The Impact of Conditional Cash Transfers on Children's School Achievement: Evidence from Colombia

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Abstract

Conditional cash transfer programs have expanded in developing countries as a way to foster human capital accumulation. Despite evidence of these programs' positive impact on school enrollment, little is known about their impact on school achievement. This study estimated the effect of *Familias en Acción* on school achievement. It found that the program has a positive effect on school achievement for children aged 7 to 12 living in rural areas. However, the study found a negative effect on the school achievement of adolescents, particularly those living in rural areas. Possible mechanisms of these effects are explored and discussed.

Keywords: program evaluation, academic achievement, international education, conditional cash transfers.

1. Introduction

While Latin America increased coverage of education during the 1990s, especially at the primary level, the region continues to suffer from enrollment deficits, primarily in secondary schools. Following the lead of the Mexican program *Oportunidades* (formerly *Progresa*), conditional cash transfer (CCT) have continued to expand in the region, targeting poor families with subsidies conditioned on investments in human capital, such as school enrollment, regular school attendance, and visits to a health center.

CCT programs aim to improve families' economic conditions so parents can send their children to school, thereby increasing the educational attainment of today's generation so they may increase that of the next. Despite evidence that CCT programs positively affect school

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enrollment and attendance, less is known about their impact on school achievement, an important measurement as one objective of such subsidies is to increase educational attainment in the long run. Most particularly, the long-term impact of these programs may be much larger if, in addition to increasing enrollment and reducing dropouts, they reduce grade retention rates, translating into more schooling completed in less time. Additionally, increased learning may boost the chances of further education and employment opportunities.

Accordingly, this paper investigates the effect of the Colombian CCT program *Familias en Acción* on school achievement as measured by grade retention and test scores. It also explores the program's impact on other potential effect mechanisms; namely, school attendance, a child's time use, and a child's health.

One major challenge in measuring the program's effect on school performance is that schools attended by treatment group subjects are likely to undergo compositional change due to increases in enrollment. This, in turn, may alter the relative measurements obtained on relevant variables from beneficiary and non-beneficiary groups. Studies in Mexico (Behrman, et al., 2005b) and Brazil (Janvry, et al., 2006; Lavinas, et al., 2001) that compare grade retention rates and test scores between CCT beneficiaries and non-beneficiaries show that, for those age groups for which there are enrollment effects, beneficiary group achievement is actually lower than non-beneficiary group achievement, which may indicate that those reenrolling or not dropping out because of the CCT program have lower achievement than non-beneficiaries. This difference between beneficiary and non-beneficiary groups means a simple comparison of grade retention rates or average test scores between groups cannot produce an unbiased effect estimate, even if beneficiary status is randomly assigned.

Yet only one study conducted in Mexico, have taken this compositional change into account. In this study, Behrman et al. (2000) adjusted differences in test scores between treatment and control groups based on the age and sex distribution of control group children so that the distributions of both groups matched. This improved method still fails to account for variables besides age and sex that might be driving both school enrollment and test scores.

The present analysis builds on this strategy to estimate the effect of *Familias en Acción* on grade retention and test scores among children enrolled in school. By matching children with the greatest propensity to stay in school (based on child and household characteristics), it constructs two comparable groups, one CCT beneficiaries still enrolled in school, the other non-beneficiaries not enrolled in school whose observed characteristics are as similar as possible. The most important assumption underlying this propensity score matching strategy is that, conditional on observed characteristics, no unobserved differences exist between these two groups that also predict outcomes. While this assumption cannot be tested, no significant differences between matched groups across a range of characteristics, including previous grade retention, which proxies for ability, were found.

Furthermore, since no test was administered to children out of school, our estimates measure the effect of the program on beneficiary children who were and would have been enrolled in school in the program's absence. Estimation of this effect is important because subsidies are being provided not only to children outside the school system, but also to children who are in school but who live in household with very limited resources. The policy goal is to increase school attainment of the nation as a whole, and thus, achievement effects of both (new enrollees as well as those already in school) is important. The rest of the paper is organized as follows. Section 2 reviews the empirical evidence on CCTs and achievement and discusses possible mechanisms through which CCTs can affect achievement. Sections 3 and 4 describe the data and empirical strategy. Section 5 presents the results, whose implications are discussed in the concluding section.

2. Conditional cash transfers and achievement

2.1. Review of empirical evidence

In Latin America, CCT programs have been in place since the late 1990s and early 2000s in Mexico, Brazil, Honduras, Nicaragua, Jamaica, Chile, Guatemala and Colombia. All programs provide a cash subsidy to families conditional on certain education-related behaviors and/or health services use. There is strong evidence that these programs have significant effects on increasing enrollment rates, especially in secondary school. In Mexico, the estimated effects range from a 7.2 to 9.3 percentage point increase for girls and a 3.5 to 5.8 percentage point increase for boys in secondary school (Skoufias, 2005). In Colombia, a positive enrollment effect of 5 percentage points for urban youth aged 14–17 and 7 percentage points for those in rural areas was found (Attanasio et al., (2006). For younger children, there is a 1-3% positive effect on enrollment, which is not negligible given this group's already high enrollment rates (for reviews see Morley and Coady (2003) and Rawlings and Rubio (2005).

CCTs in Latin America have also reduced dropout rates. For example, *Progresa* has reduced dropout rates for youth over 10 by 5.9- 11.6% (Behrman et al., 2005b). For Brazil, Janvry et al. (2006) found a 7.8 % reduction in dropout rates as a consequence of *Bolsa Escola*.

Evidence on CCT's effect on cognitive achievement is less conclusive (Reimers, da Silva, & Trevino, 2006). Using data from the Mexican *Progresa* program, Behrman et al. (2005b) identified lower repetition rates (and higher grade-progression rates) for the treatment group than the control group for ages 6–11, but the inverse for youth aged 12–14, for whom enrollment effects were also larger. Likewise, for Brazil, Janvry et al. (2006) showed a 0.8 percent increase in retention rates for program beneficiaries over non-beneficiary (but eligible) children. However, these findings cannot be interpreted as a causal effect of the programs on grade retention due to the lack of account for program-related compositional changes. Moreover, if lower-ability children return to (or are kept from leaving) school due to the program, biases towards the lower achievement level of these students would emerge in any analyses comparing grade retention rates between treatment and control groups. Yet, no existing studies account for compositional change when estimating grade retention effects.

In terms of effect on test scores, two studies using Brazilian data found lower achievement scores for beneficiary children than for non-beneficiary (Lavinas et al., 2001; Reimers et al., 2006). While these studies fail to consider enrollment-related compositional changes, Behrman et al., (2000) and Behrman et al., (2005a) take these changes into account and find no effects of *Progresa* on test scores.

2.2. Theoretical effects of CCTs on cognitive achievement

CCT programs can affect children’s achievement through several possible mechanisms. First, evidence from Brazil, Mexico, and Honduras has shown that school attendance (conditional on enrollment) increases as a result of a CCT program (Cardoso and Souza, 2004; Glewwe and Olinto, 2004; Skoufias and McClafferty, 2001). If the school is of sufficient quality, there should additionally be a positive effect of attendance on student performance.

Second, the cash transfer increases the family’s disposable income, which may affect child cognitive achievement. This may be achieved through direct investments like toys or books that provide a more stimulating home environment (Votruba-Drzal, 2003) and/or through influencing parenting behavior by relieving material hardship and parental stress (Gershoff, et al., 2007).

Another possible mechanism is food security and nutrition. *Familias en Accion* directly affects household food consumption, particularly protein and cereal (Attanasio, et al., 2005), meaning that children are less likely to experience the food deprivation associated with lower academic performance (Jyoti, et al., 2005) and learning (Winicki and Jeminson, 2003). Indeed, research shows that the Colombian program has a positive effect on young children's nutritional status. Increased nutritional status may lead to greater learning productivity (Martorell, 1999) and improved school performance (Glewwe, et al., 2001). CCTs can also positively affect children's health, and in turn academic achievement, through increased use of preventive services and healthier parental behaviors induced by the program's health workshops (Lagarde, et al., 2008).

More household income may reduce pressure on children to work outside the home, leaving time to attend school and do homework. Indeed, research has shown that CCT programs have reduced child labor (Maluccio, 2003; Skoufias ad Parker, 2001) and increased homework time, changes which may improve school achievement.

However, CCTs can also affect class size and composition, which may not positively affect student performance. Indeed, without supply side intervention (as with *Familias en Accion*), increased enrollment increases class size. Although the empirical evidence is inconclusive, some studies in developing countries have found a negative effect of class size on students' test scores (Case and Deaton, 1999; Urquiola, 2006).

Altered class composition due to new children entering school (both first time and re-entering students) may negatively affect school performance. Schools and teachers may be challenged by managing both previously enrolled children and a new group who need to catch up. If teacher capacity or instructional adjustments are inadequate, this change can negatively affect learning, at least for children that are or would have been enrolled in the program's absence. In addition, new enrollees with lower ability or a negative attitude toward schooling can adversely affect the rest of the class. Research from mostly developed countries has indicated that students are affected by peer achievement (Gaer, et al., 2007; Hanushek, et al., 2003; Whitmore, 2005).

Moreover, despite no quantitative evidence for this possible mechanism in Latin America, research from Bangladesh found that the *Food for Education Program*, while very effective at increasing school enrollment, negatively impacted test scores of non-beneficiary students through peer effects (Ahmed & Arends-Kuenning, 2006). Likewise, qualitative evidence from Brazil (Lavinas et al., 2001) indicated that reenrollment in school because of the CCT program resulted in overworked teachers and greater classroom disruption.

In addition, absent intervention at the school level (as in Colombia), per student expenditures decrease with rising enrollment, which may diminish quality. Likewise, if schools and teachers fail to adapt their resources and pedagogy to those reenrolling or staying in school because of the cash transfer, little learning may occur (Schwartzman, 2005). Finally, attendance may not translate into learning if schools enrolling children are low-quality, which may account for the lack of positive findings on CCTs (Reimers et al. 2006).

3. Data

This paper uses the baseline and first follow-up evaluation data from Colombia's *Familias en Acción*. This program provides a cash subsidy to low-income households with children 7–17 if the child is enrolled in school and attends class 80% of the time. In 2002, the monthly subsidy was $14,000 pesos (approximately US$6) per child attending primary school and $28,000 (approximately US$12) per child attending secondary school. Eligibility for the program is based on the Colombian government's household welfare index used to target social programs, called SIBEN (System for the Selection of Beneficiaries of Social Programs),[1] with extremely poor families with children (SISBEN 1) eligible.

The evaluation is based on a quasi-experimental design. *Familias en Acción* was not randomly assigned but rather was targeted to low income families (SISBEN 1) living in small municipalities that met four basic criteria. Each must have access to basic education and health infrastructure, have at least one bank (for the cash transfer to beneficiaries), have fewer than 100,000 inhabitants, and not be the capital of a regional district or in the coffee region that received special help after the 1995 earthquake. The evaluators formed 639 primary sampling units (PSUs) based on number of eligible resident families in each municipality. A PSU generally coincides with a single municipality with a few PSUs consisting of adjacent municipalities. The evaluators then grouped the PSUs into 25 strata according to geography, urbanization (size of the population living in the municipality's urban area), number of eligible families, quality of life index score (QLI[2]), and education and health infrastructures. From the treatment (*Familias en Acción*) PSUs, they selected 50 PSUs (57 municipalities) with probability proportional to their size (2 within each stratum) and then matched them with 50 control PSUs (65 municipalities). Matching was done within each stratum based on population size and the QLI.

As Table 1 shows, except for number of banks and rural inhabitants, no statistical differences emerged between treatment and control municipality characteristics. While the number of banks may relate to other characteristics associated with educational outcomes, the lack of any other significant differences suggests that both groups are reasonably comparable. Nonetheless, all characteristics are carefully controlled for throughout the study. Even differences that are not statistically significantly different from zero may produce biased treatment effects.

Finally, while eligible households were randomly selected *within* each municipality, some municipalities initiated programs before baseline data collection. To tackle this problem, the program evaluators divided the treatment sample into two sets of 25 PSUs, one for municipalities without the baseline measurement, labeled the treatment with incomplete baseline group (TIB), and another for those whose baseline data were collected before program inception, the treatment with full baseline group (TFB). Moreover, even though TIB baseline data were collected after treatment had already started, the research group was careful to collect as many retrospective data as possible. The following discussion stipulates whether retrospective data were available (meaning use of the full treatment group) or not (meaning use of TFB only).

The baseline data were collected between June 20 and October 31, 2002, and the first follow-up was done between July 28 and November 20, 2003. Of the total 18,145 youths aged 7 to 17 in the sample with enrollment data, 13,166 have grade retention information and 831 have test score data.

The baseline and follow-up surveys asked about grade retention for all school-aged children in the household, providing these data for all grades. Retrospective TIB grade retention information was acquired, allowing analysis of the full treatment group.

This study uses post-treatment language and math test scores from the *Pruebas Saber* national exam (pre-treatment scores are not available), however, as the test is only administered to fifth and ninth graders the sample for the test scores analysis is much smaller (831 cases).

While it would be ideal to use only the TFB sample, because of sample size limitations, the analysis must draw on the full sample (retrospectively controlling for grade retention).

4. Methods

Grade retention and test scores are only available for children enrolled in school, presenting a challenge for estimating the program's effect on performance. As discussed above, the program's positive enrollment effect creates compositional changes in treatment group classrooms, which may lead to biases in any analysis comparing beneficiary and non-beneficiary data. This is because many beneficiary children would have dropped out or not enrolled in the program's absence and may have different characteristics than those enrolled despite the program. Said another way, enrollment is not randomly assigned.

To illustrate this point, Table 2 lists the pretreatment characteristics of treatment group and control group subjects enrolled in school at follow-up, respectively.[3] Marked differences emerge for students still enrolled at follow-up; specifically, treatment group subjects enrolled at follow-up come from more disadvantaged households than enrolled control group subjects in terms of human capital (a household head with lower educational achievement) and economic well-being (households with fewer durables and high-valued assets that are less likely to own their home but more likely to have no assets). Treatment group subjects enrolled at follow-up also have slightly less favorable values on average for pretreatment characteristics related to school outcomes: they are more likely to have repeated a grade and less likely to have been enrolled in school just before treatment. These differences are quite small, however.

In sum, *Familias en Accion* is attracting less advantaged children to enroll in school, which is fundamentally good. However, these new enrollees alter class composition to include youth with less favorable socioeconomic and academic characteristics. When treatment and control group performance is compared, a negative effect estimate may emerge due to these changes rather than real changes in achievement.

As previously discussed the study by Behrman et al. (2000) considered this compositional effect for *Progresa* in Mexico. However, they could only weight the treatment group to match the control group with two observable pretreatment characteristics (age and sex) due to data limitations. In a second study, Behrman et al., (2005a) administered an achievement test to a subsample of adolescents in the *Progresa* program regardless of school enrollment. This had the potential advantage of observing achievement independently of school enrollment status. However, this was counteracted by attrition problems, which they addressed by using the probability of attrition given a set of pretreatment characteristics, X, to reweight the post-treatment observations to produce the same distribution of X as prior to attrition.

Building upon this intuition, this current analysis makes use of a framework, termed *principal stratification* by Frangakis and Rubin (2002), which is increasingly used to address the issue of controlling for or dealing with selection on a post-treatment variable (see also, Imai, 2008; Roy, 2008; Yannis, et al., 2007). There are two key ideas here. The first is that we can only make inferences about the program's impact on test scores and grade retention for those who would have stayed in school even in the absence of the program (unless we treat these scores as missing data – an approach not taken in this paper). However, because enrollment is a *consequence* of the program, we cannot control for it directly because the students in the treatment group are different from those in the control group. Therefore the second key idea is that, due to the compositional effects, we will have to perform adjustments if we want to compare similar kinds of people across groups. We cannot just rely on the randomization any longer. The randomization does put us in a more advantageous position than the typical observational study however. Because of the initial randomization we know that, distributionally, matches must exist for all the students about whom we want to make inferences.

We can *define* such a group of comparables based on potential enrollment outcomes —— enrollment behavior that would manifest under both treatment and control conditions— which allows definition of enrollment subgroups that are not defined as consequences of the treatment. The possible combinations of these potential outcomes (*principal strata* in Rubin's language) are listed in Table 3. The Always Enrollers are those who would have been enrolled in school whether they had been given access to the program or not; the Newcomers are those who would only have enrolled in school if they had been given access to the program. Drop-outs would not be enrolled either way, and the possibility of a child enrolling in the absence of the program that would not enroll if given access to it is explicitly disallowed.[4] The goal of this study is to estimate the impact of the program on test scores and grade retention for the Always Enrollers, because test scores only apply to children in school. To help clarify this estimand throughout we define a new variable for these principal strata categories, C, where C=a for the Always Enrollers, C=n for the Newcomers, and C=d for the Drop-outs.

Within the control group, the Always Enrollers are identified as the only ones we observe to be enrolled in school. Always Enrollers cannot be directly observed in the treatment group (that is. we can't distinguish them from the Newcomers), however, by comparing them to control group Always Enrollers using a rich array of observed covariates, we can attempt to identify them. Specifically, control group Always Enrollers are matched to the treatment group based on pretreatment characteristics using a propensity score calculated with the observed covariates. Importantly, since we identify treatment effects using observed covariates, we need to assume that we have controlled for all covariates associated with school enrollment and test scores.

This methodology is an improvement over previous methods because (a) the analysis excludes students enrolled after the program's inception (instead identifying those that would have stayed in school even in the programs' absence), allowing measurement of CCT-produced performance gains among those enrolled in school and (b) it uses a wider range of pretreatment characteristics, including previous grade retention, reducing the possibility of selection bias. Nonetheless, one important limitation is that the method does not allow measurement of the program's effect on all enrollees (notably, the Newcomers).

4.1. Matching

We find matches for the control group Always Enrollers by using propensity score matching, a statistical technique that creates a counterfactual scenario by pairing control group cases to treatment group counterparts with similar pretreatment characteristics (Rosenbaum and Rubin, 1983). If we can create observationally similar groups, we can then infer that differences between group outcomes are more likely due to the treatment rather than preexisting differences.

The first step is calculation of the propensity score, which is typically described as the conditional probability of being in the treatment group given a set of observed pretreatment characteristics. Formally, the propensity score can be written as $e(\mathbf{X}) = Pr(Z=1|\mathbf{X})$, where Z is an indicator for treatment (which takes the value of 1 if the observation is in the treatment group; 0 if it is in the control group), and **X** is a vector of pretreatment characteristics. The ultimate model specification for calculating this score is contingent upon finding the best balance on observables (i.e., no significant differences between treatment and control groups). In the next step, each observation in the treatment group is matched to the control group observation having the closest propensity score. This helps us find a comparison group whose propensity score distribution is as similar as possible to that of the treatment group. Propensity score theory tells us that if we have specified our propensity score model well, we should in turn have found a subgroup among the control group that looks as similar as possible to the treatment group with respect to observed covariate distributions. That is, when matched groups of treatment and control units have the same distribution of e(**X),** the distribution of **X** should also be the same for both groups (Rosenbaum and Rubin, 1983, 1984). Therefore we can judge the adequacy of our propensity score model but evaluating the balance in covariate distributions across treatment and control groups after matching.

In our study however we first need to limit our sample to those all those enrolled at both baseline and follow-up. Thus the propensity score is calculated as $e(\mathbf{X})=Pr(Z=1|\mathbf{X},S=1)$, where Z = 1 if the student is in the intervention group and S = 1 if enrolled both at baseline and follow-up (since we are making inferences about the enrolled pupils in the control group it may have seemed less confusing to define Z=1 to designate the control group, however mathematically the process is equivalent if we defined Z=1 to designate the intervention group and defining Z in this way will allow us to continue to define the treatment in a familiar way) . For every subject *i* living in municipality *j* and enrolled at baseline and follow-up, a propensity score is estimated using the follow probit model:

$$P(Z=1)_{ij} = \alpha + \beta\mathbf{W}_i + \gamma\mathbf{M}_j + \varepsilon_i$$

where **W** is a vector of child and household characteristics, and **M** is a vector of municipality characteristics. Child characteristics include age, sex, and whether the child has previously failed a grade. Household variables include demographic characteristics (number of offspring in the household by age group [0–6, 7–11, 12–17], number of adults in the household, marital status and age of the head of household), socioeconomic characteristics (head of household’s educational attainment, number of wage earners in the household; health insurance status, value of assets, whether the household has a vehicle [bike, motorcycle, or boat], the number of durables[5] it owns), and dwelling characteristics (access to potable water, whether the dwelling has a toilet, whether its roof is of poor materials, its floor is of dirt [rather than cement or other materials], and/or its walls are of poor materials [zinc or *guadua* rather than wood or brick]). Municipality characteristics include region, the municipality's QLI score, the number of schools per 1,000 inhabitants, and whether the child lives in an urban or rural area.

Specifically, this analysis uses nearest-neighbor matching with replacement, so that for each control group subject enrolled in school both at baseline and at follow-up we find a matched treatment group peer having the nearest propensity score, whether that peer is also used as a match for another control group enrollee or not. That is, matching with replacement in this setting allows for more than one treatment group enrollee to used as match for several different control group enrollees. Treatment group enrollees not matched are discarded (presumably these are subjects who would have dropped out from school in the program's absence, for which there is no counterfactual in the control group). This produces a matched group of treated enrollees with members as similar as possible to those in the control group who did not drop out between baseline and follow-up.

To estimate the treatment effect on Always Enrollers, a weighted regression of the outcome on the treatment variable (program participation) is fitted to the matched sample; control enrollees are each given a weight of 1 and treatment enrollees are weighted by the number of times they were matched to a control enrollee. Running a regression on the matched groups that includes the baseline covariates can improve the efficiency and precision of the estimates relative to a simple (weighted) difference in means (Dehejia and Wahba, 2002; Rubin and Thomas, 2000). For the outcome of grade retention, the following equation is estimated:

$$\log(P_i/1\text{-}P_i) = \alpha + \tau Z_i + \beta \mathbf{W}_i + \gamma \mathbf{M}_j + \varepsilon_i$$

where $\log(P_{ij}/1\text{-}P_{ij})$ represents the log odds of a child *i* in municipality *j* repeating a grade. *Z* is the treatment group indicator, and τ is the treatment effect.

It is important to underscore the assumption necessary to identify this causal effect. We must assume that, conditional on **W** and **M**, no differences in unobserved characteristics exist between matched treatment and control groups that predict the outcome. Although this assumption cannot be tested, because both groups can be matched on a wide set of child and household characteristics (see the Results section), no significant differences in demographic and socioeconomic characteristics remain in either group.

In the final step, to explore possible mechanisms that produce effects on grade retention or test scores (if any). We use the same methodology as above to measure the effect on school attendance (which is conditional on enrollment) and we use differences-in-differences estimators to measure the program's effect on outcomes that are observed independently of school enrollment status: child's time use and a child's health.

5. Results

The treatment effects were estimated separately for children (7–12) versus adolescents (13–17) because of expected age-related differences. First, parents can more easily enforce school enrollment and regular attendance among younger children. Likewise, deciding to enroll may be more difficult for adolescents because of the higher opportunity cost of attending school over work. Finally, parents may find it more difficult to help with and enforce homework as their offspring advance and encounter more challenging content.

Separate effects were also estimated for urban and rural residents for each age group in case the program effects differed by context. Urban and rural schools differ greatly in terms of infrastructure, resources (likely better in urban schools) and teaching methodologies. Likewise, differences in the need for child labor and rates of compensation mean urban and rural children face unique opportunity costs for attending school (versus working). Finally, rural areas have been more affected by Colombia's violent conflict than urban areas and are more influenced by armed groups.

5.1. Effects on enrollment

The difference-in-difference estimates listed in Table 4 demonstrate the significant positive effects of *Familias en Acción* on school enrollment. The increases for urban and rural adolescents (13-17) are 4.6% and 3.3%, respectively. For urban and rural children (7-12) the increases are 2.4% and 2.3%, respectively.

5.2. Balance of pretreatment characteristics

Table 5 reports the balance achieved after control group subjects enrolled at baseline and first follow-up had been matched with treatment group enrollees based on pretreatment characteristics. For both samples, standardized difference in means are less than 0.05 for almost all characteristics in the matched sample indicating that close balance has been achieved and, with one exception, no substantial postmatch differences in observable characteristics exist between two groups. The number of banks per person is the only substantially different postmatch characteristic, with a good match difficult to obtain because some control municipalities have no bank. Even though the number of banks may proxy for other economic variables related to school outcomes, there is a good balance between matched treatment and control groups for all other municipality characteristics. Nonetheless, this variable is included in all analyses as a control.

The school achievement results—for grade retention, test scores, and other outcomes or possible mechanisms—are first presented for young children (7-12) and then for adolescents (13-17).

5.3. Effects on children

5.3.1. Effects on grade retention among children

Table 6 presents the treatment effect estimates on grade retention for Always Enrolled children aged 7 to 12 years. Estimates for the matched sample suggest *Familias en Acción* has a significant effect in reducing grade retention among children in rural areas who would be enrolled in school even in the absence of the program. For this group, the program reduces the probability of grade retention by an average of 3%, a non-negligible effect given the 2002 average grade retention rate in Colombia of 6%. The restricted sample estimates are not significant, possibly because of the restricted treatment group's (TFB) shorter exposure to the program.[6]

5.3.2. Effects on test scores among children

Estimates of the effect of *Familias en Acción* on test scores for fifth graders are presented in Table 7. As in the grade retention analysis, regression-adjusted estimates are given for the matched and unmatched samples. The sample size for this analysis is much smaller, initially 624 cases for fifth grade, however, once matched, the sample is reduced to 300 cases. For all estimates, the regression-adjusted estimates include the same covariates as the grade retention analysis.

Estimates for the unmatched sample would suggest that among fifth graders, treatment group children do better in math than those in the control group, particularly in rural areas. Results for the matched sample indicate that the program has an significant effect on math (at a

.05 level) and language (at a .10 level) scores for rural fifth graders though results are not statistically significant for urban fifth graders. For the rural enrollees there is an estimated 3.8 point increase in math scores (40% of the *SD*) and an increase in language scores by 3.2 points (almost 47% of the *SD*).

5.3.3. Other outcomes and possible mechanisms among children

The program effect on other outcomes that may lead to higher long-run achievement (school attendance, a child's time use, and a child's health) are presented on Table 8. The top panel presents effects on school attendance using the matched sample and the second and third panels present effects on outcomes that can be observed regardless of school enrollment and which were estimated using difference-in-difference. *Familias en Acción* has a significant effect on school attendance (conditional on enrollment for both urban and rural children.

In terms of time use, *Familias en Acción* appears to significantly reduces child labor among rural children (both the probability of paid/unpaid labor and total hours worked). The signs for the effects on child labor for urban children are also negative but not significant, suggesting that the effects on labor among children are concentrated in rural areas. Also, the program has a positive effect on child health, however only significantly so among urban children.

5.4. Effects on adolescents

5.4.1. Effects on grade retention among adolescents

Table 9 presents the treatment effect estimates on grade retention for adolescents 13–17 who would have stayed enrolled in school even in the absence of the program. Contrary to the results for children, *Familias en Acción* has virtually no effect on adolescents. Moreover, estimates from the matched sample suggest that the program may have actually increased grade retention for rural adolescents that are Always Enrollers. For this group, the estimated increase in the probability of grade retention caused by the program is a small (but undesirable) 1.2%.

5.4.2. Effects on test scores among adolescents

Table 10 presents the regression-adjusted estimates of the program effect on test scores for ninth graders who would have stayed enrolled in school even in the absence of the program. The sample size here is much smaller, reduced from 207 cases to 111 after matching. For all estimates, the regression-adjusted estimates include the same covariates as the grade retention analysis (except for the case of rural areas because the sample size is very small). Consistent with the results on adolescent grade retention, the program has no statistically significant positive effect on adolescent test scores. Moreover, when the matched sample is used, we estimate that the program has a negative effect on math scores among urban adolescents enrollees and on language scores among rural adolescents enrollees.

5.4.3. Other outcomes and possible mechanisms among adolescents

Table 11 presents average the treatment effects of *Familias en Acción* on other outcomes that may lead to higher long term achievement. The top panel presents effects on school attendance using the matched sample and the second and third panels present effects on outcomes that can be observed regardless of school enrollment and which were estimated using difference-in-difference.

These results indicate that *Familias en Acción* has a reducing effect on school absenteeism among adolescents in urban areas but not in rural areas. On the other hand, the program significantly reduces child labor among urban adolescents (both the probability of working and the total hours worked) and reduces adolescent's household work both in urban and rural areas. Finally, the program appears to have no significant effects on adolescent health for these groups.

6. Discussion

Overall, the results suggest that *Familias en Acción* is having a positive effect on school achievement for children (7–12) living in rural areas who would have stayed enrolled in school even in the absence of the program. However there is practically no achievement effect on such children living in urban areas. They also indicate that the program may actually negatively affect the school achievement of adolescents (13–17) who would have stayed enrolled in school even in the absence of the program. *Familias en Accion* has a positive program effect on rural children's achievement by reducing the probability of grade retention and increasing test scores. For this group of children, the program consistently increases school attendance (conditional on enrollment), reduces child labor both outside and inside the home, increases the time children devote to homework, and improves child health.

The program has virtually no effect on grade retention or test scores for children living in urban areas. However, It is important to underscore that less than two years passed between baseline and follow-up, and that this may be insufficient to observe changes on achievement outcomes. The results for other outcomes suggest that the program has a positive effect on health and school attendance,. It is reasonable to expect that in the presence of adequate school quality these outcomes will translate into increased school achievement in the longer run. *Familias en Acción* has no positive effect on rural and urban adolescent school achievement in the short run. However, the program has a positive effect on enrollment for about 4% of adolescents aged 13–17, an important result given that in 2001, 16% of Colombian youth aged 12–15 and 41% of youth aged 16–17 were outside the school system (Ministerio de Educación

Nacional, 2004). Even if the program does not reduce grade retention or increase test scores for those who would have stayed enrolled in school anyway (as in the case of urban adolescents), its effect on enrollment means an important increase in educational coverage for very poor children whose access to education has traditionally been limited, bringing the government goal of universal coverage closer.

For adolescents living in rural areas, however, the program may have a detrimental effect among the Always Enrollers. This may be due to increased grade retention in this group. Additionally, no effect was observed on other outcomes for rural adolescents. While not encouraging, this finding should be put into perspective. The program increases the probability of grade retention for rural adolescents (who are and would have been enrolled even without the program) by at most 1%, which is not trivial given the secondary school retention rate of 3.6%. However, an enrollment effect of 4.6% for a group in which over half the subjects are outside the school system is important enough that it may outweigh the negative effect on grade retention.

One possible reason for the different program effect between rural children and adolescents is related to labor. Rural adolescent labor is highly valued. This paper has shown that while more rural adolescents are enrolling to school as a result of the program, they are not reducing their work hours.. This may leave them with less energy or capacity to focus on school work. Also, another reason for the slightly negative effect of the program on rural adolescents' achievement is that the large effect on enrollment may be having an effect on the quality of education through either peer effects of classroom crowding effects. This was the case for Bangladeshi program Food for Education Program, where the program had a negative effect on non-beneficiary students through peer effects (Ahmed & Arends- Kuenning, 2006).

Finally, it is important to stress that the analysis performed here does not allow for a causal estimate of the program's effect on school performance for new enrollees because no grade retention or test score data are available for this population among those not exposed to the program. Nonetheless, as noted above, the fact that these children are deciding to enroll in school is itself a positive outcome. Additionally, while it cannot be tested, given the program's positive effect on school attendance for all children, these newly enrolled children should be acquiring skills and information if the quality of schooling is adequate. Further research on the achievement effects of CCTs on those who enroll to school as a consequence of the program is much needed. One strategy to get at such an effect would be to use the strategy employed by Behrman et al. (2005a) who administered the same test both to those enrolled and not enrolled in school.

Also, further research on the changes occurring in schools after the implementation of *Familias en Acción* would increase understanding of why secondary-level school children are not benefiting in terms of school achievement, particularly in rural areas.

The main policy implication of the findings of this study is that attention must be paid to the implementation of such programs at the school level. That is, even though *Familias en Accion* was designed to subsidize the demand for schooling, it does so with little or no intervention from the supply side. Therefore, like many other CCT programs, the program objectives imply a black box approach (Reimers et al., 2006), which assumes that more enrollment will automatically translate into more achievement. Attention to the quality of public education can help to assure that enrollment and attendance translate into learning, thus potentiating the positive effects of this type of interventions. In other words, if quality is part of the intervention CCT programs can an effective policy tool, not only for short-term poverty alleviation but also for long-term human capital accumulation.

Notes

[1] The index, an indicator of economic well-being, is a function of a set of household demographic characteristics and variables related to the consumption of durable goods, human capital endowments, and current income. This index is divided into 6 strata, with SISBEN 1 corresponding to extremely poor or indigent, SISBEN 2 to poor, and SISBEN 3 to near poor.

[2] This index, used in Colombia as a poverty indicator, comprises the following variables: schooling of household head, average schooling of individuals older than 12, school enrollment of children, main material of house walls, main material of house floor, mode of sewage disposal, access to water, cooking fuel, mode of garbage disposal, proportion of children under 6, and number of individuals per room.

[3] Results reported on Table 2 and throughout the paper are unweighted. The analyses were also done using sampling weights, and the results do not change.

[4] Instrumental variables analyses with binary instrument and treatment variables can be thought of as a special case of a principal stratification analysis as can be seen in Angrist, et al. (1996). We cannot use IV here however both because the exclusion restriction might be violated and also because we don't have test scores on students not enrolled.

[5] Durables include refrigerator, sewing machine, television, music equipment, fan, blender, kerosene lamp, and electric generator.

[6] The TFB group has been exposed to the program for a shorter time because it was introduced for them later than for the TIB group. Thus, whereas the TIB group had received an average of $1,009,255 in subsidies by the first follow-up, the TFB group had only received an average of $760,000. Likewise, the TFB group has been exposed to an average of 6 months less than the TIB group.

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Table 1. Municipality characteristics by treatment group, means and standard deviations.

	Treatment group		Control group		t[1]
	Mean	S.D.	Mean	S.D.	
Health infrastructure					
Hospitals per 10,000 inhabitants	.425	.427	.457	.670	0.30
Health centers per 10,000 inhabitants	.448	.676	.772	1.34	1.65
Health posts per 10,000 inhabitants	2.17	2.34	2.04	2.37	-0.30
Drugstores	3.59	1.90	3.46	2.48	-0.32
Education infrastructure					
# urban schools/10,000 inhabitants	3.81	1.87	4.30	3.08	1.03
# rural schools/10,000 inhabitants	19.32	13.58	18.35	14.18	-0.39
Total schools/10,000 inhabitants	23.13	12.97	22.65	14.65	-0.19
Economic characteristics					
Taxes per 10,000 inhabitants[2]	82.9	211	85.1	235	0.05
Banks per 10,000 inhabitants	.701	.477	.404	.919	- 2.19*
Sociodemographic characteristics					
Quality of Life Index	54.67	9.99	56.19	10.67	0.81
Population	30,124	21,635	23,090	23,423	-1.71+
N	**57**		**65**		

[1] t-statistics of the difference in means across groups. +p<0.10; *p<.05; **p<.01; ***p<.001

[2] One million Colombian pesos.

Table 2. Background characteristics of control and treatment groups among those enrolled in school at follow-up, means and standard deviations

	Control		Treatment		t	
	Mean	S.D.	Mean	S.D.		
Children characteristics						
Age	10.64	2.60	10.74	2.63	-2.13	*
Gender (girl)	.49	.50	.49	.50	-0.31	
Previous grade retention	.13	.34	.14	.31	0.43	
Enrollment at baseline	.90	.30	.89	.31	-0.53	
Household composition						
Number of children 0-6	.97	1.08	1.03	1.10	-3.12	**
Number of children 7-11	1.58	.96	1.54	.95	2.00	*
Number of children 12-17	1.38	1.06	1.35	1.08	1.57	
Number of adults in the household	2.61	1.20	2.58	1.20	1.52	
Marital status of head of household						
Cohabiting	.53	.50	.52	.50	1.56	
Married	.33	.47	.31	.46	2.63	**
Divorced	.08	.27	.10	.30	-4.26	***
Single	.01	.12	.02	.13	-1.45	
Age of head of household	43.5	10.7	43.5	10.9	-0.21	
Socio-economic characteristics						
Education of head of household						
Some primary education	.44	.50	.47	.50	-2.92	**
Some secondary education	.15	.36	.09	.29	2.63	**
Completed secondary education	.05	.22	.03	.17	5.71	***
Number of adult earners in household	1.59	.88	1.55	.87	2.78	**
Child has no health insurance	.12	.33	.09	.28	5.58	***
Homeownership	.65	.47	.63	.48	2.02	*
Number of durables owned	2.48	1.60	2.25	1.58	8.17	***
Assets						
No assets	.09	.29	.12	.33	-5.06	***
Assets over Col$ 5 million	.20	.40	.17	.37	4.49	***
Household has access to water	.65	.48	.59	.49	6.50	***
Household has no toilet	.33	.47	.38	.48	-5.66	***
Dwelling has poor roof [1]	.14	.35	.23	.42	-13.4	***
Dirt floor	.39	.49	.45	.50	-6.81	***
Municipality characteristics						
Quality of Life Index	54.63	10.4	53.1	9.5	8.50	***
# of urban schools per 1,000 residents	1.07	.88	1.01	.43	5.28	***
# of rural schools per 1,000 residents	2.73	1.59	3.25	2.53	-13.26	***
Urban	.57	.49	.44	.50	14.66	***
Rural	.35	.48	.44	.50	-9.79	***
N	**5,198**		**7,968**			

+*p*<0.10; **p*<.05; ***p*<.01; ****p*<.001

[1] Recycled materials, zinc, carton or other poor-quality materials.

Table 3. Potential enrollment outcomes

Enrollment status if assigned control	**Enrollment status if assigned treatment**	**Behavioral type**
Would be enrolled school	Would be enrolled in school	Group A: Always Enrollers
Would not be enrolled in school	Would be enrolled in school	Group B: Newcomers
Would not be enrolled in school	Would not be enrolled in school	Group C: Dropouts

Table 4. Average treatment effect estimates for school enrollment

	t.e.	s.e.		N
Young children (7 to12)				
Urban	.024	.007	***	4,995
Rural	.023	.010	*	5,992
Adolescents (13 to 17)				
Urban	.033	.016	*	3,453
Rural	.046	.019	*	3,705

+p<0.10; *p<.05; **p<.01; ***p<.001

Table 5. Balance across treatment groups for both matched and unmatched samples

	Full sample (TFB & TIB)[1]		TFB Only[1]	
	Unmatched	Matched	Unmatched	Matched
Child's age	-0.04	0.03	0.01	-0.01
Child's gender (girl)	-0.01	0.00	-0.01	0.02
Number of children ages 0 to 6	-0.06	-0.03	-0.12	0.00
Number of children ages 7 to 11	0.04	-0.01	-0.04	0.01
Number of children ages 12 to 17	0.03	0.00	0.02	-0.03
Number of adults in the household	0.03	-0.01	0.02	-0.03
Marital status of head of household				
Cohabiting	0.03	-0.04	0.10	-0.04
Married	0.05	0.03	-0.04	0.03
Widow	-0.05	0.01	-0.03	0.00
Divorced	-0.08	0.02	-0.06	0.02
Single	-0.03	0.00	-0.04	0.01
Age of head of household	0.00	-0.02	0.05	-0.05
Education of head of household				
No education	-0.01	-0.02	-0.03	-0.04
Some primary education	-0.05	0.01	-0.03	0.02
Completed primary education	-0.01	-0.02	-0.02	0.03
Some secondary education	0.05	0.02	0.06	-0.02
Completed secondary or more	0.10	0.00	0.08	0.01
Number of adult earners in household	0.05	0.02	0.10	-0.02
Child has no health insurance	0.10	0.01	0.16	0.01
Own home	-0.04	-0.02	-0.09	-0.02
Number of durables owned	0.15	-0.01	0.17	-0.01
Assets				
Negative assets	0.09	-0.01	0.16	0.03
No assets	-0.09	0.04	-0.12	-0.01
Assets $1–$2 million	-0.07	0.00	-0.06	0.01
Assets $2–$5 million	-0.04	0.00	-0.06	0.01
Assets over $5 million	0.08	-0.02	0.05	-0.05
Moto	-0.09	0.01	-0.06	-0.01
Bike	-0.09	0.00	0.03	0.01
Household has access to water	0.12	0.01	-0.06	0.00
Household has no toilet	-0.10	0.00	-0.13	0.04
Dwelling has poor roof[2]	-0.24	0.02	-0.13	-0.01
Dirt floor	-0.12	-0.01	-0.05	0.02
Dwelling has poor walls[2]	-0.06	0.02	0.00	-0.01
Municipality characteristics				
Quality of Life Index	0.15	0.04	0.24	0.00
# of urban schools /1,000 res.	0.09	0.04	0.19	0.05
# of rural schools /1,000 res.	-0.23	0.05	-0.27	0.05
Urban	0.26	0.02	0.24	0.01
Rural	-0.17	-0.02	-0.10	-0.01
Banks per person	-0.65	-0.64	-0.77	-0.81
Taxes per person	0.17	0.05	0.19	0.05
N	**13,166**	**7,964**	**8,916**	**7,034**

Columns contain standardized difference in means.
[1] TFB: Treatment with Full Baseline; TIB: Treatment with Incomplete Baseline
[2] Recycled materials, zinc, carton or other poor-quality materials.

Table 6. Average treatment effect estimates on grade retention for young children (7 to 12 years old)

	Unmatched			Matched		
	t.e.	s.e.	N	t.e.	s.e.	N
TFB & TIB1						
All 7–12 yrs	-.010	.007	9,625	-.012	.008	5,922
Urban	.002	.008	4,531	.000	.009	3,160
Rural	-.017	.010 +	5,094	-.033	.012**	2,762
TFB ONLY[1]						
All 7–12 yrs	-.005	.008	6,625	.001	.010	5,243
Urban	-.004	.009	3,303	.001	.012	2,776
Rural	-.003	.011	3,322	-.004	.014	2,467

Treatment effects are marginal effects dy/dx after logistic regression for the corresponding sample.
[1] TFB: Treatment with Full Baseline; TIB: Treatment with Incomplete Baseline

+*p*<0.10; **p*<.05; ***p*<.01; ****p*<.001

Table 7. Average treatment effect estimates for test scores among 5th graders

	Unmatched				Matched			
	t.e.	s.e.		N	t.e.	s.e.		N
Math								
Total	1.99	.86	*	624	1.46	1.20		300
Urban	2.32	1.40	+	259	1.68	2.12		144
Rural	2.70	1.22	*	365	3.84	1.58	*	156
Language								
Total	1.36	.67	*	624	1.32	.85		300
Urban	1.10	1.11		259	-.97	1.40		144
Rural	1.84	.95	+	365	3.18	1.76	+	156

Treatment effects are marginal effects dy/dx after logistic regression for the corresponding sample.
+*p*<0.10; **p*<.05; ***p*<.01; ****p*<.001

Table 8. Average treatment effects for possible mechanisms among young children

	All Children 7 to 12 yrs			Urban			Rural		
	t.e.	s.e.		t.e.	s.e.		t.e.	s.e.	
School attendance									
Skipped school in the last month	-.068	.016	***	-.064	.022	**	-.072	.021	***
Days absent from school last month	-.504	.086	***	-.321	.090	***	-.695	.149	***
N		**5,850**			**3,129**			**2,721**	
Child time use									
Worked last week	-.008	.002	***	-.002	.001	+	-.010	.002	***
Hours worked last week	-.639	.283	*	-.238	.384		-1.20	.421	**
Hours in paid work	-.151	.054	**	-.140	.087		-.182	.071	*
Hours in unpaid work	-.294	.064	***	-.114	.074		-.448	.102	***
Hours in household work	-.342	.069	***	-.341	.092	***	-.379	.102	***
N[1]		**3,255**			**1,557**			**1,698**	
Health									
Sick in the last 2 weeks	-.023	.010	*	-.048	.014	**	-.005	.014	
Days sick in the last 2 weeks	-.189	.072	**	-.318	.128	*	-.123	.085	
N		**6,582**			**3,104**			**3,478**	

For discrete outcomes, treatment effects are marginal effects dy/dx after logistic regression.
+*p*<0.10; **p*<.05; ***p*<.01; ****p*<.001
[1]Work last week was only asked of children 10 years old or older.

Table 9. Average treatment effect estimates on grade retention for adolescents (13 to 17 years old)

	Unmatched			Matched		
	t.e.	s.e.	N	t.e.	s.e.	N
TFB&TIB						
All 13–17 yrs	.003	.006	3,541	.009	.006	2,002
Urban	-.003	.004	1,959	.001	.004	1,279
Rural	.008	.006	1,582	.012	.005 *	723
TFB ONLY						
All 13–17 yrs	-.001	.008	2,291	.004	.004	1,791
Urban	-.012	.007 +	1,339	-.001	.004	1,127
Rural	.009	.007	952	.010	.006 +	664

Treatment effects are marginal effects dy/dx after logistic regression for the corresponding sample.
+*p*<0.10; *p*<.05; **p*<.01; ***p*<.001

Table 10. Average treatment effect estimates for test scores among 9th graders

	Unmatched			Matched		
	t.e.	s.e.	N	t.e.	s.e.	N
Math						
Total	-1.04	1.05	207	-2.17	.98 *	111
Urban	.013	1.33	151	-2.24	1.00 +	93
Rural	.43	3.44	59	.51	2.47 [1]	22
Language						
Total	.72	1.29	207	-1.48	3.09	112
Urban	1.03	1.39	151	.136	1.43	93
Rural	-4.47	5.02	59	-1.92	3.73 [1]	19

Treatment effects are marginal effects dy/dx after logistic regression for the corresponding sample.
+*p*<0.10; **p*<.05; ***p*<.01; ****p*<.001
[1] Difference in means (no regression-adjusted because of sample size limitations)

Table 11. Average treatment effects for possible mechanisms among adolescents

	All Children 13 to 17 years old			Urban			Rural		
	t.e.	s.e.		t.e.	s.e.		t.e.	s.e.	
School attendance									
Skipped school in the last month	-.034	.022		-.059	.030	*	.010	.042	
Days absent from school last month	-.265	.089	**	-.315	.132	*	.059	.165	
N		**1,941**			**1,249**			**692**	
Child time use									
Worked last week	-.022	.010	*	-.038	.010	***	-.000	.019	
Hours worked last week	-.637	.613		-1.97	.847	*	.370	.893	
Hours in paid work	-.056	.108		-.245	.140	+	.086	.163	
Hours in unpaid work	-.096	.077		-.112	.085		-.079	.128	
Hours in household work	-.322	.076	***	-.472	.104	***	-.200	.112	+
Health									
Sick in the last 2 weeks	.002	.014		.026	.023		-.018	.017	
Days sick in the last 2 weeks	-.063	.092		.032	.126		-.133	.134	
N		**4,141**			**2,097**			**2,044**	

For discrete outcomes, treatment effects are marginal effects dy/dx after logistic regression.
+*p*<0.10; **p*<.05; ***p*<.01; ****p*<.001