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The Impact of a Conditional Cash Transfer Program on Credit Behavior in Colombia

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THE IMPACT OF A CONDITIONAL CASH TRANSFER PROGRAM ON CREDIT BEHAVIOR IN COLOMBIA

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Abstract

This paper investigates the impact of *Familias en Acción*, a conditional cash transfer program in Colombia, on participant credit behavior. The motivation of the research is derived from previous studies which indicate that conditional cash transfer programs have effects on households aside from those directly intentioned by the programs. While the direct impacts of *Familias en Acción* have been measured by the research team responsible for evaluating the program, potential indirect effects remain uninvestigated. My research specifically focuses on the impacts of the program on credit behavior. I compute estimates on the percent change in loan balance outstanding and credit participation over the four-year evaluation period by comparing households that are benefiting from the program (treatment) and those that are not (control). Because *Familias en Acción* was not a randomly assigned program, I use quasi-experimental data collected in three rounds over four years. I control for dissimilarities between the treatment and control group by utilizing a difference-in-differences approach and by controlling across a wide-range of observable household characteristics. I find that the program does affect credit behavior in treated households. In both urban and rural areas, the outstanding loan balance and the number of households involved in the credit market increases after the first year of the program. After four years of the program, the effect is still significant and positive in rural areas though not in urban areas. This indicates that the program affects credit behavior in all treated households in the short run and rural households in the long run. These findings provide new considerations for policy makers who are implementing these programs in developing countries.

I. Introduction

While there is no panacea for ending global poverty, conditional cash transfer (CCT) programs are viewed as an innovative approach, providing both short-term relief to impoverished families and long-term investments in their education and health. CCT programs have spread quickly from their early successes in South Asia and Latin America and are now found on every continent, operating in more than two dozen developing countries. Both low- and middle-income countries have used these programs as an integrated approach to poverty reduction, balancing goals of social assistance with human capital formation. The programs provide cash subsidies to poor households contingent on the household meeting certain health and education criteria, such as regular school attendance and health check-ups for children. The first generation of conditional cash transfers was marked by good execution with respect to general administration and impact evaluations (Ferreira et al., 2009). From these evaluations, CCT programs have proven capable of creating positive outcomes such as efficient targeting, increased food consumption, and improved school enrollment in the poorest of the countries' populations (for example, Rawlings and Rubio, 2005).

Though these studies have thoroughly investigated the direct effects of CCT programs, less research has been done on the externalities associated with the programs. An income transfer to low-income households has the potential to significantly impact both household decision-making and community structures in targeted areas. While there are many possible externalities associated with these transfers, this report explores the impact of these public transfers on household credit behavior in Colombia. This is done

by making use of survey data from poor households that participated in *Familias en Acción*, a quasi-experimental CCT program in Colombia.

Theoretically, participation in a conditional cash transfer can have ambiguous effects on a household's decision to utilize credit. On one hand, cash transfers have the potential to increase a household's assets making it more creditworthy to lenders than it had been in the past. The transfer may also work as insurance for the household, allowing it to tolerate higher risk and profit from riskier, more productive technology. On the other hand, the additional influx of money may prompt families to pay back any outstanding loans and possibly start a small savings. The transfer may replace the household's need for credit, thereby negating the need for a loan. In fact, the additional income may end up neither as savings nor as collateral for credit, but instead, be spent completely on increased consumption. Empirically, the effect of conditional cash transfers on recipient households in poor communities in Colombia has yet to be determined.

Changes to credit can have varying impacts on households. Loans grant borrowers the opportunity to exploit investment activities and smooth consumption beyond what the transfer itself permits. Increased demand for local credit could expand the credit market in areas which traditionally lack market access, thereby bringing in relatively cheaper credit products to the benefit of the entire community. Alternatively, increased participation in the local credit market may have harmful effects on households, allowing them to fall into vicious cycles of debt with which they are incapable of dealing.

Lacking adequate data to determine whether changes to credit market participation affect households negatively or positively, this study instead focuses on how the Colombian CCT program affects participant household credit behavior. By credit

behavior, I refer to a family's likelihood to change its loan balance as a result of the program. These behaviors may change based on adjustments within the household due to the cash transfers or because of changes external to the household, such as modifications to credit markets in recipient communities, which are a result of the program.

II. Literature Review

This research question is relevant to study because conditional cash transfer programs have spread at a prodigious rate throughout developing countries. While their intended effect is to support child health and education to promote investment in human capital and long-term economic growth among impoverished populations, there are unintended (and uninvestigated) externalities of the programs as well. As these programs continue to spread to more countries around the world, understanding the full impact of their implementation will be critical in applying them effectively and responsibly. How these cash transfers affect all aspects of the household will be important in accurately evaluating the impacts and in considering the cost-benefit analysis of the program in each country.

The general literature on conditional cash transfers largely supports the direct benefits of the programs. Researchers view them as effectively reaching their targeted populations and having positive impacts on households. This is assessed extensively through the advanced monitoring and evaluation components which are integral to many of the programs. Rawling and Rubio (2005) evaluate the impact of conditional cash transfer programs in Colombia, Honduras, Jamaica, Mexico, Nicaragua, and Turkey. In their study, they collect evidence from country specific reports which show clear

evidence of program success in increasing school enrollment rates, improving preventative health care, and raising household consumption in first generation programs in Colombia, Mexico, and Nicaragua. Other studies report similar results in different countries (for example, Britto, 2005)

Specifically in Colombia, extensive studies were done by the government-hired data-collection and analysis organizations: the Institute for Fiscal Studies, Econometría Consultores, and Sistemas Especializados de Información. These organizations find that *Familias en Acción* positively affects treated households directly. To investigate the affects of the CCT on household consumption, child education and work choices, and health and nutrition, the organizations surveyed households three times over four years. The baseline survey was conducted in 2002, the first follow-up in 2003, and the second follow-up survey in 2006. Attansasio and Mesnard (2006) find that household consumption from the first follow-up survey increased by 15% compared with the baseline and that higher quality food was being consumed. Attansasio et al. (2008) show that school enrollment increased by 5 to 7% for students between the ages of 14 and 17, though less so for younger students who already boasted a high enrollment rate. The same report finds that 13% of young children and 10% of children 14 and older worked less as a result of the program. Additional reports describe improvements in child health, and an overall income increase of 11.7% in rural areas and 13.2% in urban areas (Attansasio, Battistin, Fitzsimons, Mesnard, and Vera-Hernández, 2005a). While these results all reflect the positive, direct impacts of *Familias en Acción*, the evaluations fall short in their examination of the indirect effects of this program because the

organizations appointed to collect and analyze the data were not commissioned to investigate these findings.

While the direct impacts of conditional cash transfer programs have been well researched across countries, less emphasis has been placed on the indirect effects. In one of the few investigations, Stecklov, Winters, Regalía, and Todd (2006) discover an adverse outcome of a conditional cash transfer program in Honduras. The program was structured with unplanned incentives that promoted fertility, causing the birth rate to rise by 2 to 4% among participants. These results demonstrate the importance of investigating outcomes that are neither intended nor anticipated by program structure and implementation. A different report by Anelucci and De Giorgi (2006) finds that the conditional cash transfer program *Progresa*, in Mexico, increased the consumption of non-eligible households living in treated communities. This positive externality is perceived as the result of the liquidity injections, which changed the dynamics of the credit and insurance markets in the poor, rural communities. The report explains that the households above the poverty-threshold of the program were able to reduce savings, increase credit, and improve consumption because of the additional liquidity within their informal, village network. These results provide encouraging implications for the positive impact transfer programs can have on whole communities, in addition to participants alone. From these two examples, it is clear that the small pool of currently existing literature illuminates both the positive and negative externalities of conditional cash transfer programs in differing countries.

Currently, no existing study looks into the effects of *Familias en Acción* on credit in Colombia. Several reports, however, have investigated transfer impact on household

credit decisions in other countries. Hernandez-Hernandez (2009) investigates both public and private transfer impact on credit markets. His theoretical analysis finds that the impact of conditional cash transfers on a household's decision to take out a loan is ambiguous. He determines that a household's decision to take out a loan is based on their expected marginal return on income and resources less the expected marginal cost of these resources. The transfer enters the equation as a resource. Depending on the household's cost-benefit analysis, the transfer income will either relieve liquidity and credit constraints on the family itself, thereby negating the need for loans, or it will improve the marginal expected return on the loans, increasing the household's incentive to apply for a loan. He also adds that transfers may change household behavior towards risk, as it reduces household volatility and may improve the household's creditworthiness to lenders. In his empirical analysis of Nicaragua's CCT program, he finds that public transfers did not have a strong enough effect to change the household's decision-making in regards to withdrawing credit. He gives several possible explanations for this finding such as the conditionality of the transfer, the precarious nature of the program's duration, and the inflexibility of transfers during times of shock may not allow transfers to serve as necessary insurance tools for households deciding to utilize credit. In a different study, Nielson (2010) finds that the CCT program in Nicaragua increased credit access in the informal lending sector but had no effect on access to formal financial institutions. This study attributes the increased creditworthiness of treatment groups within the informal network to the conditional transfer. My results will add to this specific literature on the effects of public transfers on credit accessibility and household decision-making.

There is a plethora of literature that discusses the benefits of improved credit access in poor households. Finding that *Familias en Acción* improves access to credit or changes the amount of debt being utilized by families could indicate that advancements to livelihood extend beyond those envisioned by the program. Household access to credit can allow beneficiaries to smooth consumption further than possible with just the program's transfers. It can also permit households to exploit investment opportunities they otherwise would not have taken. The main idea behind these findings is that poor households are generally liquidity- and credit-constrained. If households are credit-constrained, they are either completely excluded from the lending market, or they are unable to obtain their desired loan size due to credit rationing by capital-constrained lenders. When cash is injected into the household's resources, it may alleviate some of these constraints, allowing families to make different decisions about the financial tools they choose to utilize. This was true in Mexico, where access to capital encouraged further business investment. Gertler, Martínez, and Rubio-Codina (2006) find that 12% of *Progresa* beneficiaries in Mexico invested in small businesses. After five and a half years of the program, consumption for these families had increased by 34%, exceeding the expectations of the program. The authors determine that the transfer both alleviated liquidity and credit constraints and created income-insurance in households that were generally risk-averse. The security of the consistent transfer income led to an increased level of investment that improved long-term living standards in participant households. De Janvry, Sadoulet, and Davis (2001) conduct a similar study on a different program in Mexico and find that a cash transfer program, *Procampo,* had a 1.5 to 2.6 income

multiplier effect. The high multiplier highlights the income opportunities that remain unrealized by poor households because of the liquidity constraints they face.

Credit and liquidity constraints do not allow poor households to reach their full potential. Conditional cash transfers relieve liquidity constraints directly, but may also relax the constraints on credit by expanding resources in available credit markets, improving participant creditworthiness, and reducing income-volatility. This thesis will determine whether increased credit use is indeed an externality of Colombia's conditional cash transfer program, *Familias en Acción.*

III. Background

In 2000, the Colombian government received a loan from the Inter-American Development Bank and the World Bank to finance three welfare programs focused on fostering development and reducing poverty in Colombia. The largest of these programs, *Familias En Acción,* was inspired by Mexico's CCT program, *Progresa* (now called *Oportunidades*), which grants cash subsidies to poor households based on the households meeting certain educational and nutritional requirements. Colombia's conditional cash transfer program is based on three components: health, nutrition, and education. The health and nutrition transfers are granted to households with children under the age of seven, contingent on specific healthcare requirements. Mothers must take children to growth and development check-ups and ensure that the children are receiving all necessary vaccinations. In addition, mothers are encouraged to attend classes on hygiene, nutrition, and contraception. This subsidy is a flat-rate monthly transfer of 46,500 pesos

(approximately US$20.45)[1]. The education component is larger and awarded to families with children between the ages of 7 and 17. Targeted families receive subsidies if students attend class at least 80% of the time. Children enrolled in primary education are granted 14,000 pesos (US$6.15), and children in secondary school receive 28,000 pesos (US$12.30)[2]. The size of the education grant was selected to substitute, at least in part, the income foregone by the family if increased schooling resulted in a reduction in income-generated activities (Attansasio et al., 2004a). The program began in 2002 and was fully operating in treatment areas by 2003. By September 2004, 411,837 households (5% of the population) were benefiting from the program.

Eligibility for the program is first based geographically by municipality, then by household. Each eligible municipality is required to (i) have access to basic education and health infrastructure, (ii) have fewer than 100,000 inhabitants, (iii) have at least one bank, and (iv) be registered by a local authority that provides necessary documentation, including a list of all SISBEN1 beneficiaries within the municipality. SISBEN is a proxy means test index used widely in Colombia as a targeting system for social programs. It serves as an indicator of households' economic well-being and is a function of a set of variables related to the consumption of durable goods, human capital endowment, and current income (Veléz, Castaño, and Deutsch, 1998). The index divides households into strata based on the severity of poverty and is widely accepted as an accurate measure of household means.

Of the 1,024 municipalities in Colombia, 691 were eligible for the program. From these municipalities, targeted households are those that (i) have a Colombian citizen card,

[1] Amounts computed in 2002 prices, the amount of the subsidy changes each year in relation to inflation.
[2] Amounts computed in 2002 prices, the amount of the subsidy changes each year in relation to inflation.

(ii) have children under the age of 18, (iii) live in a municipality where the program is being implemented, and (iv) are registered as SISBEN1 beneficiaries as of December 1999. SISBEN1 indicates extreme poverty, thereby signifying that the program targets the poorest 20% of the country's population.

IV. Data

1. Data Collection[3]

The data for the evaluation of *Familias en Acción* was collected by the Institute for Fiscal Studies in collaboration with a Colombian research institute, Econometría Consultores, and data collection firm, Sistemas Especializados de Información. The data collection started in December 2001. While a randomized allocation of the program would have been the clearest way to observe the effects of the program, this was impossible for political reasons. Instead, the research consortium decided to conduct a quasi-experiment. They did this by constructing a representative stratified sample of treatment municipalities. Twenty-five strata were constructed based on an index of health and education infrastructure. Those untreated municipalities which were most similar to treated municipalities in terms of population, area, and index of life were selected as the control municipalities. The majority of control municipalities meet all requirements of the program except they do not have a bank. This specific characteristic of the matching process is potentially important considering the variable of interest and will be controlled for in the analysis. In other cases, the control areas have populations of just over 100,000 inhabitants.

[3] The information from this section comes from reports by the Institute of Fiscal Studies research team.

The sample was constructed of 122 municipalities – 57 treatment and 65 control. Within each municipality, approximately 100 households were selected to participate in the survey. Therefore, in the first round of interviews between June and October 2002, approximately 11,500 households participated. This first survey was intended to be a pre-program baseline survey, capable of giving a picture of the population of interest before intervention and of being utilized to control for pre-program differences between the treatment and control groups. Due to political pressure, however, 26 of the 57 treatment groups began receiving transfers before the fieldwork commenced. Therefore, there are two distinct treatment groups in the data – one that started receiving payments before the baseline survey called "early-treat" and one that had not, "late-treat."

Owing to the nature of data collection at the baseline, there are two ways to uncover the parameters of interest in the following analysis. First, the late-treatment group can be compared to the control group, thereby dropping the early-treatment group from the analysis and only including those groups with a true pre-treatment observation. Second, the whole treatment group can be compared to the control group. The strength of the first method is clear, as the true change in the outcome of interest can only be evaluated when including observations with pre-treatment states. Including the early-treat group into the analysis diminishes any immediate effects of the program on participants, as this group may have changed their credit behavior after receiving treatment and before participating in the baseline survey. Therefore, although I include both treatment groups in the summary statistics that follow, I exclude the early-treatment group from the regression results. For the remainder of the paper, unless specifically denoted, the term "treatment" refers to only those households in the late-treatment group.

There were two follow-up surveys conducted in later years. The first was completed between July and November 2003, one year after the baseline. Due to a great effort in tracking households during the second wave, 10,742 households were surveyed, an attrition rate of only 6%. The second follow-up was conducted in 2006, with data collected for 9,566 households. All of the information for this evaluation was collected from these three rounds of extensive household questionnaires.

The surveys contain information on household characteristics as well as data on municipality infrastructure. In the analysis, I control for observable characteristics across household. Household proxies include information on household demographics, household dwelling, type of home ownership, and household wealth, as measured by factors such as education level of the head, household consumption, and available assets. These household variables show that the treatment and control areas are similar along a wide-range of observed characteristics[4]. As can be expected from a non-randomized experiment, there are some statistically significant differences between the treatment and control groups. For example, households in treated areas are less likely to have piped gas, sewage, and trash collection services than control households, possibly reflecting a slightly higher standard of living in control areas. In addition, treatment households have significantly lower consumption levels and significantly higher debt related to home construction than control groups. As seen in Table A1, however, these differences are of a small magnitude, and the overall results indicate that households across the sample are indeed similar. Regardless, I use the characteristics listed in Table A1 to control for any differences between the treatment and control groups in the analysis.

[4] See appendix Table A1 for a full-list of control variables.

2. Loan Data

The data on loans and household debt is a small section on the surveys. During the baseline, the main questions about debt focused on the amount of loans used for the purchase and construction of the home. There was, however, a separate question inquiring into the balance of household debt outside of that used for home construction. This value, which I refer to as loan balance outstanding in Colombian pesos, is available at all three rounds for both treatment and control groups and is the main variable for this evaluation. This variable describes the amount of debt that the household has not yet repaid at the time of the survey. Other loan characteristics such as the use of the loan, the type of creditor, the size of the loan request, and the repayment schedule were included in the follow-up surveys, but not in the baseline report. These variables, therefore, cannot be included in the main regression but capture relevant summary statistics on household decision-making in regards to credit.

Using the loans outstanding variable, I evaluate how household credit balances change as a result of the program. More qualitative questions regarding specific household credit decisions would have supplemented and more fully explained the results of the data. Additionally, more questions specifically focusing on loan demographics, such as size, interest rate, repayment type, and loan period, would have more clearly described the credit changes resulting from the program. Instead, the data explores the effect of cash transfers on the amount of outstanding debt held by participant households and gives some insight into the credit constraints faced in these poor communities.

V. Summary Statistics

1. Household Loan Balance Outstanding Statistics

Summary statistics from the data collected on household debt draws to light several preliminary findings. Table 1 shows the percent of households holding loans at each period. Table 2 displays the mean balance of loans outstanding for all households holding credit at each period. Therefore, Table 2 excludes households not holding debt, as households with no debt outstanding have a value of zero. Including these zero-balance households would greatly reduce the true mean amount of outstanding credit. Table 1 and 2 are divided into groupings based on control and treatment areas. The analyses are done across urban and rural areas separately.

Table 1
Percentage of sampled households with loans outstanding

	Baseline	**First follow-up**	**Second follow-up**
Total	65.9	75.9	75.2
Urban	63.1	73.2	71.9
Rural	67.9	77.7	77.4
Control	70.7	76.7	78.0
Urban	68.9	76.4	77.0
Rural	71.7	77.2	78.5
Late-treatment	56.9	76.6	74.2
Urban	56.3	71.9	69.0
Rural	57.4	80.0	77.8
Early-treatment	67.7	74.0	72.3
Urban	62.7	70.1	69.1
Rural	72.4	77.6	75.2

Table 1 illustrates that a high percentage of the sample holds credit and that the percentage of households with credit increases from the baseline to the follow-ups. The control group has a higher percentage of households with credit than the treatment groups at every period. This may indicate that there are differences in loans outstanding between treatment and control groups at the baseline that will need to be controlled for in the

evaluation. In addition, rural areas tend to have a higher percentage of households utilizing debt than their urban counterparts. This may be attributable to a variety of factors including the lending markets available in the respective areas.

Table 2
Mean balance of loans outstanding in thousands of Colombian pesos as reported by households with a positive loans outstanding balance[5]

	Baseline Mean (Std. Error)	**First follow-up** Mean (Std. Error)	**Second follow-up** Mean (Std. Error)
Total	**1,169 (2,623)**	**1,262 (5,384)**	**1,315 (2,848)**
Urban	1,319 (2,612)	1,440 (5,365)	1,629 (3,204)
Rural	1,070 (2,626)	1,145 (5,394)	1,117 (2,580)
Control	**1,184 (2,384)**	**1,344 (5,821)**	**1,359 (3,059)**
Urban	1,264 (2,245)	1,618 (6,497)	1,567 (3,079)
Rural	1,141 (2,456)	1,188 (5,395)	1,247 (3,042)
Late-treat	**1,311 (3,296)**	**1,304 (5,523)**	**1,373 (2,974)**
Urban	1,570 (3,158)	1,493 (4,976)	1,966 (3,893)
Rural	1,130 (3,379)	1,184 (5,844)	1,016 (2,173)
Early-treat	**1,041 (2,338)**	**1,111 (4,563)**	**1,120 (2,380)**
Urban	1,205 (2,553)	1,201 (4,141)	1,439 (2,698)
Rural	906 (2,138)	1,036 (4,885)	1,005 (2,067)

From Table 2, we see that the size of outstanding debt increases over every period for each group, except for the late-treat group in the first follow-up. It is important to note the large size of the standard errors on these statistics. I will control for this large variation in the regressions by creating a log loans outstanding variable, thereby making the data more manageable. The size of outstanding credit increases from 1,184,328 pesos (US$526.37) in the baseline to 1,344,021 pesos (US$537.61) in the first follow-up to 1,359,241 pesos (US$543.70) in the second follow-up for the control group. In the late-treat group, the average size of loans outstanding increases from 1,310,716 pesos (US$524.29) in the baseline to 1,304,330 pesos (US$521.73) in the first follow-up to 1,372,775 pesos (US$549.11) in the second follow-up. This indicates that households

[5] Amounts computed in 2008 prices.

utilizing loans are increasing the size of their debt across both treatment and control groups over the period. The following analysis will determine whether this is happening significantly more in the treatment than in the control group, albeit whether participation in the program impacts these trends.

2. Other Loan Statistics

Other loan statistics are available only for the first and second follow-up periods, as these specific questions were not asked in the baseline survey. This information contributes to the overall understanding of the Colombian lending market in the sampled communities.

A high percentage of households in this sample utilize alternative lending options as opposed to formal financial institutions. By alternative lending options, I refer to informal sources of credit such as that from friends, family members, neighbors, and other non-bank affiliated lenders within the community. Due to the low-income level of these families, this is an expected statistic, as very poor households often do not meet the qualifications for credit in formal banks. Instead, problems associated with small loan requests, moral hazard, and adverse selection result in the reliance on informal lending markets in poor communities. Table 3 was constructed from results from the first and second follow-up surveys.

Table 3
Source of loans as reported by sampled households with loans outstanding

	First follow-up				Second follow-up			
	Control		Treatment		Control		Treatment	
	Urban	Rural	Urban	Rural	Urban	Rural	Urban	Rural
Financial institution	3.32	4.04	4.80	5.11	8.46	11.27	10.8	11.91
Family/friend, neighbor	63.70	63.77	64.32	63.46	59.06	59.26	56.37	57.37
Other	32.98	24.03	30.87	23.80	35.17	31.35	30.14	28.83

Table 3 suggests that informal lending sources are used more frequently than formal institutions by the sampled groups in Colombia, as approximately 64% of households report utilizing family, friends, and neighbors and only 4.5% use formal institutions in the first follow-up. The "other" option is not defined in the surveys, but it is likely associated with other forms of informal lending sources such as moneylenders or community lending schemes. This indicates that as much as 95% of the sample uses informal lending options in the first follow-up survey and 88% in the second follow-up survey. The cash injections of the program, therefore, have the potential to significantly impact these informal lending networks.

In Table 4, the percent of households with savings in either a bank or more informal source is listed. The results show that a very low percentage of the sampled population utilizes savings. These statistics are predictable, as it is often very difficult for low-income households to maintain savings. As savings and credit can be used as substitutes, it is clear from comparing the results of Table 1 and Table 4 that the sample in this evaluation prefers using credit to savings.

Table 4
Percent of sampled households that reported savings

	Baseline	First follow-up	Second follow-up
Total	3.32	4.18	3.15
Urban	2.90	3.72	3.47
Rural	3.22	4.41	2.93
Control	3.21	5.16	2.55
Urban	2.69	5.51	2.01
Rural	3.08	4.96	2.84
Late-treatment	3.02	3.99	5.31
Urban	3.07	3.26	7.06
Rural	3.87	4.52	4.13
Early-treatment	3.71	2.86	1.99
Urban	2.95	2.27	2.16
Rural	2.81	3.39	1.85

Table 4 shows that no more than 8% of households have savings available at any period in any group. On average, only 3.32% of households have savings at the baseline, 4.18% in the first follow-up, and 3.15% in the second follow-up. The effects of the program on savings are not within the scope of this report. However, because only a small percentage of households in this sample use savings and because there is very little variation in the number of households using savings between treatment and control groups, the effects of the program on credit are more relevant to this sample than the effects on savings.

It is also useful to understand for what households utilize credit. These uses are reported in Table 5 by treatment and control groups, and by urban and rural divisions. These results were collected in the first follow-up survey.

Table 5
Percent of households in the first follow-up survey that reported using their last loan for the following reasons

	Total			Control			Treatment		
	Total	Urban	Rural	Total	Urban	Rural	Total	Urban	Rural
To buy food	23.7	23.4	24.0	19.4	19.3	19.5	26.0	25.1	26.8
For home expenditures	23.1	23.4	23.0	21.6	21.0	21.9	24.0	24.4	23.6
To cover education expenses	7.1	6.0	8.0	7.6	6.3	8.2	6.8	5.8	7.8
To cover for an emergency	31.5	32.2	31.0	32.4	33.0	32.0	31.1	31.9	30.4
Other	14.6	15.0	14.0	19	20.4	18.4	12.1	12.8	11.4

From Table 5, it is clear that, generally speaking, control and treatment groups employ loans for similar uses. Additionally, rural and urban households utilize debt in similar ways. Approximately 31% of all households report using loans to cover in an emergency, between 20 and 25% of households use loans to buy food, and another 23% utilize credit for household expenditures. This indicates that households with outstanding debt are utilizing it for basic necessities, and that in these communities with such low savings, loans are being used as a way to smooth consumption and hedge against emergencies.

VI. Program Impacts

1. Methodology

To measure the impact of the program on household outstanding loan balance, I would like to observe the effect on the treatment group both with and without the program. This, however, is impossible given that the outcome of interest cannot be

observed without treatment in treatment areas. This problem is addressed by including into the evaluation a control group that is used to estimate the counterfactual.

I begin, therefore, by comparing the post-program differences between treatment and control groups at both follow-up phases. I compare the outstanding loan balance of these groups at the first follow-up[6],

$$(1)\ \Delta = E(Y_{T,1}|W=T,X) - E(Y_{C,1}|W=C,X)$$

where $Y_{i,j}$ is the dependent variable loans outstanding with i indicating treatment (T) or control (C) and j denoting whether the observation is monitored at the baseline (0), first follow-up (1), or second follow-up (2). W denotes T for treated groups and C for control groups, and E indicates expected value. This is done conditionally on a range of observable characteristics, X, that may affect the dependent variable[7].

This simple difference has limitations because it assumes that the estimated difference is fully attributable to the program. Because there is the possibility of unobserved characteristics, which are not related to the program, affecting credit outcomes, this difference estimator risks confusing effects of the program with non-program related unobserved factors that affect credit. As control municipalities were selected to match treatment municipalities, the sample under investigation is not a random sample. In many cases, control municipalities were eligible for the program across all criteria except that they did not have a bank. As this criterion is directly

[6] The same equation holds for the second follow-up simple difference by replacing j=1 with j =2.
[7] See Appendix Table A1 for the full-list of control variables used.

relevant to the variable of study, but may be the result of unobservable characteristics within the communities, it must be controlled for in the evaluation.

To control for these unobserved characteristics, I use a difference-in-differences approach and guarantee that pre-existing dissimilarities, such as the number of banks in treatment and control municipalities at the baseline, do not affect the estimated impacts of the program. This estimator measures the average change in Y in the treatment group over the course of the experiment minus the average change in Y in the control group over the same time.

$$(2)\ \Delta = [E(Y_{T,1}|W=T,X) - E(Y_{T,0}|W=T,X)] - [E(Y_{C,1}|W=C,X) - E(Y_{C,0}|W=C,X)]$$

The assumption behind this analysis is that the treatment and control groups have common time effects and, therefore, would have changed in the same amount had the program not been implemented. Although there is no retrospective data available on household credit, results from previous studies on *Familias en Acción* reveal several analyses performed on the same sampled areas to reassure critics of the legitimacy of this assumption. In Attanasio et al. (2008), the authors show that household income per capita in the three years prior to the program was very similar in treatment and control areas. While income per capita does not relate directly to the outcome variable under study, it is relevant enough to appease some concerns regarding the authenticity of this assumption. The same study then proves that pre-program data collected from household-level surveys by the Demographic Health Surveys (DHS) in 1990, 1995, and 2000 is not statistically different across treatment and non-treatment groups at the five percent level

of significance. This again assures cynics that there are similar time effects between these two groups, at least in the periods preceding the implementation of the program. A second assumption I make with this analysis is that any differences in loan usage that are due to unobserved factors between the treatment and control groups are fixed overtime. This assumption, known as the common-trends assumption, is accepted given the similarities between treatment and control household and municipality characteristics at the baseline.

With this difference-in-difference analysis, I evaluate the change in loans outstanding between these periods, not simply the levels of the variables before and several years into the program. The strength of this test is its ability to eliminate pre-treatment differences in Y by focusing on the change in Y over the course of the experiment. This removes the influence of initial values of Y that vary systematically between treatment and control groups. From this, I can isolate the actual effect of the program on the dependent variable.

2. Empirical Specifications

To evaluate the effect of the program on household credit balance, I look at the change in loans outstanding for each household over the three periods of data. I assume that the data does not suffer from omitted variable bias, contingent on controlling for a detailed list of household characteristics. I also assume that the treatment and control areas are effectively identical in nature and that no unobserved factors affect the outcome of interest differently in treated and control areas. Because of similarities in observed

characteristics and the care taken in matching control municipalities to the selected treatment municipalities, these assumptions are accepted for the following analysis.

Because the data holds information from three rounds of surveys but a difference-in-differences approach only allows for analysis across two, I compare between the baseline and first follow-up, the baseline and second follow-up, and the first and second follow-ups separately. Consequently, I utilize three similar, yet distinct equations in my analysis. The effect can be estimated from slight variations of the following simple regression[8]:

$$(3)\quad Y = b_0 + b_1 T*F + b_2 T + b_3 F + X\, b_3 + \bar{e}$$

where Y is the loan balance outstanding variable, T is a dummy that equals 1 when the observation is from a treatment household and 0 otherwise, F is the dummy that equals 1 if the observation occurred at a follow-up stage[9] and 0 otherwise, T*F is a dummy that equals 1 if the observation is a treatment household *and* is observed in a follow-up stage[10] and 0 otherwise, and X is the set of all control variables. The difference-in-differences point estimate of the program is given by b_1.

The three estimations derived from equation (3) attribute to an overall understanding of the change in the outcome of interest over the evaluation period. While comparisons between the baseline and first follow-up describe the effect of the program

[8] This is the generic notation used to derive the three equations utilized in this paper.
[9] When comparing across the baseline and first follow-up, the baseline and the second follow-up, and the first and second follow-ups, this dummy variable represents 1 if the observation occurred in the first follow-up, second follow-up, and second follow-up respectively. The round not included in the comparison is dropped.
[10] This notation holds as described in the previous footnote.

in the short term (over one year), the comparison between the baseline and second follow-up explain the effect in the long term (over four years). Comparisons between the first and second follow-up can give insight into the magnitude of the effects several years into the program especially when compared to the magnitude of the effects initially.

VII. Results

1. Regression Findings

For loans outstanding, I estimate the effect of the program using several different methodologies. At every step, I do this across the sample as a whole, then for households in urban and rural areas separately. All regressions are run across the three comparison rounds. In each of the three rounds, I present the results of the regression when (1) withholding control variables, (2) using all controls except for region, and (3) controlling for all observable characteristics.

In Table 6, I report the estimates of the program on log loans outstanding. Because of the large range of values and high standard errors across the loans outstanding variable, I take the logged value of the outcome in order to analyze the data on a manageable scale[11]. Due to the high concentration of zero observations, I use the log (loans outstanding +1) to ensure that all observations are defined[12].

[11] The loans outstanding variable is converted to pesos in 2008 prices.
[12] For the remainder of the paper, when log loans outstanding is listed, it refers to this log (loan outstanding + 1) variable.

Table 6

Effects of *Familias en Acción* on log loans outstanding

	Baseline vs. first follow-up			**Baseline vs. second follow-up**			**First vs. second follow-up**		
	1	2	3	1	2	3	1	2	3
Total	1.720**	1.632**	1.647**	1.226**	1.215**	1.224**	-0.494*	-0.426*	-0.437*
	(0.201)	(0.0.198)	(0.198)	(0.209)	(0.205)	(0.204)	(0.199)	(0.196)	(0.195)
Urban	0.864**	0.754*	0.788*	0.593	0.640	0.640	-0.271	-0.146	-0.184
	(0.329)	(0.321)	(0.320)	(0.347)	(0.338)	(0.337)	(0.333)	(0.235)	(0.323)
Rural	2.286**	2.197**	2.192**	1.647**	1.603**	1.610**	-0.640**	-0.585*	-0.578*
	(0.252)	(0.298)	(0.251)	(0.261)	(0.257)	(0.257)	(0.249)	(0.245)	(0.244)
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Region FE	No	No	Yes	No	No	Yes	No	No	Yes
No. of observations									
Total	14656	14656	14656	13854	13854	13854	13388	13388	13388
Urban	5615	5615	5615	5261	5261	5261	5084	5084	5084
Rural	9041	9041	9041	8593	8593	8593	8304	8304	8304

** denotes statistical significance at the 1% level or less

* denotes statistical significance at the 5% level

Note: Standard errors included in parenthesis

List of controls: Number of people in household^{+}, number of children under 7^{+}, number of children 7-11^{+}, number of children 12-17^{+}, urban household, private insurance, age of head^{+}, single parent, number of female adults^{+}, household head level of education, material of home walls, has piped gas, has piped water, has sewage, has trash collection, has a toilet connected to sewage system, owns a home, household member died in 2001, household member suffered from illness in past year, household suffered from violence in past year, log household consumption^{+}, log household savings^{+}, log home loan outstanding^{+}, lives in Eastern region, lives in Pacific region, and lives in Central region. All variables except those denoted by $^{+}$ are dummy variables. For the dummy variables, proportions of households answering "yes" are reported.

The estimations from Table 6 approximate the percent change in the amount of loans outstanding over the given period. The effects observed between the baseline and the first follow-up are relatively large and significant. When excluding all control variables as in column 1, the loans outstanding balance increase by 228.6% in rural areas and by 86.4% in urban areas. The loans outstanding balance, when controlling for all control variables except region, increases by 219.7% in treated rural areas and by 75.4% in treated urban areas compared to control areas. As is shown in column 3, when all control variables are included, rural areas increase their outstanding loan amount by 219.2% and urban areas increase by 78.8%. This indicates that in the short run, households in both urban and rural areas utilize more loans after entering the program than those households not participating in the program.

As discussed above, there is some variation between the point estimates predicted in the three columns of each period. The most accurate results are those displayed in column 3 for each round, which controls for all observable household characteristics. Because this is not a random sample, there are notable differences between the control and treatment groups before the start of the program, and it is important to control for these differences in the analysis. Therefore, for the remainder of the results, I focus primarily on the estimates from column 3, as they provide the most precise specifications for determining the effects of the program on loans outstanding balance.

From the baseline to the second follow-up, there are similar increases in the loans outstanding balance in rural treated areas. The size of the loans variable increases by 161% in rural areas. However, loan balance does not change significantly in treated urban areas over the four-year period. Finally, I note a 57.8% decrease in the amount of loans being utilized in rural areas between the first and second follow-up surveys. This indicates that rural households increased their balance over the four year period, but significantly more so in the first follow-up than in the second. The result is a significant decrease in the amount of loans outstanding balance in rural households between the first and second follow-up. The change in urban areas between the follow-up surveys is also negative but insignificant.

In Table 7, I measure the outcome of the program on loans outstanding as a dummy variable. If the household has a positive outstanding loan balance, the household is given a value of 1. If the household has no outstanding loans, the household is given a value of 0. This technique determines whether treatment households are more likely to

take out loans if they did not have any at the baseline, or are more likely to pay off outstanding debt as a result of the program compared to control households.

Table 7
Effects of *Familias en Acción* on loans outstanding as a dummy variable

	Baseline vs. first follow-up			**Baseline vs. second follow-up**			**First vs. second follow-up**		
	1	2	3	1	2	3	1	2	3
Total	0.137**	0.132**	0.133**	0.100**	0.099**	0.100**	-0.036*	-0.033*	-0.034*
	(0.015)	(0.015)	(0.015)	(0.016)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
Urban	0.073**	0.066**	0.068**	0.046	0.049	0.048	-0.026	-0.020	-0.023
	(0.024)	(0.024)	(0.024)	(0.026)	(0.025)	(0.025)	(0.025)	(0.024)	(0.024)
Rural	0.179**	0.174**	0.173**	0.137**	0.133**	0.134**	-0.042*	-0.040*	-0.039*
	(0.019)	(0.019)	(0.019)	(0.020)	(0.020)	(0.020)	(0.019)	(0.019)	(0.019)
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Region FE	No	No	Yes	No	No	Yes	No	No	Yes
No. of observations									
Total	14656	14656	14656	13854	13854	13854	13388	13388	13388
Urban	5615	5615	5615	5261	5261	5261	5084	5084	5084
Rural	9041	9041	9041	8593	8593	8593	8304	8304	8304

** denotes statistical significance at the 1% level or less
* denotes statistical significance at the 5% level
Note: Standard errors included in parenthesis
Refer to Table 6 for full-list of control variables

The results from Table 7 describe the percent change in the number of households holding credit over the indicated period. The estimate captures the number of households who start with a zero balance and take out credit after the baseline, compared to those that pay off outstanding loans from the baseline and have a zero balance at the follow-up phases between the treatment and control groups. Households that did not change their loan status are essentially excluded from the estimator. By looking at the difference-in-differences estimate, it can be determined that involvement in the program increases the number of households that utilize credit from the baseline to follow-up periods. From the baseline to the first follow-up, the number of households holding credit increases by 17.3% in treated rural and 6.8% in treated urban areas compared to the control groups. Between the baseline and second follow-up period, treated rural areas see a 13.4% increase while the change in urban areas is insignificant. In addition, there is a significant

decrease in the percent of households holding credit in rural areas between the first and second follow-up. The results indicate that 3.9% fewer people hold credit in the second follow-up than in the first in rural areas. These results are similar to those found in Table 6 for this period in rural areas. The estimate in urban areas for this period is also negative, but not significant at the 5% level.

In Table 8, I compute the results of the program as log loans outstanding but constrain the sample to only those households who had a loan balance of zero at the baseline. This gives insight into the magnitude of change in loans outstanding of the households not holding credit before the start of the program. Performing this analysis isolates the change in loans outstanding for those households who decide to utilize loans only after the start of the program from the overall change in loans outstanding. While some households *choose* to remain outside of the credit market, others are excluded from participating. This analysis allows me to see if this exclusion is alleviated by participation in the program and if credit access is improved.

Table 8

Effects of *Familias en Acción* on log loans outstanding when constraining the sample to only those households with a zero loan balance at the baseline

	Baseline vs. first follow-up			**Baseline vs. second follow-up**			**First vs. second follow-up**		
	1	2	3	1	2	3	1	2	3
Total	0.711**	0.678**	0.701**	0.017	0.070	0.070	-0.695	-0.689	-0.723*
	(0.241)	(0.240)	(0.239)	(0.241)	(0.240)	(0.239)	(0.366)	(0.358)	(0.355)
Urban	-0.379	-0.416	-0.358	-0.900*	-0.768*	-0.783*	-0.521	-0.409	-0.503
	(0.386)	(0.388)	(0.386)	(0.391)	(0.388)	(0.387)	(0.593)	(0.580)	(0.575)
Rural	1.513**	1.482**	1.485**	0.661*	0.702*	0.707*	-0.853	-0.807	-0.807
	(0.305)	(0.305)	(0.305)	(0.305)	(0.303)	(0.302)	(0.460)	(0.453)	(0.452)
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Region FE	No	No	Yes	No	No	Yes	No	No	Yes
No. of observations									
Total	5104	5104	5104	4802	4802	4802	4624	4624	4624
Urban	2053	2053	2053	1912	1912	1912	1843	1843	1843
Rural	3051	3051	3051	2890	2890	2890	2781	2781	2781

** denotes statistical significance at the 1% level or less
* denotes statistical significance at the 5% level
Note: Standard errors included in parenthesis
Refer to Table 6 for full-list of control variables

Table 8 presents the percent change in the credit level of households that had no loans outstanding before the program. A positive coefficient indicates that within this constrained sample the outstanding loan balance increased in the treatment groups relative to control groups after the baseline. A negative coefficient indicates that the outstanding loan balance increased in control groups relative to treatment groups after the baseline. When comparing treatment and control groups, there is a 148.5% increase in the loans outstanding balance in rural areas and an insignificant decrease in urban areas between the baseline and first follow-up. Between the baseline and second follow-up, there is a 70.7% increase in the log loans outstanding balance in rural areas, and a 78.3% decrease in the loans outstanding amount in urban areas. This indicates that households with a zero-balance at the baseline in treated rural areas increase their loan balance more than control groups, while in urban areas the control groups increase their loan balance more than the treatment groups. This could imply that, in the long run, the program

negates the need for loans in treated urban households that hold no credit at the baseline. There are no significant differences between the first and second follow-up for urban or rural areas.

In Table 9, I measure the results of the program on loans outstanding as a dummy variable, but constrain the sample to only those households with a loan balance of zero at the baseline. If the household has a positive outstanding loan balance in the first or second follow-up, the variable takes a value of 1. If the household has no outstanding loans in these rounds, the variable is given a value of 0. Because of the constrained sample, all households have a value of 0 at the baseline. This analysis allows for further discussion of changes to credit access in treated areas.

Table 9

Effects of *Familias en Acción* on loans outstanding as a dummy variable when constraining the sample to only those households with a zero loan balance at the baseline

	Baseline vs. first follow-up			**Baseline vs. second follow-up**			**First vs. second follow-up**		
	1	2	3	1	2	3	1	2	3
Total	0.052**	0.050**	0.052**	0.006	0.010	0.010	-0.046	-0.046	-0.049
	(0.019)	(0.019)	(0.019)	(0.019)	(0.013)	(0.019)	(0.029)	(0.028)	(0.029)
Urban	-0.036	-0.039	-0.035	-0.074*	-0.066*	-0.067*	-0.038	-0.030	-0.038
	(0.031)	(0.031)	(0.030)	(0.030)	(0.030)	(0.030)	(0.046)	(0.046)	(0.045)
Rural	0.117**	0.115**	0.115**	0.062**	0.065**	0.066**	-0.055	-0.052	-0.052
	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.036)	(0.036)	(0.036)
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Region FE	No	No	Yes	No	No	Yes	No	No	Yes
No. of observations									
Total	5104	5104	5104	4802	4802	4802	4624	4624	4624
Urban	2053	2053	2053	1912	1912	1912	1843	1843	1843
Rural	3051	3051	3051	2890	2890	2890	2781	2781	2781

** denotes statistical significance at the 1% level or less
* denotes statistical significance at the 5% level
Note: Standard errors included in parenthesis
Refer to Table 6 for full-list of control variables

The variable of interest in Table 9 denotes the percent change in the number of households that, after holding no credit at the baseline, decide to utilize loans in later periods. While a negative coefficient is not initially intuitive, it simply indicates that

more control households in this sample utilize loans than treatment households after the baseline. This variable can give some implications into credit access, as a possible agent holding credit equal to zero at the baseline is access. While there are other possible explanations, a large increase in treated households taking out loans following the enactment of the program could imply that the cash transfer alleviates household credit constraints and encourages households to utilize loans.

The results indicate that there was an 11.5% increase in the number of treated rural households that decided to utilize credit in the first follow-up and a 6.6% increase from the baseline to second follow-up. For treated urban households, the change is insignificant in the first follow-up and negative in the second follow-up. The findings indicate that among households with a zero loan balance at the baseline, the number of control urban areas holding debt at the second follow-up increases by 6.7% compared to treatment areas. There are no significant differences between the first and second follow-up when controlling for observable characteristics between the two groups.

2. Robustness Check

To verify the robustness of the results, I run the same regressions as above but utilize a censored regression model. Censored regression models apply when the variable to be estimated is partly continuous but has a positive probability mass at one or more points. This is the case in this sample, as approximately 30% of households do not hold credit and, therefore, have a credit balance of zero. Because the negative loans outstanding value (savings at the lending interest rate) is not observed, the dataset contains a latent variable and is censored below at zero.

The Tobit regression model is a frequently used tool for modeling censored variables in econometrics research. I use this model to control for the censor bias in the sample and to verify my results. Although the coefficients from a Tobit model cannot be interpreted directly from the results, the sign of the coefficients and the significance of the estimates should be similar to my results from the OLS model.

As with the OLS estimations from above, I compute the Tobit estimations across all three rounds (1) without control variables, (2) with all controls except region, and (3) with all observable characteristics of the sample. The estimations are calculated across the whole sample then for urban and rural areas separately. Table 10 illustrates the effects of the program on the log loans outstanding variable using a Tobit regression.

Table 10

Effects of *Familias en Acción* on log loans outstanding using a Tobit regression

	Baseline vs. first follow-up			**Baseline vs. second follow-up**			**First vs. second follow-up**		
	1	2	3	1	2	3	1	2	3
Total	1.838**	1.679**	1.654**	1.623**	1.620**	1.631**	-0.436*	-0.312	-0.295
	(0.230)	(0.233)	(0.234)	(0.241)	(0.245)	(0.246)	(0.212)	(0.216)	(0.217)
Urban	0.555	0.336	0.340	0.718	0.538	0.533	-0.100	-0.046	-0.093
	(0.381)	(0.496)	(0.498)	(0.399)	(0.411)	(0.417)	(0.345)	(0.356)	(0.361)
Rural	2.656**	2.475**	2.468**	2.197**	2.146**	2.156**	-0.727**	-0.570*	-0.554*
	(0.288)	(0.292)	(0.292)	(0.302)	(0.308)	(0.308)	(0.270)	(0.273)	(0.273)
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Region FE	No	No	Yes	No	No	Yes	No	No	Yes
No. of left-censored obs.									
Total	3628	3628	3628	3100	3100	3100	3404	3404	3404
Urban	1436	1436	1436	1199	1199	1199	1323	1323	1323
Rural	2192	2192	2192	1901	1901	1901	2081	2081	2081

** denotes statistical significance at the 1% level or less
* denotes statistical significance at the 5% level
Note: Standard errors included in parenthesis
Refer to Table 6 for full-list of control variables

The estimates from Table 10 have the same sign as the estimates from the OLS regression in Table 6. In addition, the significance of the estimates is similar across the two tables. This indicates that the results from Table 6 are robust. The three other

regressions I describe in my results all control for any bias associated with this censored variable. Table 7 and 9 use a dummy variable as the dependent variable and, therefore, do not suffer from censored variable bias. The constraints imposed on the sample in Table 8 also control for this bias.

Because the results from the Tobit regression are similar to the OLS estimates derived earlier in this paper, I can verify that the results found through the difference-in-differences model are accurate and robust.

3. Discussion

There are several important findings to be drawn from the results. The first is the difference between rural and urban communities in respect to the effects of the program on loan balance outstanding. These differences may be attributed to a number of factors that will be analyzed in the Possible Explanations section. The second important finding is the timeline of the effects of the program. It appears the effects are most prevalent after one year of the program. The third finding is the overall significance of the data. The program does have externalities related to household credit behavior.

In rural areas, there is a significant increase in the amount of loans being used by treated households and in the number of treated households utilizing credit between the baseline and follow-ups. I note a 219.2% increase in the loans outstanding balance in treated households compared to control households from the baseline to the first follow-up; 148.5% of this increase is attributed to new borrowers and the rest to increased balances in households formerly holding debt. In addition, there is a 17.3% increase in the number of households holding credit from the baseline to first follow-up, and an

11.5% increase in the number of households with no debt at the baseline that take out loans after receiving treatment. Relative to the control group, these rural treated areas are increasing both involvement in and utilization of the loan market even after just one year of the program.

Results in rural areas over the long run are similar, but to a lesser degree. Over the four-year period, the level of loans outstanding increases by 161% in treated areas. Compared to new borrowers in control areas, new borrowers in treatment areas increase their loan balance by 70.7% over the period. There is also a 13.4% increase in the number of all households utilizing credit and a 6.6% increase within households that did not utilize credit at the baseline. However, between the first and second follow-up, there is a significant decrease in both the size of the debt outstanding and in the amount of households increasing their loan activity. This indicates that the effects of the program on credit are felt more strongly at the initiation of the program than several years into the program. While increased credit involvement is a long-run trend in rural areas over the four-year period, the magnitude of the effect is in decline as the initial impact of the program fades.

The results differ in urban areas. There is not a significant change in the loan balance of treated urban households over the four years of the program. There is, however, a loan outstanding increase of 78.8% between the baseline and first follow-up and a 6.8% increase in the number of households holding credit over that period. Neither of these results is significantly attributable to new entrants in the lending market. Instead, I find that the increase in loan balance is almost completely caused by former borrowers utilizing more debt. From the baseline to the second follow-up, households with a zero

loan balance at the baseline actually take out fewer loans and enter the credit market more infrequently than households in the control group. This could signify that urban households with a zero-balance at the baseline maintain their excluded status regardless of participation in the program or that the need for loans decreases in this group after participating in the program.

Generally in urban areas, there is a preliminary increase in credit involvement in treated households, however, the trend does not continue significantly into the second follow-up survey. Households with zero-balance at the baseline even decrease their credit involvement compared to control groups in the second follow-up, indicating that the program increases some urban households' credit participation initially, but the effect is neither indiscriminate nor lasting in the long run.

The second interesting finding is the negative trend between the first and second follow-up. Though the estimates are only significant in rural areas, the entire sample has negative coefficients in this period, implying that the effects of the program on credit are felt most dramatically during the early stages of the program. When comparing between pre-treatment and post-treatment states, there are clear increases in both credit involvement and loan amount. However, the negative estimates between the follow-up rounds indicate that households enter into a more credit-involved status following the start of the program, but do not maintain this same level of involvement in the long run. It appears the program enables households to enter the credit market, but does not increase their participation perpetually. Instead, the program acts as a boost into an increased state of credit use that declines throughout the evaluation period.

The general finding is that *Familias en Acción* does impact households beyond those effects directly intended by the program. Households receiving transfers change their credit behavior relative to similar households not receiving treatment. The effects are more dramatically felt in rural areas, but increased loan balance and credit market involvement are significant impacts felt across the entire sample in the short run. Whether increased credit involvement is a positive or negative externality is not within the scope of this paper. However, policy makers should consider this effect when expanding this program to other areas in Colombia or when adapting similar programs in other countries.

IX. Possible Explanations

There are several possible explanations for the increased use of loans in treated communities. The first is a potential effect of the program external to household decision-making. It is possible that the program increased liquidity within the treated communities. Anelucci and De Giorgi (2008) found this to be a positive externality of cash transfers in their report on *Progresa* in rural Mexico. If this were the case in Colombia, it is likely that the community-based informal lending networks (which I found up to 88% of the sample utilizing) were improved by these liquidity injections. Even if informal sources were not the direct recipients of the transfer, it is likely that their lending capacity was impacted by the increased cash flows within the communities, thereby making it possible to increase the value and volume of loans.

The program also had the potential to increase a household's creditworthiness, making it more capable of entering the credit market or increasing its loan balance. The cash transfer has proven to increase income in treated areas by 11.7% in rural and 13.2%

in urban areas in Colombia (Attansasio et al., 2005a). The heightened income-level could serve as collateral for lenders within the community. Although knowledge of program participation may not serve as adequate collateral for formal banks, it is likely to have an impact on household creditworthiness within the informal lending market.

The results of the data show evidence of credit rationing at the baseline survey. If credit rationing were not present, a household with credit would be satisfied with the amount it was holding and would not increase its loan balance if more credit were available. Instead, the results prove the reverse. A large part of the increase in loans outstanding balance is attributable to households formerly holding credit. This signals that, at the baseline, households are not receiving their demand in loans. This rationing is very typical in poor communities as formal banks often do not provide their services to impoverished households and the informal credit market is often too undersized and capital-constrained to meet demand. The increase in loan balance among households formerly holding debt implies that this credit rationing could have been alleviated, at least in part, by the additional liquidity of funds within the community or by the increased creditworthiness of borrowers.

An effect of the program which may have more directly impacted the household's decision to increase its credit activity is the income-insurance associated with the transfer. A consistently higher, stable income has the potential to ease risk-adverse behavior and encourage households to utilize debt. This income-insurance effect could partially explain both the increase in the amount of debt held by treated households and the increase in the number of households entering the credit market. This report,

however, does not have adequate data to evaluate whether this was the reason for increased credit use in this sample.

Whether the effects of the program were external or internal to the household's decision-making, it is important to note that the overall effect of the program is increased credit utilization. With the exception of urban households with a zero-balance at the baseline, the transfer did not negate the need for loans in these areas; it, instead, prompted their increased use. This indicates that the majority of households in these communities both need and want more credit than is available to them, and this conditional cash transfer program enables them to meet their demands at an improved level.

The difference between rural and urban areas in the findings is distinct. Though both groups increase credit involvement in the first year of the program, the impacts are much stronger in rural areas. Innate differences between rural and urban areas and the effect these differences can have on informal markets can potentially explain the differences in the findings.

Reports indicate that informal credit sources in Colombia are more developed in rural than in urban areas (Attansasio et al., 2004a). This is because of the smaller formal bank presence, the higher concentration of poor households, and the lending culture in rural areas. In addition, rural households are often dependent on credit for their income-generating activities because of the high investment costs associated with agricultural endeavors. These factors increase the demand for informal credit options thereby establishing a more relied upon informal network between rural households. This established network explains the higher percentage of households with debt outstanding in rural areas. It also can explain why the program had a larger effect in rural areas. It is

likely that the liquidity introduced by the program increased the cash flows within these established community lending networks more dramatically in rural than in urban areas.

X. Conclusion

Conditional cash transfer programs are becoming an increasingly popular safety net policy among developing countries, as programs both increase consumption and encourage participants to invest in human capital. An integral part of many established programs is an extensive evaluation component which measures the impact of the program on the targeted population. These evaluation components focus primarily on the direct effects of the program and do not investigate the externalities associated with program implementation or involvement. As cash injections into poor households have potentially large impacts on households aside from those studied in the reports, these impacts must also be investigated in order to understand the overall effects of these programs on households.

This report examines *Familias en Acción*, the conditional cash transfer program implemented in Colombia in 2002. The program included a four-year evaluation period from 2002 to 2006 in which three rounds of surveys were conducted with 11,500 households. The results from the organizations hired to evaluate the program report evidence of increased school attendance, improved child health, and increased consumption among program participants. The reports highlight the direct impacts of *Familias en Acción* but lack in their investigation of indirect effects. I measure how the program changes credit behavior in participant communities.

Through a difference-in-differences analysis between control and treatment groups over the four-year period, I found significant changes occurring to loan balances and credit market participation in treated households. In rural areas, the effects were felt across the entire evaluation. Rural households increased their outstanding loan balance by 219.2% after just one year of the program and by 161% over the four-year period. In addition, the number of households holding credit increased by 17.3% in the first year of the program and by 13.4% over the four years. There were also increases to credit participation within participants who did not have debt before the start of the program. Although the overall impact is increased credit involvement in rural areas over the four year evaluation, the effects are felt most powerfully in the first year of the program. Following the first year, loan balance and credit participation begin to decline compared to the initial effect. In urban areas, the impacts of the program on credit are only felt significantly in the first year of the program. Urban households increase their loan balance by 78.8% and their credit participation by 6.8% in the first year. The results in the long run are insignificant, implying that the effects of the program may only be temporary in urban areas. In treated urban households with a zero loan balance before the start of the program, there is a decrease in credit participation relative to control households in the long run. This indicates that in this specific group of households, the cash transfer may actually reduce the need for credit. These findings are in contrast to the results from all other treated segments.

Overall, the results indicate that credit behavior is impacted by participation in the program. All treated households increase both their loan balance and credit involvement after the first year of the program. This effect declines over time and is even negative in a

small part of the sample in the long run. However, the significance of the results is relevant for policy makers. It implies that conditional cash transfers do impact credit behavior, and that this externality is a direct effect of the program.

Credit enables poor households to smooth consumption and exploit investment opportunities. Vicious credit cycles, however, can quickly trap poor households into levels of debt out of which they cannot escape. This report does not advocate the merits or the faults associated with increased credit market participation in low-income communities. Instead, it indicates that increased cash flows within the participant communities leads to an increased incidence of credit use, especially in the short run and in rural communities. Countries devising conditional cash transfers and those that already have functioning programs should consider this effect as they design, modify, and apply these programs in their countries.

Appendix

Table A1

Mean characteristics of late-treat, early-treat, and control areas at the baseline

	Late-treat Mean (Std. Error)	**Early-treat** Mean (Std. Error)	**Control** Mean (Std. Error)
Number of people in household[+]	6.027	5.786	5.966
	(2.355)	(2.235)	(2.426)
Number of children under 7 year of age[+]	1.243**	1.102	1.108
	(1.173)	(1.125)	(1.447)
Number of children ages 7-11[+]	1.081	1.003	1.041
	(0.966)	(0.923)	(0.961)
Number of children ages 12-17[+]	1.009*	1.000**	1.064
	(1.017)	(0.923)	(1.026)
Urban	0.415**	0.486**	0.361
	(0.493)	(0.499)	(0.480)
Private Insurance	0.036**	0.036**	0.053
	(0.187)	(0.186)	(0.224)
Age of head[+]	43.518**	44.930	44.856
	(12.555)	(12.830)	(12.863)
Single parent	0.128	0.140**	0.117
	(0.334)	(0.347)	(0.321)
Number of female adults[+]	1.376	1.393	1.382
	(0.711)	(0.732)	(0.740)
Head's Education:			
- Incomplete primary	0.431	0.454**	0.424
	(0.495)	(0.498)	(0.494)
- Complete primary	0.145	0.138	0.137
	(0.352)	(0.345)	(0.344)
- Secondary	0.082	0.095	0.091
	(0.275)	(0.294)	(0.288)
-Secondary or more	0.083	0.086	0.095
	(0.275)	(0.280)	(0.293)
Home walls: mud	0.093**	0.082**	0.132
	(0.291)	(0.274)	(0.338)
Home walls: good-quality wood	0.443**	0.284**	0.192
	(0.497)	(0.451)	(0.394)
Home walls: poor-quality wood	0.151**	0.197	0.196
	(0.358)	(0.398)	(0.397)
Home walls: cardboard/None	0.010*	0.014	0.016
	(0.099)	(0.118)	(0.124)
Has piped gas	0.055**	0.091*	0.075
	(0.228)	(0.288)	(0.264)
Has piped water	0.649	0.534**	0.640
	(0.477)	(0.499)	(0.480)
Has sewage	0.297**	0.211**	0.263
	(0.457)	(0.408)	(0.440)
Has trash collection	0.317**	0.261**	0.358
	(0.465)	(0.439)	(0.479)
Has a toilet connected to sewage system	0.272	0.206**	0.253
	(0.445)	(0.404)	(0.435)
Owns a home	0.663**	0.627	0.628
	(0.473)	(0.484)	(0.483)
Household member died	0.070	0.068	0.062
	(0.256)	(0.253)	(0.242)
Household member suffered from illness in past year	0.180**	0.167	0.155
	(0.384)	(0.373)	(0.362)
Household suffered from violence in past year	0.035	0.029**	0.040
	(0.184)	(0.168)	(0.197)
Log household savings	0.350	0.415	0.374
	(2.052)	(2.202)	(2.176)
Log household consumption	12.023**	12.252	12.261
	(1.581)	(0.891)	(1.098)
Log home loan outstanding	0.424**	0.303	0.240
	(2.448)	(2.087)	(1.854)

	Late-treat Mean (Std. Error)	**Early-treat** Mean (Std. Error)	**Control** Mean (Std. Error)
Region:			
- Lives in Eastern region	0.213	0.215	0.2213
	(0.409)	(0.411)	(0.415)
- Lives in Central region	0.276**	0.264**	0.222
	(0.447)	(0.441)	(0.415)
- Lives in Pacific region	0.144	0.118**	0.141
	(0.351)	(0.323)	(0.348)
No. of observations	3101	3418	4527
No. of municipalities	31	26	65

** denotes that the treatment group is significantly different from the control group at the 1% level

* denotes that the treatment group is significantly different from the control group at the 5% level.

Note: All variables except those denoted by $^{+}$ are dummy variables. For the dummy variables, proportions of households answering "yes" are reported.

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