variables at baseline survey

	Control		Treatment	
Children				
Age	12.240	(2.235)	12.201	(2.280)
Sex (Boys)[a]	0.535	(0.504)	0.533	(0.505)
School grade	3.627	(1.023)	3.609	(0.989)
Household Head				
Age	47.118	(11.906)	46.377	(11.630)
Incomplete elementary school	0.466	(0.507)	0.453	(0.503)
Complete elementary school	0.148	(0.354)	0.149	(0.347)
Incomplete secondary school	0.083	(0.269)	0.097	(0.295)
Complete secondary school	0.030	(0.181)	0.042	(0.199)
Household Composition				
Household size	5.612	(1.771)	5.548	(1.765)
Number of children 0–6	0.711	(0.962)	0.805	(0.966)
Members 7–13	1.624	(1.053)	1.715	(1.074)
Members 14–17	1.311	(0.971)	1.234	(0.948)
Access to Insurance				
Health insurance[a]	0.858	(0.349)	0.910	(0.286)
If have debt[a]	0.057	(0.260)	0.071	(0.291)
Own a house or land[a]	0.683	(0.477)	0.654	(0.481)
Occupation agriculture[a]	0.116	(0.165)	0 .105	(0.156)
Single parent[a]	0.196	(0.394)	0.187	(0.398)

	Control		Treatment	
Municipality				
No. private schools	4.632	(8.626)	3.788	(9.472)
No. public schools	37.391	(29.920)	43.334	(29.993)
No. banks	0.874	(1.585)	1.786	(2.423)
No. health centers	9.989	(11.258)	10.182	(8.457)
Rural[a]	0.443	(0.497)	0.556	(0.497)
Observations	7721		5198	

Notes: Averages based on three rounds. Standard errors in parenthesis. [a] Mean values of dummy variables represent percentages of children living in households that meet each of the conditions of the variables.

Methodology and Econometric Model

In this section we discuss the specifications used to examine the effect of shocks on child labor and schooling and whether FA might protect households from using children as risk-coping instruments. First, we examine the effect of shocks on school attendance prior to the program for treatment and control households. In addition, we test if access to credit prevents households from using children as risk-coping instruments. The role of credit is very important in the theoretical model used to formulate the econometric equations estimated in this paper. According to the theoretical model, households don't have to rely on self-insurance instruments to smooth consumption under the complete credit markets assumption.

To test the assumption that credit-constrained households have a higher probability of taking children out of school to buffer adverse income shocks we use retrospective questions from the baseline survey about school attendance and shocks the year 2001. We exclude households receiving payments from FA prior to the baseline survey. Results from this equation provide evidence regarding to what extent poor rural households in Colombia use children as risk-coping instruments as well as the role of credit in household consumption smoothing. We use the following specification:

$$SE_{ijt} = \beta_1 + \beta_2 S_{ijt} + \theta X_{ijt} + \varepsilon_{ijt} \qquad (6)$$

$$CL_{ijt} = \beta_1 + \beta_2 S_{ijt} + \delta_1 C + \delta_2 S_{ijt} C + \theta X_{ijt} + \varepsilon_{ijt} \qquad (7)$$

The dependent variable *SE* is a dummy variable that indicates whether children were enrolled in school and attended more than 85% of the classes each year, and *CL* indicates whether children received any income during the same year, with subscripts representing the child *i*, household *j*, and time *t*. *S* is our measure of the income shock; *C* measures households' access to credit; *W* is a dummy for each year of the survey; and *X* contains a set of controls including individual, household, and community characteristics. Equations (6) and (7) are estimated by a fixed-effects logit model.

We anticipate transitory shocks to lead to a decrease in school attendance thus we expect $\beta_2 < 0$ in Equation (6), and $\beta_2 > 0$ (an increase in child labor) in Equation (7). To investigate if the effect of shocks on child labor is due to credit constraints, we examine whether the effect of shocks varies with households' access to credit. The effect of interest is δ_2, which captures the differential impact of a shock among households with access to credit. To the extent that we believe $\beta_2 > 0$ in Equation (7) is due to credit constraints, we expect access to credit to mitigate the effect of shocks, i.e., $\delta_2 < 0$.

Second, we estimate the effect of being a beneficiary of the FA program on child labor and school enrollment, controlling for access to credit and different income shocks. For that we use a difference-in-difference (DiD) framework, under the assumptions that, conditional on observed individual, household, and community characteristics, unobserved community characteristics do not change before or after the program and there are common time effects across treatment and control areas. The basic DiD equation is represented by Equation (8):

$$y_{ict} = \alpha_0 + \beta_1 C_{ict} + \beta_2 S_{ict} + \beta_3 T_{ict} + \beta_4 C_{ict} S_{ict} + \beta_5 T_{ct} S_{ict} + \beta_6 C_{ict} T_{ict} S_{ict} + \theta X_{ict} + \mu_{ic} + \varepsilon_{ict} \qquad (8)$$

The dependent variable is a dummy variable that indicates if children work full time, attend school full time, or combine school with work, with subscripts representing the child *i*, community *c*, and time *t*. S_{it} is our measure of income shocks at household and community levels for the year prior to the survey; T_c is a dummy variable indicating whether the child lives in a treatment municipality;

C_{ic} measures households' access to credit; ; and X_{ict} contains a set of controls including individual, household, and community characteristics. The two error terms are μ_{ic}, which captures all observed and unobserved household or individual level time invariant factors, and ε_{ict}, which captures the unobserved idiosyncratic household or individual and time-varying error. Equation (8) is also estimated for child labor and schooling hours. As before, we anticipate income shocks to lead to an increase in child labor if insurance is limited, i.e., we expect $\beta_2 > 0$ and we expect credit access and FA to mitigate the effect of shocks and displace child labor as an instrument to buffer shocks, or β_4, β_5 and $\beta_6 < 0$, being β_6 higher in absolute value if credit access reinforces the insurance role of FA, or viceversa. . For this model we use observations at baseline data and at each of the following surveys (one year and three years after the implementation of the program). However, we show only the estimated coefficients for the second round of the survey.

The main concern for the empirical analysis is that not randomization of the program may have created heterogeneous characteristics between control and treatment populations. Heckman, Ichimura, and Todd (1997) suggested that nonparametric conditional DiD, or DiD matching estimators, help eliminate initial heterogeneity while at the same time taking advantage of DiD's ability to control for time-invariant characteristics. Compared to the original matching estimator, this method allows for unobserved determinants of participation since it does not require that selection bias is eliminated by conditioning on the observed covariates, only that the bias is the same in the pre- and post-treatment periods. Compared to pure DiD, this estimator has the advantage of being nonparametric, so that successful identification does not depend on specific functional forms for the respective expectations (Heckman et al., 1997).

We estimate the effect of the program on our different dependent variables using non-parametric kernel propensity score, without replacement, controlling for observable characteristics and imposing common support restrictions. Separate estimates are calculated by each of the shocks analyzed. The estimated effect of the program ATT represents the difference of the mean values of the outcome variables between treatment and control groups and between baseline and second round survey.

Results

Table 18 and Table 19 report estimates of shock variables (β_2 and δ_2) on school attendance and child labor prior to the program according with equations (6) and (7). Shock variables are measured as binary variables that indicate when a household experienced any of the following shocks during the year prior to the interview: crop or job loss, illness of a household head, death of a household member, bankruptcy, violence, or a weather shock. According to the survey, these shocks are considered severe shock events. We estimated equations for each shock separately and for a set of pooled binary variable of households that experienced one, two, and three of the above shocks during the year prior to the survey.[40] Regressions were run for age/sex groups, with all specifications control for community-time fixed effects.

Our results suggest that for the specific shocks under scrutiny, children's probability of school attendance is not affected by income shocks with the exception of weather shocks, which increase the probability of school enrollment for boys between 14 and 17 years old.[41] However, we observe that idiosyncratic shocks such as illness and death increase the probability of child labor. Specifically, illness of the household head increases the probability of work by 30% for older boys, while or job loss shocks increase the probability of work by 28% and 18% for older boys and young girls, respectively. On average, a severe income shock increases the probability that young girls and older boys work by 18% approximately.

Parameter estimations of the interaction term of access to credit with shocks indicate that households with access to credit are less likely to increase children's labor to buffer shocks.[42] This result is in line with the predictions of our theoretical model. Finally, we observe that households located in rural areas and households dedicated to

[40] Alternatively, we measured shocks as negative changes in reported labor income between the two periods considered. However, we did not find significant estimates for this measure of shocks.
[41] Although this finding goes against the predictions of our model, we explain it by the fact that weather shocks might reduce labor demand, particularly in agricultural jobs
[42] We don't interact access to credit with specific shocks because the number of observations with positive values is very small and in some cases is equal to zero for all observations.

agricultural business are positively strongly correlated with child labor, increasing the probability that children work by more than 30%.[43]

Table 18. Probability of school enrollment for children 10–17 prior to FA: Fixed effects Probit Model

	Boys 7–13	**Girls 7–13**	**Boys 14–17**	**Girls 14–17**
Death of a household member	0.050	0.202	–0.122	0.198
	(0.21)	(0.16)	(0.17)	(0.20)
Severe illness of the hh head	–0.190	–0.004	0.152	–0.149
	(0.11)	(0.12)	(0.12)	(0.15)
Crop or job loss	0.052	–0.021	0.031	–0.011
	(0.08)	(0.10)	(0.10)	(0.13)
Weather shock	0.179	0.188	0.313**	0.450
	(0.13)	(0.20)	(0.06)	(0.33)
Violence shock	0.025	–0.153	–0.054	–0.112
	(0.23)	(0.16)	(0.25)	(0.26)
One shock	–0.046	–0.001	0.060	0.055
	(0.06)	(0.07)	(0.07)	(0.08)
Two shocks	0.084	0.034	0.069	0.053
	(0.06)	(0.07)	(0.07)	(0.08)
Three shocks	0.076	0.086	0.032	0.093
	(0.06)	(0.07)	(0.07)	(0.08)

Notes: Shock variables are binary variables that indicate households that experienced a shock during the year previous to the survey. Robust standard errors, clustered at the municipality level, are in parentheses. Additional repressors were included but not reported. Municipality-year effects are included. Each individual coefficient is statistically significant at the *10%, **5%, or ***1% level.

[43] Estimations for control variables are available upon request.

Table 19. Probability of work for children 10–17 prior to FA: Fixed effects Tobit Model

	Boys 7–13	**Girls 7–13**	**Boys 14–17**	**Girls 14–17**
Death of a household member	–0.180	–0.101	–0.283	–0.183
	(0.17)	(0.18)	(0.16)	(0.16)
Severe illness of the hh head	0.168	0.167	0.301*	–0.040
	(0.12)	(0.16)	(0.14)	(0.20)
Crop or job loss	–0.007	0.188*	0.286*	0.145
	(0.12)	(0.09)	(0.12)	(0.03)
Weather shock	0.222	0.197	0.337	0.206
	(0.34)	(0.20)	(0.21)	(0.15)
Violence shock	0.237	0.391	0.046	–0.160
	(0.27)	(0.34)	(0.18)	(0.31)
One shock	0.106	0.197*	0.173*	0.074
	(0.07)	(0.08)	(0.08)	(0.08)
***Access to Credit**	–0.063	–0.059*	–0.135***	–0.107**
	(0.07)	(0.02)	(0.03)	(0.03)
More than one shocks	0.126*	0.169*	0.178*	0.164**
	(0.02)	(0.03)	(0.03)	(0.02)
***Access to Credit**	–0.068*	–0.085*	–0.173*	0.122**
	(0.02)	(0.03)	(0.03)	(0.02)

Notes: Shock variables are binary variables that indicate households that experienced a shock during the year previous to the survey. Robust standard errors, clustered at the municipality level, are in parentheses. Additional repressors were included but not reported. Municipality-year effects are included. Each individual coefficient is statistically significant at the *10%, **5%, or ***1% level.

To sum up, we observe that poor households in Colombia don't take children out of school to buffer shocks, but credit-constrained households do increase children's participation in work activities. This is an interesting result since evaluations of other CCT programs such as Progresa in Mexico and Red de Protección Social in Nicaragua (de Janvry et al., 2006; Maluccio, 2005) have assumed that parents take children out of school when they face income shocks.[44] Besides, the basic theoretical model that describes household decisions regarding education under uncertainty (Jacoby & Skoufias, 1997) predicts a decrease in school enrollment when households face income shocks.

In addition, these findings support the idea that schooling and child labor are not substitutes (Ravallion & Wodon, 2000) and that shocks might affect children's time use at intensive margin rather than at extensive margin. That is, it is possible that income shocks increase the hours of children's labor without reducing time at school, or increase the proportion of children that work and study simultaneously. Thus, the effect of shocks will be absorbed by the study and leisure time of the children.

Results from this section support the following conclusions: (i) poor rural households use child labor to cope with income shocks, but at the expense of the leisure or studying time of children, not at expense of school enrollment; (ii) shocks have heterogeneous effects on children by age and sex; (iii) the relationship between economic shocks and school attendance is in most of the cases insignificant, and different types of shocks are likely the product of heterogeneous impacts; (iv) as predicted by the model, credit-constrained households have a higher probability of using child labor as a risk-coping instrument; and (v) the effect of shocks should account for complementarities of schooling and working activities. These results are used to construct our empirical equations in the following section to estimate the effect of the FA program on children's schooling and working time use.

[44] These studies have no pre-baseline data that allow them to test such assumptions.

Impact of FA on Children's Time Use Under Risk

Table 20 shows the estimated average treatment effect on the treatment (ATT) [45] of the FA program on (i) children's school and work hours and (ii) participation in school full time or work full time or the combination of school and work. Impact of the program is estimated using DiD matching methods for each of the shock events considered in this paper, i.e., death of a household member, illness of the household head, crop or job loss, weather, and violence shocks.[46] Alternatively, we estimate ATT of FA for the whole sample of households that have been hit by any of the shock events under evaluation, and for households that have hit by more than one shock. We also estimate the effect of the program for households with access to credit and for credit constrained households, under the assumption that the program will serve as safety net for those households.

Considering shocks one at a time, we see that illness of the household head or crop loss increase by 1.25 the average number of school hours for beneficiary households with respect of not been in the program and reduces the number of work hours for children by 0.3 hours. Estimations show that the program consistently reduces the number of hours of child labor and increases the number of schooling hours for households under shocks. As we have observed that households are not likely to take children out of school when they face income shocks, our estimates suggest that the estimated positive effect of the program on schooling hours is on children's time doing homework and studying instead of attending school, which is fixed in many of the cases.

We also observe that access to credit does not reinforce the effect of the program on displacing child labor as a risk-coping instrument and have also an insignificant effect

[45] We exclude households that are not receiving payments.

[46] We are not able to find a good counterfactual for the subsample of households hit by bankruptcy shocks, so don't present results.

on schooling hours. In general, the mitigating effect of the program of adverse shocks on schooling hours is always higher than the mitigating effect of the program on working hours, meaning that the program is more effective at protecting schooling time than at reducing child labor under risk environments. Finally, we see that the program has a higher effect on work time when households face covariate shocks, such as weather and violence shocks, than idiosyncratic shocks. On average, the program increases 1.5 schooling hours, decreases 0.5 working hours for households exposed to shocks.

At the extensive margin, results are also very interesting. For all shocks considered, the program has small or no effects on the probability of children working or studying full time but very important effects on the probability of children working and studying simultaneously. We observe that being a beneficiary of the program increases the probability that children work and study simultaneously by 10% for illness of the household head, by 7% for crop or job loss or violence, and by 8% for households hit by any shock event. We also see that FA increases the probability that children study full time by 4% for households hit by illness of the household head or crop or job loss shocks and reduces the probability that children work full time by 4% for households hit by violence shocks.

For households hit by at least one shock, the program has no significant effect on school enrollment but protects children from working full time. As we observed at the intensive margin, access to credit has no significant effect on child labor when households are hit by one shock. The effect of credit became significant for households hit by more than one shock, protecting that children combine work and school. Therefore, we provide evidence that the FA program is effective in reducing child labor even for households under risk and provides strong incentives to combine work and school for children under shocks for credit constrained households but not for households with access to credit.

If we compare the impact of the program on households with shocks with the effect of the program in households with no shocks, we find that the program does not fully mitigate the effects of adverse shocks on school and work hours. We find that for households with no shocks the FA program increases schooling hours by 3.18 hours and reduces working hours by 1.05 hours. Thus, at the intensive margin, the effect of the program on households without shocks more than doubles the effect of the program on households with shocks.

At the extensive margin we see that for households without shocks the program increases the probability that children study full time by 5%, increases the probability that they combine school and work by 4%, and reduces the probability that they work full time by 10%. Therefore, the program FA does not fully mitigate the effect of shocks on child labor and child schooling participation decisions but provides some insurance that children will remain enrolled in school by increasing the probability that children combine work and school.

Table 20. Average treatment effect of FA program for the second round of the survey

	Intensive Margin		Extensive Margin		
	School Hours	Work Hours	Study Only	Work Only	School and Work
Death of a household member	2.2486***	–1.0730*	–0.0434	–0.0043	0.0297
	(0.9114)	(0.7608)	(0.0913)	(0.0984)	(0.1224)
Illness of the household head	1.1594***	–0.3607*	0.0390*	–0.011	0.1024***
	(0.3021)	(0.2418)	(0.0277)	(0.0277)	(0.0368)
Crop or job loss	1.3328***	–0.3737**	0.0451**	–0.0281	0.0787***
	(0.2643)	(0.2079)	(0.0247)	(0.0259)	(0.0331)
Weather	2.4025*	–2.1843**	0.0062	–0.1079	0.0773
	(1.5872)	(1.1538)	(0.1974)	(0.2066)	(0.2506)
Violence or displacement	2.2522**	–2.0045**	0.0749	–0.0436***	0.0734***
	(1.2550)	(0.8991)	(0.1466)	(0.0250)	(0.0326)
One shock	1.4277***	–0.4904***	–0.0309	–0.0301**	0.0846**
	(0.1879)	(0.1489)	(0.1178)	(0.0180)	(0.0236)
***Access to credit**	0.7853	–0.2684	0.0585	–0.0341	-0.1488
	(0.4473)	(0.8385)	(0.7574)	(0.5426)	(0.3242)
More than shock	1.4624***	–0.5628***	0.0209	–0.0301**	0.0856***
	(0.1879)	(0.1489)	(0.6836)	(0.0145)	(0.0158)
***Access to credit**	0.3653	–0.4672	0.0275*	–0.0638**	-0.0362***
	(0.5621)	(0.7849)	(0.0178)	(0.0180)	(0.0236)
No shock	3.1871*	–1.0529**	0.0522***	–0.1026*	0.0486**
	(0.3732)	(0.2841)	(0.0103)	(0.0489)	(0.0114)

Notes: Shock variables are binary variables that indicate households that experienced a shock during the year previous to the survey. Imbens' standard errors, clustered at the municipality level, are in parentheses. Additional repressors were included but not reported. Each individual coefficient is statistically significant at the *10%, **5%, or ***1% level.

In general, we observe that the FA program effectively displaces costly risk-coping instruments such as child labor and protects study time, not school enrollment, for households under specific shocks, and that access to credit provides some insurance for reducing child labor when households are hit by more than one shock. These results complement previous findings and provide evidence to much of the conjectures in the literature. Previous studies have found that CCT programs protect enrollment but don't prevent parents from increasing child work in response to shocks, suggesting that children combine school and work. Until now, this internal solution hasn't been tested in the program evaluation literature. This study finds strong evidence that beneficiaries of the program have a higher probability of increasing the child labor supply to buffer shocks in special for credit constrained households but keeping children enrolled in school than if they were not in the program.

Conclusions

CCTs such as FA provide cash to poor families imposing conditions in children school attendance. Recent empirical work has suggested that CCTs may be one of the policy instruments that enable households to better cope with adverse economic shocks. For instance, if the effect of the condition on school attendance, acting as a price effect, is much larger than the income effect of economic shocks, then CCTs can protect children's education under risk. In addition, if school and work are substitutes we can expect that increasing schooling will displace child labor as a risk-coping instrument. However, there is strong evidence that the relation between school and work is not one to one and that child work rather displaces study time or leisure time.

This paper goes beyond previous studies by testing the effect of a CCT and income shocks on schooling and labor decisions, allowing for complementarities between school and work activities, and controlling for the credit constraints. Using a panel dataset from the evaluation survey of a CCT program implemented in Colombia, Familias en Accion, we observe that shocks are highly prevalent and that children's participation in work activities is very high. Based on a theoretical model of human capital accumulation under uncertainty with incomplete markets, our empirical estimations suggest that poor rural households in Colombia use child labor to cope with income shocks at the expense of the children's leisure or studying time, not at the expense of school enrollment. This is an interesting result, as most studies assume substitutability between school and work. In line with the model predictions, we also find that credit-constrained households have a higher probability of using children as risk-coping instruments.

With respect to the safety net value of FA, we find that the program provides strong incentives for children to combine work and school when households are exposed to shocks. Results reveal that CCT programs can work as insurance for the schooling of the poor but are not able to completely displace costly risk-coping strategies such as child labor. Therefore, the role of FA on the safety net of the uninsured will depend on the future returns of studying time at home. That is, if combining school and work results in lower future returns of schooling, then the benefits of the program as a safety net are going to be negligible. Alternatively, if attending school is enough for human capital accumulation, we can say that CCTs serve as a safety net program.

This paper contributes to the literature of household behavior under uncertainty and provides insight into the potentialities of a specific CCT program, Familias en Accion, to protect households from using costly risk management strategies under imperfect credit markets. We show that child labor plays a significant role as a self-insurance strategy of poor households in Colombia. From a policy perspective, results from this research are very useful, as they provide evidence of the unintended effects of a broad program in Colombia and therefore in its potentialities to effectively reduce poverty in the long term.

Chapter 3 . INTRAHOUSEHOLD TIME ALLOCATION

Introduction

Conditional cash transfer (CCT) programs have been implemented in many developing countries in an effort to increase human capital and poverty alleviation. These programs provide monetary grants to poor families on the condition that the families keep their children in school and take them to regular visits to health clinics. In trying to accomplish these objectives, CCT programs may affect beneficiary families in many dimensions. These include the level and patterns of consumption, the health conditions of family members, investment in human and physical capital, and the labor supply of children and adults. In this paper we focus on the effects of CCT programs on intrahousehold time allocation on various activities—market work, domestic labor, school, and leisure for children and adults.

A number of studies have shown CCT programs to be effective in increasing school enrollment and reducing child labor (Attanasio et al, 2006; Schultz, 2004). Besides estimating the impact of CCT programs on child labor, the empirical analysis presented here will also shed some light on which household members change their labor supply decisions in response to the program. Although the program is neutral in terms of adult labor supply incentives, if exogenous changes in the income of the household impact children's work and school enrollment, there is a potential for effects on the labor supply of adults in beneficiary households.

In principle, CCT programs can be considered as having income and price effects. The cash transfer increases household income, which in turn increases both consumption and leisure, reducing labor supply of all households' members. This is the income effect. The price effect is associated with meeting the conditionalities of the program. The condition on school attendance implies a reduction in the shadow wage of children, which would result in an increase in the amount of time children spent in school relative to work. Induced changes in the allocation of time of children are likely to generate a reallocation of time of parents. These are the cross-substitution effects. That is, if children become unable to perform certain work or domestic activities, other household members (adults) may substitute for their work. Therefore, the impact of the CCT program in the allocation of time of household members is ambiguous under economic theory. We address this question empirically, taking into account the substitution possibilities in the time of family members used in household production, paid work, and leisure.

The Colombian government has been operating the CCT program Familias en Accion (FA) since 2002. For the purpose of rigorous evaluation, control municipalities were carefully chosen to be as similar as possible to the treated ones. Between 2002 and 2005, one pre-intervention and two follow-up survey instruments collected data on education, labor, income, and health at individual, household, and community levels. Time use diary data was collected for all household members above 10 years old during all three rounds. The availability of this data offers an extraordinary setting in which to analyze time allocation responses to exogenous changes in income.

In Colombia, labor markets are characterized by very high labor force participation rates of men in paid work and very low labor market participation rates of women. In addition, children tend to begin their labor force participation at early ages. One of the principal objectives of FA is to increase beneficiary children's enrollment in and attendance at school and thereby reduce early labor force participation. In the impact evaluation of FA, Attanasio et al. (2006) found that the program increased school participation by 6 percentage points for children 14 to 17 years old and by 2 percentage points for younger children. In terms of work, participation in domestic work decreased by around 10% to 12% for younger children, but participation in income-generating work remained largely unaffected. Evaluation of the impact of the program on adult time allocation has not been addressed yet.

Results show that the program affects time allocation of children and adults and that there are important substitution effects among household members and across activities. Using time use data after attending school for children enrolled in school before the program, we find a positive effect of the program on schooling hours , however the effect of the program on labor of children already enrolled in school is insignificant. We also observe some patterns of substitution between activities for children attending school as a result of the program. We find that the program increases the leisure time of boys, displacing paid work, but reduces the leisure time of girls, increasing domestic labor.

The paper also examines the effect of the program on adults' time use. The most surprising result is the increased labor supply of the adults in the program. Particularly, we find that males increase paid work at the expense of domestic labor and that females increase domestic labor at the expense of leisure time. In order to provide an explanation of these results, we analyze the effect of males', females', boys', and girls' wages on the labor supply equations of adults and children. We find important substitution and complementary relations among household members. We observe that hours of domestic work between girls and female adults of the household are complementary and that hours in labor market activities between males and boys are substitutes. These cross-substitution effects explain the increased labor supply in paid work for male adults and in domestic labor for female adults as a response to the CCT program.

This study contributes to the literature providing evidence regarding the labor supply responses of children and adults to CCT programs and their interaction among household members. Evidence of the extent to which these transfers result in significant effects on the adult labor supply is quite scarce (Cuesta, 2004; Foguel and Barros, 2008; Parker and Skoufias, 2000) and, in most cases, weak due to the lack of good data on time use of household members. The design of the FA program and the availability of panel data on individual time allocation offer a great opportunity to analyze intrahousehold time allocation in a context of policy intervention. Whether policy interventions constrain the time of some or all household members can have crucial impacts on the success of the program and future well-being of beneficiaries.

This paper is organized as follows. The next section provides an overview of the program and a description of the evaluation sample used for the empirical analysis. Section 3 reviews the literature on time allocation. Section 4 discusses the hypothesis of the program effects within a unitary model framework. Sections 5 and 6 describe the data and the empirical strategy used in the analysis. Estimation results are presented in section 7, and section 8 concludes.

Familias en Acción

The program Familias en Acción is a welfare program run by the Colombian government to foster the accumulation of human capital in rural Colombia. It is similar to other CCT programs, such as Progresa, in Mexico (now called Oportunidades); Red de Proteccion Social, in Nicaragua; and Bolsa Familia, in Brazil, that have been implemented in middle-income countries during the last decade in an effort to break the intergenerational transmission of poverty. The FA program is aimed primarily at improving the education, health, and nutrition of poor families. The nutrition component consists of a basic monetary supplement that is given to all beneficiary families with children under seven years of age. The health component consists of vaccinations and growth and development checks for children, as well as courses on nutrition, hygiene, and contraception for their mothers. Participation in the health component is a precondition for receiving the benefits of the nutritional component. All children between 7 and 18 years old are eligible for the educational component. To receive the grant, they must attend classes during at least 85% of the school days in each school month as well as during the whole academic year. The size of the grant increases for secondary education and is equal for girls and boys. The amount of the subsidy on a monthly basis at the time of the baseline survey was 14,000 Colombian pesos (COP) or (US$6) for each child attending primary school and COP$28,000 or (US$12) for each child attending secondary school in 2005. The nutritional supplement[47] is paid to families with children aged between 0 and 6 years. The amount is COP$46,500 or (US$20) per family per

[47] This subsidy is an alternative to participation in a pre-existing program called Hogares Comunitarios. Beneficiaries cannot participate in both programs with the same children. However, families with children both under and over the age of 6 can choose to send the young children to a Hogar Comunitario and to participate in FA with the older children.

month. The average transfer received per household is COP$61,500, which represents approximately 25% of average household income of beneficiary households. In general, all the transfers are received by the female head of the household every two months.

Familias en Acción determined household eligibility in two stages: first by identifying target communities and then by choosing low-income households within those communities. Selection criteria for target communities were based on the following conditions. The town must: (i) have fewer than 100,000 inhabitants and not be a departmental capital, (ii) have sufficient education and health infrastructures, (iii) have a bank, and (iv) have a municipality administrative office with relatively up-to-date welfare lists and other official documents deemed important. A subset of 622 of the 1,060 Colombian municipalities qualified for the program. Eligible households were those registered at SISBEN[48] level 1 at the end of December 1999, with children under 17 years old, living in the target municipalities. SISBEN 1 households account for roughly the lowest quintile of Colombia's household income distribution (Attanasio, 2004).

The program started operating in the latter half of 2002.[49] It has benefited approximately 1,500,000 households since its beginning, and the cost has ascended to the sum of 300 thousands of millions of Colombian pesos annually (US$150 million). The cost of the program corresponds to the 0.5% of the Colombian GDP and represents approximately 10% of educational expenditures in the country.

The Evaluation Sample

For evaluation purposes, it was decided to construct a representative stratified sample of treatment municipalities and to choose control municipalities among those that

[48] SISBEN, Sistema Unificado de Beneficiarios, is a six-level poverty indicator used in Colombia to target welfare programs and for the pricing of utilities.
[49] In a few municipalities the program started as early as the end of 2001.

were excluded from the program but that belonged within the same strata. The strata were determined by region and by an index of infrastructure based on health and education. The control towns were chosen within the same stratum to be as similar as possible to each of the treatment towns, in terms of population, area, and quality of life index. Most of the control municipalities were towns with basic school and health infrastructures but without banks or, in the few cases chosen to match relatively large municipalities, just over 100,000 inhabitants. As a consequence, control towns are broadly comparable to treatment towns (Attanasio, 2004). In the end, the evaluation sample was made up of 122 municipalities, 57 of which were treatment and 65 of which were controls.

For each municipality, approximately 100 eligible households were included in the evaluation sample. The total evaluation sample consists of 11,462 households interviewed between June and October 2002 (baseline survey), 10,742 households interviewed between July and November 2003 (first wave), and 9,566 households interviewed between November 2005 and April 2006 (second wave). The attrition rate between the three rounds was approximately 16%.[50] Most of the observations lost were households which children´ age exceeded the required age or households that move from their location and were no possible to find again. Compliance was very high,[51] more than 97% of the eligible households participate in the program, so for the analysis we include in the sample all observations from treatment municipalities.

The final longitudinal data used in this study include information from 6,519 repeated households, after excluding households that received payments before the

[50] According to Attanasio (2007) attrition between baseline survey and the second follow up survey is not statistically different between treatment and control households. Therefore we assume that lost of observations is random.

[51] The main reason for no compliance was lack of the required documents for registration. None of the households reported lack of interest of participation in the program.

baseline survey and households located in control municipalities that received payments during the second survey[52]. At the household level, the sample consists of families that are potential beneficiaries of the program—that is, households with children from the poorest sector of society. Data are collected at both the household and the individual level. The available data provide a rich set of variables that allows us to measure consumption of durables and non-durables, family composition, household socio-demographic structure, labor supply, nutritional status of children, education, household assets, income, and different shocks to income, for both rural and urban households.

Intrahousehold Time Allocation: A Review

The analysis of time use is essentially an analysis of the allocation of time to various activities such as work for wages, work in the family business, domestic chores, and school activities for children. Becker (1965), and later Gronau (1977), extended the conventional labor supply model of consumption and leisure by incorporating home production as another labor activity. They argue that work at home (or home production) will respond to economic incentives such as changes in market wages, unearned income, and productivity in a way similar to market work.

In the context of determinants of intrahousehold time allocation, several studies have econometrically addressed the effects of age, gender, household composition, market prices, and other variables on the time allocation of household members. Ellis (1994) presents a good survey of descriptive studies of time allocation patterns in rural households in developing countries. The studies consistently find that (i) female time use

[52] A total of 13 municipalities of the control sample were converted to treatment municipalities in 2005, before the second wave of the evaluation survey.

is more elastic with respect to her and her partner's wage rates than male time use as well as to the presence of young children in the house, (ii) roles and cultural norms are important determinants of time use and ignoring them introduces bias in time use equations, and (iii) non-wage income deters labor force participation by raising reservation wages and increases the consumption of leisure.

Evidence of the cross-substitution effects between child labor and adult labor supply is very scarce. Skoufias (1994) estimates the interrelationships amongst market wage rates and the time allocated by adult male, adult female, and younger household members in market work, home production, and schooling and finds that wage rates of both adults and children are important determinants of adults' and children's time use. From this study it can be concluded that if labor markets exist there appears to be an association between adult and child time, usually between the mother and the daughter. Thus, an increase in female wages can increase the female labor supply to market activities and a subsequent pulling in of girls into housework.

In spite of the existing literature on time allocation patterns of households, there are some issues that still remain to be developed. First, few studies include non-market activities, such as domestic labor, as dependent variables, and fewer still are able to include the time men spend on household chores, primarily because of data limitations. Most of the studies focus only on the labor supply of some household members, usually that of men and women in the household. However, a large number of activities in developing countries occur in the house, so accounting for such activities is very important when we want to determine the welfare of the household as a whole. Second, while there is evidence that household time is not equally distributed across members, there is still little evidence of how the demographic composition by gender/age affects time allocated in the market, home production, and schooling.

The effect of CCT programs on children's time allocation has been widely studied in the literature. In general, CCT programs have been successful at increasing school enrollment and reducing child work. Frequently, these impacts have been concentrated among older children. Schultz (2004) shows that the Progresa program in Mexico has a positive effect on schooling and helps to reduce child work, particularly for boys, while it also helps decrease domestic work for girls. In Nicaragua, the Red de Proteccion Social (RPS) reduced child work by 3–5 percentage points among children aged 7–13 (Maluccio and Flores, 2005). However, some evaluations have found no significant effect on child labor. For example, an analysis of Bolsa Escola, a CCT program implemented in Brazil, finds that the program has a big impact on increasing school enrollment but no influence on child labor (Bourguignon, Ferreira, & Leite, 2003; Cardoso & Souza, 2004). Attanasio et al. (2006) find that FA has a positive effect on school enrollment, especially in older children, a negative effect on domestic work for young children, and a neutral effect on income-generating work. In most cases, there is evidence that the effect of CCT programs is much stronger on school than on work and that child labor is usually increased at the expense of child leisure rather than school enrollment.

Very few researchers have studied the time allocation effects of CCT programs on adults and other members of the household. In practice, CCT programs appear to have been a modest disincentive or have had nil effects on adult work. Parker and Skoufias (2000) have used Progresa time use data to study the impacts of the intervention on adults' time allocation. The authors found significant effects of the program on adult females' household work, and no effects were observed on adults' market labor supply. The data used by Edmonds and Schady (2008) suggest that the Bono de Desarrollo Humano (BDH) program in Ecuador had no effects on the adult labor supply. Only in Nicaragua is there some evidence of significant negative effects on adult work. Maluccio and Flores (2005) show that the RPS resulted in a significant reduction in hours worked by adult men in the preceding week (by about 6 hours), with no effect among adult women. The few significant impacts imply adjustments in the intensive margin (hours) rather than the extensive margin (participation).

The effect of CCT programs on intrahousehold time allocation has not been widely explored, although it has been recognized that the household is an important intermediary between aggregate policies and individuals and that any change in the constraints, technology, or prices facing the household will induce the household to reallocate resources in order to conform to the optimized allocation. Moreover, Ardington, Case, and Hosegood's (2009) results from South Africa indicate that transfers might affect even more complex within-household interactions, inducing unexpected labor supply responses. There might also be potential heterogeneity in the effects in the gender and age dimensions, among others.

Model

Unitary Model

The theoretical basis for understanding the intrahousehold decision making process of resource allocation has seen three distinct developments: the unitary model, bargaining models, and the collective model. In this paper we formulate our hypothesis under the unitary model framework.

The unitary model considers each household as one decision unit with its own utility function and preferences. This is observable in the case of common preferences households, where everyone in the household has the same preferences in making household decisions, and dictator households, where one person makes all the household decisions according to his or her preferences without including the preferences of other household members. In the unitary model, the household decision process can be viewed as a household utility maximization problem where individuals specialize in certain activities because of comparative advantages. Because men's wage rates are higher than those of women, the model predicts for two-earner households that men specialize in paid labor and women specialize in household tasks (Becker, 1965, 1991).

A drawback of the unitary approach is that it imposes two restrictions on household behavior that are often rejected in the empirical literature. The first restriction is referred to as *income pooling* and means that it does not matter which member of the household generates household income. The second restriction is referred to as *Slutsky symmetry* and means that marginal compensated wage changes of the spouses have the same effect on each other's labor supply. The empirical literature that rejects the unitary model is substantial (see, among others, Thomas [1990], Browning and Costas [1991], Browning, Bourguignon, Chiappori, and Lechene [1994], and Browning and Chiappori [1998]).

A number of empirical studies have attempted to overcome the constraints of the unitary model through a collective or bargaining approach (Aronsson et al., 2001; Cuesta, 2004; Haddad, Hoddinott, & Alderman, 1997; Udry, 1996). However, still today there is an absence of clear conceptual work on a collective model of intrahousehold time allocation. Such models, while more realistic, have not provided clear directions as to how intrahousehold allocations will differ when some individual attains more bargaining strength (Rosenzweig & Schultz, 1984). For that reason, we decide to use a unitary model for our analysis.

Comparative Statistics

The impact of FA on time use can be analyzed through a family labor supply model under the unitary household framework, where the time allocation decisions of all household members are affected by the value of time of all household members. This model is a simple extension of the one-person model to account for the interrelated decisions of two or more household´s members concerning labor supply. One basic difference in empirical implications from the one-person model is the inclusion of other family members' wages in an individual's labor supply function rather than just the individual's own wage and the sum of all other income. This allows for interesting patterns of substitute/complementary relationships among the labor supply functions of different household members. Equation (9) represents our labor supply equation.

$$H_i^j = H(P, w_i, \ldots, w_m, R, A, \varepsilon) \qquad (9)$$

Where:

H_i^j = hours on activity *j* of individual *i*

R = non-wage income

w_i = wage for market work of individual *i*

P = price of consumption goods

A = individual characteristics

A simple comparative statistics analysis allows us to identify the mechanisms through which the economic incentives raised by a CCT program affect the allocation of time of all household members. First, we analyze the effect of the program on children's time allocation. The price effect is caused by the conditions of the program on beneficiary household members. The condition on children's school attendance implies that the shadow value of children's time in activities other than school is reduced. This would tend to imply an increase in school and a reduction in participation in other activities. On the other hand, households that receive the transfer are less likely to be dependent on the income of their children and therefore may reduce child work, as suggested by a number of theoretical models (Basu & Van, 1998; Baland & Robinson, 2000). That is the income effect of the subsidy. If schooling and work are substitutes, the income effect will reinforce the reduction in work through the substitution effect of the subsidy, so that it is possible to observe a larger reduction on work than an increase in school for children. However, time spent in school is ordinarily only a fraction of a day, so it is possible to increase time dedicated to school by reducing leisure without necessarily reducing work.

It should be noted that for households that would have sent their children to school even in the absence of the program, the transfer is effectively an unconditional transfer that increases household income without altering any relative prices. However, the subsidy might still bring about changes in household behavior due to the increase in income. The same effect has the nutrition subsidy.

The effect of the program on adults' time allocation has different patterns since the level of benefits given to households is not affected by household members' decisions to work or the income level of the household.[53] We assume households have no incentives to reduce the labor supply in order to meet conditions to be in the program. Thus, it can be argued that the main effect of FA on adults' labor supply is a pure income effect. We assume that an increase in income (through the subsidy) increases the demand of all normal goods, which includes both consumption and leisure, reducing adult labor supply. In addition, an additional impact on work and leisure time for women could be expected, as they must comply with the clinic and school attendance requirements of family members, reducing their work or leisure time.

Table 21 summarizes our hypothesis, which will be tested in the empirical equations. Under the unitary model framework we should consider cross-substitution effects between adults and children. The sign of the cross-substitution effects between adults and children will be positive or negative depending on whether time allocation of adults and children are substitutes or complements. For example, the sign of a cross-substitution effect on the labor participation of an adult will be positive if the adult is substituting for the child's work. Because schooling appears as a more attractive option after receiving the benefit, the shadow value of other activities performed by a child also decreases, which in turn implies an increase in the adults' shadow value of time to such activities. Labor income of working minors is extremely relevant in the analysis of substitutability and complementarity between adult labor and child labor. Under the

[53] Although eligibility is based on a poverty index that includes income along with other socioeconomic variables, the index is updated every three years and changes are mainly explained by improvement in housing or significant increases in income. Therefore, we don't expect households to reduce labor supply in order to retain eligibility for the program.

substitutability hypotheses, a drop in adults' income should be followed an increase in child labor and children's labor income.

Table 21. Hypothesis of the impact of the CCT program on the labor supply

Activity	Price Effect	Income Effect	Total Effect
School	+	+	+
Children's work (Substitute to school)	–	–	–
Children's work (In addition to school)	?	–	?
Adults	None	–	–

Data

The data used in this paper come from the evaluation survey of FA, which was designed especially for the purposes of impact evaluation of the program. The survey collected information of individuals and households located in treatment and control municipalities between 2002 and 2005: the baseline survey was in 2002, the first round was in 2003, and the second round was in 2005. The surveys contain information on a wide range of variables, including the household socio-demographic structure, housing conditions, education and health variables for household members, household consumption, labor supply, income, and transfers. Thus, we use this valuable balance panel dataset to model household behavior of eligible households[54] and to evaluate the effect of the program on diverse economic decisions.

[54] According to Attanasio et al. (2004), eligible households in the sample are representative of populations at extreme poverty living in small towns in Colombia.

For the purposes of this analysis, we use a module on time use data that contains information on the time each household member older than 10 allocates to each of a total of 6 activities during the day before the interview.[55] These activities were classified into different work categories: market work, domestic work, schooling, and leisure. Market work is defined as work for wage or working in their own business; domestic work is household chores and work without payment; schooling is time attending school and time doing homework at home; leisure is the difference between 24 hours and the total time spent on all other reported activities. We exclude from the sample individuals interviewed on a Sunday or a Monday, since time use patterns over the weekend are likely to be different from time use patterns during the week, especially for children attending school. Given that the reference period (one day) is short, we could expect underestimation on the percentage of individuals who carry out each activity.

The survey also has a module of time use after school for children older than 7 conditional on attending school at the moment of the survey. We use this data to test for substitution/complementary relation between child work, domestic labor, schooling, and leisure. Leisure is reported in the survey as time playing and lazing around. This measure has a comparative advantage over the usual measure of leisure since leisure calculated as a residual inherits the measurement errors of the other time use components, such as temporal double counting. Then, this data offers a unique opportunity to identify the effect of the program on children already attending school and the complementary relation of schooling with other activities.

[55] The 24-hours method can miss activities that do not leave market traces, such as childcare. Another problem is that respondents usually recall the day in which the activity is predominant and then use the hours spent on that activity on that day as representative of typical time spent on that activity, thus frequently resulting in over-reporting.

The final dataset used for estimation purposes consists of 25,436 individuals in 5,670 eligible poor households, with time use variables for three years, including pretreatment data. In this subsample, the average household size is about 6 members, and all the households have children younger than 18 years old. The head of the household is on average 46 years old, and the average level of education is incomplete primary school for the head of the household and complete primary for all other individuals. Only 25% of the households own their house, and 55% of the households are located in poor neighborhoods.

Table 22 compares mean values of observable household and municipality characteristics for treatment and control subsamples at the baseline. We expect no pretreatment differences among municipalities since control municipalities were randomly selected in order to have a control sample as similar as possible to the treatment sample. A Hotelling T-squared test of the difference of the means between treatment and control groups shows balancing of our samples; also, a T-test shows no difference between the means along most of the observable dimensions included. Therefore, we assume that our sample resembles a random assignment of the program and we are able to offset selection bias problems.

Table 22. Summary statistics by treatment group, sample means

Variable	Treatment	Control	T-test (p_value)
Individuals			
Sex+	0.507	0.518	0.061
Age	27.815	27.885	0.525
Education	3.383	3.352	0.640
Household head+	0.201	0.202	0.873
Household			
Female household head+	0.183	0.172	0.109
Single parent+	0.199	0.190	0.340
Health subsidy +	0.641	0.655	0.205
Household head age	46.180	47.267	0.000
Adults age	39.227	40.176	0.000
Household head education	2.811	2.895	0.130
Adults education	3.046	3.118	0.000
Agriculture+	0.109	0.107	0.000
Household size	5.648	5.211	0.243
Own house+	0.287	0.244	0.230
Children 14–17 years old	1.298	1.230	0.000
Children 7–13 years old	1.427	1.493	0.000
Children 0–6 years old	0.782	0.683	0.000
Ln wage men	6.828	6.835	0.260
Ln wage women	2.845	3.004	0.000
Ln wage children	0.435	0.425	0.140
Municipality			
Urban+	0.442	0.562	0.000
Hospitals	0.046	0.042	0.000

Variable	Treatment	Control	T-test (p_value)
Health centers	10.182	9.989	0.608
No. private schools	3.788	4.632	0.000
No. public schools	41.334	37.391	0.000
Banks	1.786	0.028	0.000
Strata 1*	0.545	0.524	0.090
Strata 2	0.260	0.266	0.267
Distance to school	13.885	13.657	0.262
Distance to town	48.416	52.407	0.000
Observations	11945	13490	

+ Dummy variables; mean values represent percentages.

* Strata is a six-level socioeconomic index used in Colombia for the pricing of utilities, where strata 1 corresponds to the lowest income decil in the society.

Notes: The measure of wages is in log and in units of 2002 pesos. Municipality variables indicate the number of health and educational institutions. Distance to school and town are expressed in minutes.

Time Use of Adults and Children

In this section we provide a general description of labor market activities and time use from treatment and control groups prior to implementation of the program by gender/age groups. Table 23 shows labor force participation of women and men by age group reported during the baseline survey. Participation on each activity is measured by binary variables indicating individuals that spend more than 1 hour on each specific activity. As we can observe, communities where FA operates are characterized by very high labor market participation rates in paid work for men and very low labor market participation rates for women. Women have higher participation in domestic labor in all age groups. Participation in school is very similar for girls and boys.

Table 23. Labor force participation prior to the program, percentages

Age	Gender	Work	Domestic	School
10–17 years	All	32.23	72.67	68.40
	Boys	35.48	65.03	71.49
	Girls	28.90	80.14	65.18
18–60 years	All	61.54	70.70	27.04
	Men	76.26	50.31	27.74
	Women	46.11	90.48	27.67
>60 years	All	50.50	65.12	24.88
	Men	62.67	52.10	23.10
	Women	33.39	81.06	25.06

Table 24 shows daily hours spent by adults in paid and domestic work. Men spend on average 5 hours on paid work and less than 2 hours on domestic labor. On the contrary, women spend 5 hours on average on domestic labor and a little less than 2 hours on market work. Individuals older than 60 spend on average 2 hours per day on domestic labor and 2.5 hours on market work. We observe the same specialization of men in market work and women in domestic labor that we found on labor force participation (see Table 23). Pretreatment differences in time allocation between treatment and control households is controlled in our empirical estimates using DiD matching methods.

Table 24. Time allocation of adults, number of hours

Group		**Treatment**		**Control**	
		Baseline	Follow-Up Surveys	Baseline	Follow-Up Surveys
Hours of paid work	Men 18–60	5.193	5.449	5.346	5.347
	Women 18–60	1.622	1.662	1.796	1.744
	> 60	2.640	2.304	3.062	2.245
Hours of domestic labor	Men 18–60	1.520	1.282	1.028	1.216
	Women 18–60	5.107	4.935	4.992	4.884
	> 60	2.113	2.265	1.740	2.496

Notes: Numbers of hours spent on each activity the day before the interview. Mean number of hours for the sample includes those that report zero hours on each activity.

Time Use of Children

Child labor is a major problem in Colombia. According to the FA evaluation survey, an estimated 15% of children between the ages of 10 and 17 were working at the moment of the survey and not attending school. Children tend to begin their labor force participation at early ages, on average at 14 years old, in order to contribute to family income levels. Children’s wages are very low relative to those of the adults, and most of them receive no health or unemployment benefits. One of the principal objectives of FA is to increase children’s enrollment in and attendance at school and thereby reduce this early labor force participation of children. Table 25 shows the number of hours spends on activities by children in our sample. Pretreatment differences in time allocation between treatment and control households is controlled in our empirical estimates using DiD matching methods.

Table 25. Time allocation of children, number of hours

		Treatment		Control	
	Group	Baseline	Follow-Up Surveys	Baseline	Follow-Up Surveys
Hours of paid work	Boys 10–13	0.139	0.182	0.049	0.149
	Girls10–13	0.054	0.044	0.024	0.064
	Boys 14–17	1.355	1.757	1.125	1.480
	Girls 14–17	0.517	0.458	0.332	0.528
Hours of domestic labor	Boys 10–13	0.716	0.630	0.461	0.689
	Girls 10–13	0.777	0.788	0.465	0.763
	Boys 14–17	2.070	1.500	1.527	1.374
	Girls 14–17	2.651	2.024	2.138	2.002
Hours of school	Boys 10–13	3.001	3.183	2.859	3.667
	Girls 10–13	2.964	3.109	2.949	3.190
	Boys 14–17	3.543	3.254	3.223	3.319
	Girls 14–17	3.558	4.216	3.732	3.987

Notes: Number of hours spent on each activity the day before the interview. Mean number of hours for the sample includes those that report zero hours on each activity.

Time use data of children who attend school are available for children between 7 and 17 years old; however, we report statistics for children between 10 and 17 years old to make data comparable with the overall measure of time use. Participation in paid work, domestic work, and leisure time activities after school is reported in Table 26, and mean number of hours on each activity is reported in Table 27. Older children and boys spend more hours on market labor than younger children and girls. However, children who attend school spend most of their time on school-related activities, leisure activities, and domestic activities.

Table 26. Labor force participation of children conditional on attending school

		Baseline		Follow-Up Surveys	
	Group	Treatment	Control	Treatment	Control
Paid work	Boys 10–13	3.13%	3.38%	4.99%	3.42%
	Girls 10–13	1.39%	0.85%	1.00%	2.43%
	Boys 14–17	12.78%	8.09%	16.74%	10.65%
	Girls 14–17	3.85%	1.53%	2.90%	3.30%
Domestic labor	Boys 10–13	65.14%	59.71%	63.05%	61.14%
	Girls 10–13	74.14%	71.91%	70.18%	76.11%
	Boys 14–17	70.03%	68.72%	70.79%	71.76%
	Girls 14–17	92.63%	90.41%	92.91%	88.21%

Table 27. Time allocation of children conditional on attending school

		Baseline		Follow-Up Surveys	
	Group	Treatment	Control	Treatment	Control
Hours of paid work	Boys 10–13	0.076	0.072	0.141	0.079
	Girls 10–13	0.046	0.016	0.032	0.052
	Boys 14–17	0.832	0.144	0.756	0.380
	Girls 14–17	0.121	0.028	0.147	0.158
Hours of domestic labor	Boys 10–13	0.977	0.713	0.940	0.831
	Girls 10–13	1.086	0.900	1.191	1.057
	Boys 14–17	0.802	0.611	0.649	0.995
	Girls 14–17	1.465	1.033	1.725	1.443
Hours of study	Boys 10–13	1.350	1.282	1.424	1.390
	Girls 10–13	1.528	1.437	1.537	1.545
	Boys 14–17	1.091	0.986	1.600	1.618
	Girls 14–17	1.413	1.280	1.817	1.870
Hours of leisure	Boys 10–13	2.524	2.493	2.925	3.082
	Girls 10–13	2.342	2.441	2.261	2.740
	Boys 14–17	1.487	1.412	1.422	1.551
	Girls 14–17	1.430	1.455	1.085	1.215

Notes: Number of hours spent on each activity the day before the interview. Mean number of hours for the sample includes those that report zero hours on each activity.

Income and Wages

This paper analyzes not only the time allocation of household members but also the interrelations among market wage rates and the time allocated by adults and children by including individuals´ and other household members' wages as controls on time use equations.

Income and wages in Colombia differ greatly among gender/age groups even though our sample is limited to poor households in Colombia. Income in our data is the sum of all labor income and non-wage income. Labor income is income from an employer or their own business; non-wage income is income from rents, pensions, and other government transfers. As we can observe from Table 28, labor income represents a large percentage of total income, especially for men and boys, while non-wage income represents almost 23% of girls' income.

Table 28. Monthly average income at baseline

	Income	**Labor Income**	**Non-Wage Income**	**Hourly Wage**
Adult males	221,009	215,779	5,230	6,503
Adult females	64,539	58,061	6,478	5,073
Boys	13,681	12,912	769	3,267
Girls	3,128	2,552	576	1,582

Note: Income measures are deflated to the 2002 price level in Colombian pesos.

Empirical Model

The estimation of the impact of FA on labor force participation and time use is based on the difference-in-difference (DiD) estimator. This estimator is based on comparing differences between the treatment and control groups before and after the beginning of the program. It offers the advantage that any pre-program differences between the treatment and control groups are eliminated in the estimation of impacts, under the assumption that any unobserved heterogeneity between the treatment and control groups is fixed over time. We use a number of control variables that may be useful for reducing any remaining statistical bias.

The following regression equation defines a DiD model that lets us estimate various difference estimators allowing for individual, household, and locality observed characteristics:

$$Y_{ijt} = \alpha + \sum_t \beta_t W_t + \sum_t \gamma_t T_j W_t + \theta_{ijt} X_{ijt} + \varepsilon_{ijt} \qquad (9)$$

Y_{ijt} denotes the value of the outcome of interest for individual *i* and household j in period *t*; T_j is a binary variable taking the value of 1 if the household resides in a treatment community and 0 if it resides in a control community; W_t are binary variables equal to 1 for each round of the follow-up surveys and the value of 0 for the baseline survey. The vector X summarizes individual, household, and municipality characteristics, and ε_{ijt} is an error term summarizing the influence of random disturbances. The parameters of interest are γ_t, which indicate the effect of the program on the outcome for each follow-up survey.

In this research we find unbiased estimates of the effects of the program by pre-processing our dataset with matching non-parametric methods and then applying parametric techniques to increase efficiency. This procedure makes parametric models produce more accurate and considerably less model-dependent causal inferences (Ho, Imai, King, & Stuart, 2007). First, we use a non-parametric kernel propensity score with replacement to match treatment and control observations. We use matching weights of the control observations for estimations of the parametric equations. Second, we estimate the parametric specification of Equation (9), restricting the analysis to individuals in the common support[56] to minimize any bias due to extrapolation. The standard errors were estimated taking into account the clustered nature of the sample. Finally, we report a bias-

[56] The region over which treated individuals have a counterpart in the group of controls.

corrected matching estimator proposed by Imbens (2004) which adjusts the difference within the matches for the differences in their covariate values

We estimate impact on the following outcomes: hours of paid work, domestic work, school, and leisure. We estimate a Tobit model when the dependent variable is the amount of hours spent on each activity and ordinary least squares when the dependent variable is leisure hours.

For the empirical estimation we separately consider time use by gender and age groups. For adults we consider hours on paid work and domestic labor separately for men and women between 18 and 60 years old and for individuals older than 60. In the case of children, we have 4 groups: boys and girls between 10 and 13 years old and boys and girls between 13 and 17 years old.

To check cross-substitution effects we estimate the interrelationships between adult male, adult female, and child market wage rates and time allocated to work, domestic labor, and schooling by boys and girls. Assuming that utility functions of the household members are weakly separable, we estimate labor supply equations for children as a function of own wages and other household members' wages. We also assume that transfer from the program is pooled at the household level and has the same effect as non-wage income. We use parameter estimates of wage and income variables to understand the substitutibility and complementarity between child labor and adult labor.

Control Variables

We use a number of different individual, household, and municipality variables likely to influence children's and adults' time allocation. With respect to individual characteristics, regressions include education level, age, age squared, non-labor income, and wages of all household members on market activities. Parental and household characteristics include household head and spouse education levels, the age of the mother and father , whether the occupation of the household head is in agriculture, whether the household head is a woman, the number of children between 0 and 6 years old (potentially beneficiaries of nutrition subsidy), the number of children between 7 and 11 years old (potentially beneficiaries of education subsidy for elementary), the number of children between 12 and 17 years old (potentially beneficiaries of education subsidy for secondary), household size, whether the household owns a house, whether the household has health insurance, the log of the potential FA cash transfer, the non-wage income of the household, and wages of all members of the household.

Local or municipality characteristics include variables used to select the sample of treatment and control municipalities: number of hospitals; public schools for elementary and secondary education; banks; travel time to the nearest school; travel time to the nearest town; population; whether it is located in an urban or a rural area; average wages for males, females, and children at the municipality level; dummies for socioeconomic strata were the household is located inside the municipality; and dummies for geographical economic regions in Colombia. These variables are very important for the matching methods since selection of treatment and control samples were based on these variables.

Results

In this section we turn to the results of the impacts of FA on time use for adults and children by gender/age groups and analyze the possible substitution or complementary relationship between child labor and adult labor supply.

Impacts on Children's Time Use

Table 29 reports the estimated impact of the FA program on boys' and girls' total schooling time, market work, and domestic work for young children (10–13 years old) and for older children (14–17 years old). For each activity, we report the estimated coefficient of the treatment variable, the parameter γ from equation (9) for the second round of the survey, which is the average increase/decrease in the number of hours allocated to each activity if a household is a beneficiary of the program with respect of not being in the program. All the effects are estimated for those individuals within the common support. The estimated impact of the program in this table reflects the income and substitution effect of the CCT program.

Table 29. Impact of the program on hours of child time uses, Tobit model

Number of Hours	**Boys 10–13**	**Girls 10–13**	**Boys 14–17**	**Girls 14–17**
School	2.182***	0.717***	2.269***	2.577**
	(0.31)	(0.21)	(0.66)	(0.95)
Paid work	1.855	0.045	–1.408*	–0.799
	(1.02)	(0.05)	(0.65)	(0.54)
Domestic labor	–0.545***	–0.355**	–0.524*	0.195
	(0.20)	(0.16)	(0.25)	(0.31)

Note: Estimations are the ATT of the program FA. Imbens´ standard in parentheses. Each individual coefficient is statistically significant at the *10%, **5%, or ***1% level.

According to the results, the FA program has increased schooling hours for all children by 2 hours on average; has decreased domestic labor, particularly for young children; and has significantly decreased paid work for older children. We observe that increased schooling hours displace domestic and income labor. These results are in line with previous studies that have estimated the effect of the program on schooling and working activities (Attanasio et al., 2006).

We also estimate the effect of the program on time use for children who were attending school previous to the program. For that, we use a module that asks for hours in activities different to attending school. These effects are very interesting since enrollment rates were relatively high during the baseline survey (around 85% of the children). In addition, time use data collected for this sample of children lead us to understand substitutibility or complementarity of different activities with school attendance.

Table 30. Impact of the program on hours of child time uses for children who were attending school prior to the program, Tobit model

Number of Hours	**Boys 10–13**	**Girls 10–13**	**Boys 14–17**	**Girls 14–17**
Paid work	–0.083	–0.182*	–0.330*	–0.371
	(0.17)	(0.08)	(0.16)	(0.28)
Domestic labor	0.027	0.504*	0.343	–0.908
	(0.11)	(0.22)	(0.35)	(0.51)
Doing homework and studying	–0.036	–0.029	–0.678	0.056
	(0.35)	(0.30)	(0.40)	(0.22)
Playing and leisure	–0.078	–0.302*	0.279*	–0.618
(excludes sleeping time)	(0.34)	(0.17)	(0.12)	(0.51)

Note: Estimations are the ATT of the program FA. Imbens´ standard errors in parentheses. Each individual coefficient is statistically significant at the *10%, **5%, or ***1% level.

Results in Table 30 show that for children who were previously enrolled in school, the program has no effect on activities complementary to school. It also has very important policy implications, as we show that the same program without a conditionality on school attendance will not have an effect on schooling time. Nevertheless, the effect of the subsidy is reflected in reduced working hours and leisure time for young girls, which is compensated by increased domestic labor. The program also has a negative effect on working hours for older children, which is compensated by increased leisure time. In general, the effect of the subsidy reduces paid working time for children enrolled in school, as households are receiving extra income. This effect goes in line with our predictions. However, the effect of the program on domestic labor is more difficult to understand, as other household members should be doing this work.

According to the results shown in Table 29 and Table 30, domestic labor decreased at the expense of schooling time for boys who were not attending school before the program, but it has increased domestic labor for girls already enrolled in school at the expense of their leisure time. However, the increase of domestic labor for young girls is just a fraction of the total reduction of domestic labor by all children. Therefore, we expect adults to increase time on domestic chores. We test this in the following section.

Finally, FA does appear to have negative significant effects on leisure time of younger girls but a positive effect on leisure time of boys. It is interesting to observe how household preferences in leisure time allocation among household members differ by sex. Our results suggest higher preferences for leisure time for boys than for girls. However, this can be also explained by wage differences among sexes. From these results, we can conclude that leisure is a substitute for work and domestic labor, and all work can be complementary of schooling.

Impacts on Adult's Time Use

Table 31 presents the results for adults in terms of the effect of FA on paid work and domestic labor. We expect the impact of FA on all work to be negative if leisure is a normal good. Nevertheless, we observe opposite behavior: hours of paid work increase for male adults, and hours of domestic labor increase for female adults. This means that adults could be substituting for hours of work of children in all work activities. For instance, there may have been a substitution between male adults and boys in income-generating labor but a complementary relationship between women and girls in domestic labor. Overall, the results do not show significant impacts of FA on the work time of old men or women. We conclude that there is evidence to support the hypothesis that FA has reduced the leisure time of adults.

Table 31. Impact of the program on hours of adults' time uses by age/sex groups, Tobit model

	Males 18–60	**Females 18–60**	**>60**
Paid work	0.889***	0.174	0.583
	(0.27)	(0.19)	(0.09)
Domestic labor	–0.406***	0.274*	–0.092
	(0.15)	(0.20)	(0.39)
Leisure	–0.209*	–0.159*	0.321
	(0.13)	(0.07)	(0.24)

Note: Estimations are the ATT of the program FA. Imbens´ standard errors in parentheses. Each individual coefficient is statistically significant at the *10%, **5%, or ***1% level.

Cross-Substitution Effects

To better understand the cross-substitution effects of the CCT program between parents and their children, we include own wages and other household members' wages in the labor supply equations estimated for children and adults. We also include the amount of the subsidy as non-wage income. Results suggest the importance of the opportunity cost of time in intrahousehold time use.

The signs and significance of the wage coefficient estimates turned out to be robust across specifications. We observe that increases in the opportunity cost of time of one family member not only affect the amount of time devoted to various activities by that family member, mainly reducing their hours in market work, but also have a significant effect on the time allocation of other family members.

In general, we observe that own wage has a negative effect on all activities. In addition, children's wage does not affect male adults' time allocation. We also observe some complementarity of hours of work between children and female adults of the household. On the other hand, male wages do not affect the time allocation of women, while female wages do affect the time allocation of men. We can conclude that hours in labor markets between males and females are substitutes.

Conclusions

The estimates presented in this study provide some evidence of the intrahousehold time allocation effect of a CCT program in Colombia. The analysis comprises the intended effects on children's time use and the potential scope of unintended labor supply effects among adults, which is especially relevant given the increasing coverage of the program throughout the country.

Based on our results, we prove that the program is effective at increasing schooling time for all children, displacing income and domestic labor. However, we find that the effect of the program on schooling of children already enrolled in school is insignificant. We also observe some patterns of substitution between activities for children attending school as a result of the program. We find that the program increases the leisure time of boys, displacing paid work, but reduces the leisure time of girls, increasing domestic labor.

The study also examines the effect of the program on adult's time use. The most surprising result is the increased labor supply of adults in the program. Particularly, we find that males increased paid work at the expense of domestic labor and that females increased domestic labor at the expense of leisure time. Neither economic theory nor previous evidence explains such behavior. We provide some explanations, as this effect was robust across several specifications. First, the income elasticity of leisure may be very low for extremely poor households. This is a realistic explanation as the estimated coefficient for the FA subsidy in our different regressions, when statistically significant, is negative and relatively low[57]. Second, the positive impact of CCT programs on children's school attendance might free time previously spent in childcare, further reducing the cost of work for adults (Baker, Gruber, & Milligan, 2005). If hours on labor markets between males and females are substitutes and hours of work between girls and

[57] Results indicate that a 10% increase in FA subsidy decreases boys' and males' labor supply by 0.4%.

female adults are complementary, as our estimates suggest, this is a very plausible explanation.

There are other reasons that might help explain why there have not been large disincentives to adult´s labor supply associated with CCT programs, but these reasons are not likely in our specific program or need further research to be proven. First, for some households the reduction in income from child work and the increase in school expenditures associated with the additional school enrollment might offset the amount of the transfer (Fiszbein, Schady, Ferreira, Kelleher, Olinto, & Skoufias, 2009). We consider this is not the case in our sample, as average household transfer is almost equal to average child income; however, we might need to test for increased schooling costs. Second, it is possible that adults would not change their labor supply if households perceived transfers to be temporary rather than permanent (Fiszbein et al., 2009). We think this is not very probable in this case, as FA has been continuously expanding since its implementation in 2002 and the government has promoted it as a permanent program for the poor. Third and finally, it is possible that the program has brought changes to wages in the market, changing work incentives. For the purpose of this analysis we have assumed that the program has not affected market wages. Nonetheless, it is recommendable that future research looks at the potential general equilibrium effects of the program, as this could have important consequences for the interpretation of the effects of the program on different outcomes.

This research provides relevant results for our understanding of labor markets in general and especially in how the role of the family interacts with public interventions such as CCT programs. Results have large implications on economic policy, as intrahousehold time allocation is crucial to comprehend the income generation process of the poor and to assess the overall well-being of beneficiaries of the program.

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VITAE

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