

# Documentos CEDE

ISSN 1657-5334

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

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FEBRERO DE 2009

Serie Documentos Cede, 2009-08  
ISSN 1657-5334

Febrero de 2009

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Edición, diseño de cubierta, pre prensa y prensa digital:  
Proceditor Ltda.  
Calle 1C No. 27 A – 01  
Bogotá, D. C., Colombia  
Teléfonos: 2204275, 220 4276, Fax: extensión 102  
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Impreso en Colombia – Printed in Colombia

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**THE IMPACT OF CONDITIONAL CASH TRANSFERS ON  
CHILDREN'S SCHOOL ACHIEVEMENT:  
EVIDENCE FROM COLOMBIA**

**Sandra García<sup>1</sup>**  
**Jennifer Hill<sup>2</sup>**

**Abstract**

During the last decade, conditional cash transfer programs have expanded in developing countries as a way to increase school enrollment and deter youth from dropping out of school. However, despite evidence of these programs' positive impact on school enrollment and attendance, little is known about their impact on school achievement. Thus, using data from the Colombian conditional cash transfer program *Familias en Acción*, this study estimated the effect of the conditional subsidy on school achievement. It found that the program does have a positive effect on school achievement for children aged 7 to 12 living in rural areas but practically no effect for the same population living in urban areas. Moreover, the program may actually have a negative effect on the school achievement of adolescents, particularly those living in rural areas. Possible mechanisms of these effects are explored and discussed.

*Key words:* policy analysis, student achievement, subsidies, conditional cash transfers.

*JEL Classification:* I28, I38, J13.

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## **EL IMPACTO DE LAS TRANSFERENCIAS CONDICIONADAS EN EL LOGRO ESCOLAR: EVIDENCIA PARA COLOMBIA**

**Sandra García<sup>3</sup>**  
**Jennifer Hill<sup>4</sup>**

### **Resumen**

En la última década, la implementación de programas de transferencias condicionadas se ha expandido en los países en vías de desarrollo como parte de una política para aumentar la matrícula escolar y evitar la deserción. La evidencia disponible indica que estos programas tienen un impacto positivo tanto en matrícula como en asistencia escolar. Sin embargo, existe muy poca evidencia acerca del impacto de estos programas en el logro escolar. En este estudio utilizamos datos del programa colombiano Familias en Acción para estimar el efecto del subsidio condicionado en logro escolar. Encontramos que el programa tiene un efecto positivo en el logro escolar para niños entre 7 y 12 años en zonas rurales pero no en zonas urbanas. Adicionalmente, encontramos que el programa tiene un leve efecto negativo en el logro escolar de los adolescentes, particularmente en zonas rurales. Los posibles mecanismos de estos efectos son explorados y discutidos en el presente estudio.

*Palabras clave:* análisis de políticas públicas, logro escolar, subsidios, transferencias condicionadas.

*Clasificación JEL:* I28, I38, J13.

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## INTRODUCTION

Even though during the 1990s, Latin America made important progress in increasing coverage of education, particularly primary education, the region still suffers from significant deficits in school enrollment, primarily at the secondary school level. In the last decade, there has been an expansion of conditional cash transfers (CCT) programs in the region, with the Mexican program *Oportunidades* (formerly known as *Progresa*) as the pioneer. Targeted at poor families, CCTs are conditioned on investments in human capital, such as school enrollment, regular school attendance, and regular visits to a health center.

CCT programs aim improve the 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. Nevertheless, despite research evidence that CCT programs positively affect school enrollment and attendance, less is known about their impact on school achievement. Measuring the effect of these programs on school achievement is important because one objective of such subsidies is to increase educational attainment in the long run. Most particularly, if these programs not only increased enrollment and reduced dropout rates but also reduced grade retention among those remaining in school, their long-term impact would be much larger because enrollment would translate into higher educational attainment (i.e., more years of schooling completed) in a shorter period of time. Additionally, increased learning may augment the chances of further education, perhaps in a college or technical school.

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 grade retention and test scores are only available for children enrolled in school, but because of the program's significant effect on enrollment, the schools attended by treatment group subjects are likely to undergo compositional change. Therefore, even if beneficiary status is randomly assigned, a simple comparison of grade retention rates or average test scores between beneficiaries and nonbeneficiaries will not produce an unbiased effect estimate because many beneficiary children would not have enrolled or would have dropped out if the program were not in place. Moreover, these out-of-school children would probably have had very different characteristics from those that would have enrolled even in the absence of the program. Indeed, studies in Mexico (Jere R. Behrman, Sengupta, & Todd, 2005) and Brazil (Janvry, Finan, & Sadoulet, 2006; Lavinias, Barbosa, & Tourinho, 2001) that compare grade retention rates and test scores between CCT beneficiaries and nonbeneficiaries show that for those age groups for which there are enrollment effects, beneficiary group achievement is actually lower than nonbeneficiary group achievement, which may indicate that those reenrolling in school or not dropping out because of the CCT program have lower achievement than those who stay in school.

Yet only two studies, both for Mexico, have taken this compositional change into account when estimating the program's effect on test scores. In the first, 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. Although an improvement over studies comparing test scores between treatment and control groups (e.g., Lavinias et al., 2001, for Brazil), this method still fails to account for other variables besides age and sex that might be driving both school enrollment and test scores. In the second, Behrman, Parker and, Todd (2005) applied an achievement test to a subsample of adolescents in the

*Progresa* program regardless of school enrollment, and then, to calculate treatment effects, reweighted the differences between the treatment and control groups to adjust for differences in the distribution of observable characteristics due to attrition (based on the probability of attrition from the program).

This present analysis builds on this strategy to estimate the effect of *Familias en Acción* on both grade retention and test scores. First, drawing on a rich set of child and household characteristics to match children with the greatest propensity to stay in school, it constructs two comparable groups, one receiving treatment, the other not. Such propensity score matching produces a treatment group whose observed characteristics are as similar as possible to those of the control group enrolled in school both before and after program implementation. The most important assumption underlying this strategy is that, conditional on observed characteristics, no unobserved differences exist between these two groups that also predict outcomes. Even though this assumption cannot be tested, the analysis did find no significant differences between groups across a wide range of characteristics, including previous grade retention, which proxies for ability. This strategy enabled measurement of the program's effect on school performance, conditional on enrollment (i.e., whether the CCT produced performance gains among children enrolled in school). The rest of the paper is organized as follows. The remaining of this introductory section provides background information on education in Colombia, after which the second section 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 then presents the results, whose implications are discussed in the concluding section.

## Education in Colombia

Education in Colombia is compulsory from age 5 to 15, including one year of preschool and 9 years of basic education (5 of primary school and 4 of basic secondary school). Formal education also includes 2 years of high school (*educación media*), which are not compulsory but are required for a high school degree and entry into post secondary education (i.e., a college or technical school).

During the 1990s, Colombia made important progress in increasing educational coverage. From 1992 to 2003, enrollment rates for children aged 7–11 increased from 90.2% to 96.3%, and those for youth aged 12–17 from 69.2% to 79.6%. Yet, despite these significant improvements, large inequities remain by income, age, and geography. For instance, in 2001, among children aged 7–11, less than 1% of those from families in the top income quintile were not enrolled in school, whereas 9% of children aged 7–11 from the bottom income quintile were outside the school system. The gap for older children was even larger: among youth 12–15, whereas only 5% of those from the top quintile were not in school, close to a quarter (23%) of those from the bottom quintile were outside the system; and among youth 16–17, a quarter of those from the top quintile and almost half (48%) from the bottom quintile were not enrolled in school (Ministerio de Educación Nacional, 2004). The gap in educational coverage between urban and rural areas is also large and increases with age. As of 2003, on average, 12% of school-aged children in urban areas and 25% of school-aged children in rural areas were not enrolled in school.

School dropout and grade retention also represent serious problems in the Colombian educational system. Although at the beginning of the 1990s, 90% of children attended first grade, only 60% finished fifth grade, and of those, only 40% finished it in 5 years. Moreover, only 33% of children entering primary school would finish high school, with the situation being even

worse in rural areas where only 16% of children entering primary school would graduate from high school. Dropout rates, which are highest in first grade (18%) and in the transition from elementary to secondary school (15%), are also higher in rural than in urban areas, almost twice as high (11% vs. 6%) in 2001. Grade retention rates are also higher in rural (12%) than in urban areas (8%) and higher for basic secondary school (Grades 6 through 9) at 12%, followed by primary school at 9%, and then middle secondary school (Grades 10 and 11) at 8% (Ministerio de Educación Nacional, 2004).

## **II. CONDITIONAL CASH TRANSFERS AND ACHIEVEMENT**

### **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, and Colombia. All programs provide a cash subsidy to families with children conditional on certain education-related behaviors and/or health services use. The evidence that these programs have significant effects on increasing enrollment rates is strong. For both Mexico and Colombia, the largest effects on enrollment are for children in secondary school. The estimated effects for Mexico 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), while research for Colombia found a positive enrollment effect of 5 percentage points for urban youth aged 14–17 and 7 percentage for those in rural areas (Attanasio et al., (2006). For younger children, the effects are smaller, ranging from a 1 to 3 percentage point increase in enrollment, which is, however, not negligible given the already high enrollment rates for this age group in both countries (for reviews see Morley & Coady (2003) and Rawlings & Rubio (2005).

CCTs in Latin America have also proven effective at reducing dropout rates. For example, *Progresa* has significantly reduced dropout rates for youth over 10, with an effect ranging from 5.9 to 11.6 percentage points (Jere R. Behrman et al., 2005). For Brazil, Janvry et al (2006) found a 7.8 percentage point reduction on dropout rates as a consequence of *Bolsa Escola*.

In contrast, evidence on whether these programs affect cognitive achievement is both limited and inconclusive (Reimers, da Silva, & Trevino, 2006). Indeed, using data from the Mexican *Progresa* program, Behrman et al, (2005) identified lower repetition rates (and higher grade-progression rates) for the treatment group than the control group for ages 6–11, but higher repetition rates (and lower grade progression rates) for the treatment group than the control group for youth aged 12–14, for whom enrollment effects were also larger. Likewise, for Brazil, Janvry et al. (2006) showed an increase of 0.8 percentage points in retention rate for program beneficiaries over nonbeneficiary (but eligible) children. However, as neither study took into account the compositional changes that might occur because of induced increases in enrollment and reduction of dropout rates, these findings cannot be interpreted as a causal effect of the programs on grade repetition. Moreover, if these programs encourage children with lower ability to reenroll in school or prevent them from dropping out, a simple comparison of grade retention rates between treatment and control groups would be biased toward the lower achievement level of those enrolling or not dropping out because of the program. Yet no extant studies seem to consider this compositional change when estimating the programs' effects on grade retention.

In terms of the program effect on test scores, two studies using Brazilian data found lower achievement scores for beneficiary children than for nonbeneficiary (Lavinias et al., 2001; Reimers et al., 2006). However, these studies also failed to consider possible changes in class

composition because of enrollment. There are only two studies (outlined in the Introduction) that did take into account this compositional change (Behrman et al., 2000; Behrman et al., 2005) and found no effects of *Progresá* on test scores. In the first study, Behrman et al., (2000) adjusted for changes in age and gender composition. Although this is an improvement from studies comparing test scores between treatment and control groups, the study only adjusted for changes in age and gender composition due to data limitations. In the second study, Behrman et al. (2005) administered their achievement tests in the home to all children regardless of school enrollment, meaning that test scores were also observed for children not enrolled in school. Behrman et al. (2005) did account for attrition (mainly due to migration) by weighting the treatment estimates to adjust for differences in the distribution of observable characteristics over time, with the probability of attrition based on characteristics like child's age, gender, parental education, and household characteristics. This current study expands these techniques by using a rich set of pretreatment characteristics to estimate the probability of being in the control group conditional on enrollment before and after program inception and then matching treatment group subjects, conditional on pretreatment characteristics, so that they are comparable to the control group.

### **Theoretical effects of CCTs on cognitive achievement**

CCT programs like *Familias en Acción* can affect children's achievement through several possible mechanisms. First, as research evidence from Brazil, Mexico, and Honduras has shown, school attendance (conditional on enrollment) increases as a result of a CCT program (Cardoso & Souza, 2004; Glewwe & Olinto, 2004; Skoufias & McClafferty, 2001). The school's being of sufficient quality should also produce a positive effect of attendance on student performance.

Second, the cash transfer increases the family's disposable income, which in turn may affect child cognitive achievement (Aughinbaugh & Gittleman, 2003). In particular, increased income can affect child cognitive development through direct investments like toys or books that provide a more stimulating home environment (Votruba-Drzal, 2003) and can influence parenting behavior by relieving material hardship and reducing parental stress (Gershoff, Aber, Raver, & Lennon, 2007).

Another possible effect mechanism is food security and nutrition. That is, like the Mexican program *Progres a , Familias en Accion* directly affects household food consumption, particularly protein and cereal (Attanasio, Battistin, Fitzsimons, Mesnard, & Vera-Hernández, 2005), meaning that children are less likely to experience the food deprivation associated with lower academic performance (Jyoti, Frongillo, & Jones, 2005) and learning (Winicki & Jeminson, 2003). In the long run, better food security can translate into better nutritional status. In fact, consistent with evidence for CCT programs in Mexico and Nicaragua, research has already shown that the Colombian program has a positive effect on young children's nutritional status (Attanasio et al., 2005). Increased nutritional status may in turn lead to greater learning productivity (Martorell, 1999) and improved school performance (Buvinic, Valenzuela, Molina, & González, 1992; Glewwe, Jacoby, & King, 2001). CCTs can also positively affect children's health through increased use of preventive services and healthier parental behaviors induced by the program's health workshops (Lagarde, Haines, & Palmer, 2008), which can in turn have a positive effect on academic achievement.

More household income may also mean reduced pressure on children to work outside the home leaving them more time to attend school (given enrollment) and do homework. Not only has research shown that CCT programs have reduced child labor (Maluccio, 2003; Skoufias &

Parker, 2001), but they have also increased the time spent after school doing homework, which may lead to improved school achievement (Skoufias & McClafferty, 2001).

On the other hand, CCTs can also affect class size and composition, which may not necessarily have a positive effect on student performance. Indeed, in the absence of supply side intervention (as in the case of *Familias en Accion*), increased school enrolment will necessarily lead to increased class sizes. Although the empirical evidence on the effects of class size on school achievement is inconclusive, some studies in developing countries have found a negative effect of class size on students' test scores (Case & Deaton, 1999; Urquiola, 2006).

Another factor that can negatively affect school performance is that increased enrollment means new children entering school (many for the first time; others after some time outside the system) who may change the class composition. This change poses a challenge for schools and teachers who must now manage both an existing heterogeneous group of children and a new group who need to catch up. If this change exceeds teacher capacity or is unaccompanied by instructional adjustments, it can result in a negative effect on learning, at least for children that are or would have been enrolled had the program not been in place. In addition, if new enrollees have lower ability or a negative attitude toward schooling (e.g., chatting in class or skipping classes), they can adversely affect the rest of the class. Indeed, research from mostly developed countries has indicated that students are affected by peer achievement (Gaer, Pustjens, Damme, & Munter, 2007; Hanushek, Kain, Markman, & Rivkin, 2003; Whitmore, 2005; Zimmer & Toma, 2000).

Moreover, despite no quantitative evidence for this possible mechanism in Latin American CCT programs, research from Bangladesh did find that the Food for Education Program, which was very effective in increasing school enrollment, had a negative impact on the

test scores of nonbeneficiary students through peer effects (Ahmed & Arends-Kuenning, 2006). Likewise, qualitative evidence from Brazil (Lavinás et al., 2001) indicated that reenrollment in school because of the CCT program results in overworked teachers and greater classroom disruption.

In addition, even in the absence of peer effects, having more students enrolled in school poses a challenge for fixed resources. Specifically, if the program increases enrollment but no intervention occurs at the school level (as in Colombia), the amount of school spending per student decreases, which may translate into deteriorating 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, if school quality is inadequate, as it may be in the schools enrolling these children, more school attendance does not necessarily translate into more learning. In fact, Reimers et al. (2006) argued that the lack of positive findings on CCTs' effects on school achievement stems precisely from the fact that the schools are of very low quality.

### **III. DATA**

This paper uses the baseline and first follow-up data of the evaluation of Colombia's CCT program *Familias en Acción*. This program provides a cash subsidy to low-income households with children 7–17 on the condition that the child is enrolled in school and attends class at least 80% of the time. In 2002, the subsidy was \$14,000 pesos (approximately US\$6) per month per child attending primary school and \$28,000 (approximately US\$12) per month per child attending secondary school. Eligibility into the program is based on a household welfare index used by the Colombian government to target social programs to poor households, called

SIBEN (System for the Selection of Beneficiaries of Social Programs).<sup>1</sup> Only families with children classified in SISBEN 1, corresponding to extremely poor, are eligible for the program.

The program evaluation is based on a quasi-experimental design that groups municipalities according to the number of eligible resident families to form 639 primary sampling units (PSUs). In general, a PSU coincides with a municipality but in a few cases, consists of two small adjacent municipalities. The evaluators then grouped the PSUs into 25 strata according to geographic region, level of urbanization (size of the population living in the municipality's urban area), number of eligible families, quality of life index score (QLI<sup>2</sup>), and education and health infrastructures. From among all the PSUs targeted, they selected 50 treatment PSUs (corresponding to 57 municipalities) with probability proportional to their size (2 within each stratum) and then matched them with 50 control PSUs (corresponding to 65 municipalities). This matching was done within each stratum and based on population size and the QLI.

As Table 1 shows, with the exception of the number of banks and rural inhabitants, no statistical differences emerged between treatment and control municipality characteristics (health infrastructure, education infrastructure, economics, and sociodemographics). Moreover, even though the number of banks may be related to other characteristics associated with educational outcomes, the lack of any significant differences in education, health infrastructure, or the QLI suggests that both groups are comparable. Nonetheless, all characteristics (including number of banks) are carefully controlled for throughout the study because even differences that are not statistically significantly different from zero may produce biased treatment effects.

Finally, even though the evaluators randomly selected eligible households *within* each municipality, in some municipalities the program had already started, meaning that preprogram

data are not available for these participants. To tackle this problem, the program evaluators divided the treatment sample into two sets of 25 PSUs, one for municipalities in which the program had begun before the baseline measurement (here labeled the treatment with incomplete baseline group or TIB), and the second for those whose baseline data were collected before program inception (the treatment with full baseline group or TFB). Moreover, even though the TIB group baseline data were collected after treatment had already started, the research group was very careful to collect as many retrospective data as possible. Therefore, the following discussion stipulates whether retrospective data were available (meaning use of the full treatment group) or not (meaning use of the TFB group 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.

### **Outcome measures**

Both the baseline and follow-up surveys asked about grade retention for all school-aged children in the household, meaning that these data are available for all grade levels. In addition, grade retention information was asked retrospectively of the TIB group, allowing analysis of the full treatment group.

On the other hand, because test score data are only available for children in the fifth or ninth grade, the available sample for the test scores analysis is much smaller, a total of 831 cases (624 fifth graders and 207 ninth graders). These test scores correspond to the *Pruebas Saber*, the national exam taken in these two grades that covers language, math, and citizenship skills, although this study uses only the language and math test scores. In addition, because only one set

of (posttreatment) test scores is available, the analysis uses no pretreatment test score measures. Thus, even though it would be ideal to use only the TFB sample, because of sample size limitations, the analysis must draw on the full sample (and control for the grade retention measured retrospectively).

#### **IV. EMPIRICAL STRATEGY**

One major challenge in measuring the program's effect on school performance is that the grade retention and test scores are only available for children enrolled in school. Given that this program has a significant effect on enrollment, particularly among adolescents (see Attanasio et al., 2006 and below), a simple comparison of grade retention rates or average test scores between beneficiaries and nonbeneficiaries will not provide an unbiased estimate of its effect because in the absence of the program, many beneficiary children would not have enrolled or would have dropped out of school (and may have different characteristics than those enrolled despite the program).

To illustrate this point, the first four columns of Table 2 list the pretreatment characteristics of treatment group and control group subjects, respectively, who were enrolled in school at follow-up, and the fifth column presents the *t*-test of the difference in means across both groups.<sup>3</sup> Conditional on enrollment at follow-up (i.e., the group for which achievement data are available), marked differences emerge; 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). More important, treatment group subjects enrolled at follow-up have less favorable 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.

In sum, *Familias en Accion* is attracting less advantaged children to enroll in school, which is fundamentally good. However, in estimating the program's impact on school performance, any comparison of school outcomes between the entire treatment group and the control group would include youth with less favorable characteristics both socioeconomically and academically. Therefore, a negative effect would be likely due to compositional changes rather than real changes in achievement.

As previously discussed, only two studies in the CCTs literature (Jere R Behrman et al., 2005; Jere R. Behrman et al., 2000), both using data for the Mexican program *Progresa*, have taken this compositional effect into consideration. Whereas Behrman et al. (2000) weighted the treatment group to match the control group on the two observable pretreatment characteristics of age and sex, it could not include more characteristics because of data limitations. To account for attrition between the baseline and follow-up surveys, Behrman et al., (2005) used the probability of attrition given a set of pretreatment characteristics,  $X$ , to reweight the posttreatment observations to produce the same distribution of  $X$  as prior to attrition.

This current analysis relies on similar intuition but extends it in several ways. It makes use of a framework termed by Frangakis and Rubin (2002) as *principal stratification* which is increasingly being used to address the general issue of how to either control for or deal with selection on a post-treatment variable (see also, Imai, 2008; Roy, 2008; Yannis, Rotnitzky, Shepherd, & Gilbert, 2007). There are two key ideas here. The first is that, implicitly, we can only make inferences about the impact of the program on test scores and grade retention for those who would have stayed in school even in the absence of the program (unless we want to

treat these scores as missing data – an approach we will not take in this paper). The tricky part however is that enrollment is a *consequence* of the program therefore we cannot control for it directly because, as described above, the types of people who enrolled in the treatment group were quite different from those that enrolled in the control group. Therefore the second key idea is that, due to the compositional effects that result from differential selection into enrollment across the treatment and control groups, we'll have to perform adjustments if we want to try to compare similar kinds of people across treatment and control groups – that is, we can't just rely on the randomization any longer.

Formally, we can *define* such a group of comparables based on enrollment potential enrollment outcomes — that is, 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.<sup>4</sup> The study goal is to estimate the impact of the program on test scores and grade retention for the Always Enrolled, that is, those students who would have been enrolled in school even if they hadn't had access to the program, because test scores only apply to children in school.

Within the control group we can identify the Always Enrollers because they are the only ones we observe to be enrolled in school. Although we cannot directly observe which students in the treatment group are also Always Enrollers, we can attempt to identify them through their

comparability to the control group Always Enrollers with regard to a rich array of observed covariates. Specifically, control group subjects who were enrolled in school both at baseline and first follow-up (that is, excluding Newcomers and Dropouts) are matched to the treatment group based on pretreatment characteristics through use of a propensity score (described below) that is calculated based on the observed covariates. Importantly, since we are using observed covariates to identify our treatment effects, we need to assume that we have controlled for all covariates that are associated with both school enrollment and subsequent test scores.

This methodology is an improvement over previous methods because (a) the analysis excludes students who enrolled after the program's inception (instead identifying those that would have stayed in school even in the programs' absence), allowing to measure the program's effect on grade retention and test scores conditional on enrollment (i.e., whether CCT produces 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 (following Table 3, this method does not allow for inferences about Newcomers).

### **Matching**

We match both groups using propensity score matching, a statistical technique that pairs observations from a treatment and control group whose pretreatment characteristics are similar enough that the control group observations are counterfactual to those of the treatment group (Rosenbaum & Rubin, 1983). If matching successfully creates two observationally similar groups, then differences in the outcome measures between the two groups are more likely the result of treatment (in this case, the CCT) than preexisting population differences.

Propensity score matching consists of three main steps. The first, which typically uses a logistic or probit regression, is calculation of the propensity score, 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  $\mathbf{X}$  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). Next, each observation in the treatment group is matched to the control group observation having the closest propensity score. The main idea behind is to find a group among the control group that looks as similar as possible to the treatment group with respect to observed covariates. In this situation, when two groups of treatment and control units have the same value of  $e(\mathbf{X})$ , the distribution of  $\mathbf{X}$  is the same for both groups (Rosenbaum & Rubin, (1983; Rosenbaum & Rubin, 1984).

Here, to match control group subjects enrolled at baseline that had not dropped out by follow-up, 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 control group and  $S = 1$  if enrolled both at baseline and follow-up. 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  $\mathbf{W}$  is a vector of child and household characteristics, and  $\mathbf{M}$  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], and the number of durables<sup>5</sup> 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 each control group subject enrolled in school both at baseline and at follow-up is matched with a treatment group peer having the nearest propensity score. Matching with replacement allows each treatment group observation to be used more than once (i.e., more than one control group subject can be matched to the same treatment group subject), and treatment units 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 procedure results in a treatment group whose members look 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, in which treatment observations are weighted by the number of times they were matched to a control observation. Running a regression on the matched groups that includes the baseline covariates can improve efficiency and precision of the estimates relative to a simple (weighted)

difference in means (Dehejia & Wahba, 2002; Rubin & Thomas, 2000). For the outcome of grade retention, the following equation is estimated:

$$\log(P_i/1-P_i) = \alpha + \tau T_i + \beta X_i + \gamma M_j + \varepsilon_i$$

where  $\log(P_{ij}/1-P_{ij})$  represents the log odds of a child  $i$  in municipality  $j$  repeating a grade.

$T$  is a treatment group indicator, and  $\tau$  is the treatment effect.

It is important to underscore the assumption that, conditional on  $\mathbf{W}$  and  $\mathbf{M}$ , no differences in unobserved characteristics exist between matched treatment and control groups that would 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.

Finally, to explore possible mechanisms that produce effects on grade retention or test scores (if any), differences-in-differences estimators measure the program's effect on the following: school attendance, a child's time use (including homework and outside work), and a child's health.

## **V. RESULTS**

The treatment effects were estimated separately for children (here defined as those 7–12) versus adolescents (13–17) because of expected age-related differences. First, parents have more control over younger offspring, meaning they can more easily enforce school enrollment and regular attendance. Likewise, because the opportunity cost of attending school (rather than work) is higher for adolescents than for children, the decision to enroll in school may be more difficult for the former than the latter. Finally, the older the student (and the higher the grade attended), the more difficult the school content, so parents may find it more difficult to help their offspring with schoolwork and enforce homework.

Within each age group, the effects were also estimated separately for urban and rural residents in case the program effects differed by context. First, schools differ greatly between urban and rural areas, not only in terms of infrastructure and resources (which are likely to be better in urban schools) but also in terms of teaching methodologies. Likewise, the opportunity cost of attending school (versus working) is different in urban than in rural areas because of differences in the need for child labor and the compensation paid. Finally, rural areas have been more affected by Colombia's violent conflict than urban areas and are more influenced by guerrilla and paramilitary groups.

### **Effects on enrollment**

First, to set the context for estimating the effects on school achievement, Table 4 outlines the effects of *Familias en Acción* on school enrollment by age group and rural versus urban. Based on difference-in-difference estimates, the program has significantly increased enrollment, but the effect is larger for adolescents (13–17), with a 4.6% increase among urban and a 3.3% increase among rural adolescents, than for children (7–12), a 2.4% increase for urban children and a 2.3% for rural.

### **Balance of pretreatment characteristics**

Table 5 reports the balance achieved after control group subjects enrolled at baseline that did not dropout by first follow-up had been matched with treatment group subjects based on pretreatment characteristics. The first panel corresponds to the standardized difference in means between control and treatment groups before and after matching using the full sample, which includes the control, TFB (treatment with full baseline), and TIB (treatment with incomplete baseline) groups. The second panel lists the same estimates for the restricted sample, which excludes the TIB group. For both samples, once control group subjects enrolled at both baseline

and follow-up are matched to their treatment group counterparts, balance between treatment and control groups on most characteristics is achieved. That is, except for number of banks per person, standardized differences in means for the matched sample are not higher than 0.05, indicating no substantial postmatch differences in background characteristics between the control and treatment groups within the matched sample. The only postmatch characteristic that is substantially different across treatment and control group is number of banks per person, primarily because some control municipalities have no bank, which makes it difficult to obtain a good match on this characteristic. Moreover, even though the number of banks may proxy for other economic variables related to school outcomes, there is a good balance between treatment and control group for all other municipality characteristics. Nonetheless, this variable is included in all the 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 to 12 years old) and then for adolescents (13 to 17 years old).

## **Effects on children**

### *Effects on grade retention among children*

Table 6 presents the treatment effect estimates on grade retention for children 7 to 12. The top panel lists the estimates for the entire treatment group (TFB and TIB), while the bottom panel gives those for the restricted treatment group (TFB only). The first panel at left gives the estimates for the unmatched complete cases sample; and the second, those for the matched sample. The first and second columns within each panel correspond to the treatment effect and standard error after adjustment for pretreatment background characteristics (as given in Table 5).

As Table 6 shows, the estimates for the matched sample suggest that *Familias en Acción* has a significant effect in reducing grade retention among children in rural areas. On average, for this age group in rural areas, the program reduces the probability of grade retention by 3%, a non-negligible effect given the 2002 average grade retention rate in Colombia of 6% (Ministerio de Educación Nacional, 2002).

Although the estimates using the restricted sample (i.e., excluding treatment group children whose baseline data were collected after program inception) are not significant, most are in the same direction as those for the entire treatment group, possibly because of the restricted treatment group's (TFB) shorter exposure to the program.<sup>6</sup> This assumption of a dosage effect is tested using the amount of money received by the second follow-up as the treatment. As Table 7 shows, consistent with the above results for a dichotomous treatment, strong grade retention reduction is observable for young rural children but not for urban children. Additionally, for rural children, this effect clearly increases as the total subsidy received increases, suggesting that the longer rural children stay in the program, the better the probability of reduced grade retention.

#### ***Effects on test scores among children***

Estimates of the effect of *Familias en Acción* on test scores for fifth graders are presented in Table 8. As in the grade retention analysis, regression-adjusted estimates are given for the unmatched sample (first panel) and for the matched sample (second panel). As noted earlier, the sample size for this analysis is much smaller, initially 624 cases for fifth grade. Once control group subjects enrolled at both baseline and follow-up are matched with their corresponding treatment group peers, 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 show that among fifth graders, treatment group children do better in math than those in the control group, particularly in rural areas. Likewise, results for the matched sample suggest that the program has a stronger effect on math and language scores for rural fifth graders than for urban fifth graders, with a 3.8 point increase in math scores (40% of the *SD*) and a 3.2 point increase for language scores by 3.2 points (almost 47% of the *SD*).

### ***Other outcomes and possible mechanisms among children***

The program effect on other outcomes that may lead to higher long-run achievement (i.e., school attendance, a child's time use, and a child's health) is estimated using differences-in-differences. Table 9 presents the average treatment effects of *Familias en Acción* using the restricted sample (i.e., excluding the TIB group, for which there are no feasible retrospective data on these particular outcomes). The first panel at left shows the treatment effects and standard errors for all children 7–12, while the second and third panels show estimates for urban and rural children, respectively. Clearly, *Familias en Acción* has a significant effect on school attendance (conditional on enrollment) for urban children.

In terms of children's time use, *Familias en Acción* significantly reduces child labor among rural children (both the probability of working for paid or unpaid work and total hours worked). However, although the signs for the effects on child labor for urban children are also negative, they are not significant, suggesting that the effects on child labor among children are concentrated in rural areas. Nonetheless, not only does the program reduce household work for both urban and rural children, it increases the time that children devote to homework, with the effect being larger among urban children. Finally, the program has a positive effect on child health, particularly among urban children.

The fact that the program's effect on school attendance and time devoted to homework is larger for urban children than for rural children is somewhat surprising given that the opposite is true for the program effect on grade retention and test scores. This difference may occur because children living in urban areas are making a greater time commitment to participating in the program. This assumption is supported by the program's virtually nonexistent effect on reducing child labor among urban children, meaning that these children are increasing their study time without necessarily reducing their work time. Consequently, they may be more tired and have less energy to concentrate in class.

### **Effects on adolescents**

#### *Effects on grade retention among adolescents*

In Table 10, which presents the treatment effect estimates on grade retention for adolescents 13–17, the top panel shows estimates for the entire treatment group (TFB and TIB) and the bottom panel, estimates for the restricted treatment group (TFB). The first panel at left gives the estimates for the unmatched sample, while the second panel presents those for the matched sample. The first and second columns within each panel correspond to the treatment effect and standard error after adjustment for pretreatment background characteristics (listed in Table 5).

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 actually *increased* grade retention for rural adolescents. This finding implies that among rural adolescents, the program is having a detrimental effect on performance, particularly among those children who are and would have been enrolled in school even in the absence of the program. For this group, the increase in grade retention is about 1.2%, a small but undesirable effect.

Table 11 presents estimates of the dosage effects when the amount of money received by the first follow-up is used as the treatment. Consistent with the results presented in Table 10, the program has no effects for urban youth but has detrimental effects for rural youth, with the treatment estimate for grade retention increasing as the amount of payment increases. It should be noted that the total amount of payment received is also a proxy for time exposure to the program. Therefore, this detrimental effect may not necessarily result from the cash transfer itself but from other events during the youth's exposure to the program. One possibility is that since the program has a strong effect on enrollment (particularly among adolescents), classes are becoming more crowded and the quality of education may not necessarily be improving. Indeed, the negative increase in grade retention for adolescents in rural areas (but not in urban areas) may indicate that urban schools are addressing the increased enrollment differently than rural schools. Likewise, the children may be behaving in ways that do not facilitate learning, for example, maintaining their number of labor hours.

#### *Effects on test scores among adolescents*

Table 12 presents the regression-adjusted estimates of the program effect on test scores for ninth graders, both for the unmatched sample (first panel) and the matched sample (second panel). As in the analysis for fifth graders, the sample size here is much smaller than the initial 207 cases, being reduced to 111 once the treatment group is matched to control group subjects enrolled both at baseline and follow-up. 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 positive effect on adolescent test scores. Moreover, when the matched

sample is used, the program has a negative effect on math test scores among urban adolescents and on language scores among rural adolescents.

### *Other outcomes and possible mechanisms among adolescents*

The analysis for adolescents also addresses the program's effect on the other three outcomes that may lead to higher long-term achievement: school attendance, a child's time use, and a child's health. Table 13 presents average the treatment effects of *Familias en Acción* using the restricted sample (i.e., excluding the TIB group), estimated using differences-in-differences. The TIB group is again excluded from the analysis because of a lack of retrospective data.

The first panel at left shows the treatment effects and standard errors for all adolescents (13–17), while the second and third panels show estimates for urban and rural adolescents, respectively. These results indicate that *Familias en Acción* has no reducing effect on school absenteeism among adolescents (rather, there is a small increase in total days absent from school for urban adolescents). 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.

## **VI. 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 but practically no effect on children living in urban areas. At the same time, they also indicate that the program may actually have a negative effect on the school achievement of adolescents (13–17). In terms of other outcomes that may be related to school achievement in the long run, there are clearly differential effects by age and rural versus urban, which may be due to differences in household dynamics (e.g., as a

result of the program, rural adolescents are working just as much or doing more homework) or in the schools (e.g., older children are enrolling in larger numbers than younger children, so crowded classrooms may make teaching more difficult).

The results suggest that the program has a positive effect on rural children's achievement through a 3% decrease in the probability of grade retention and an increase in 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.

On the other hand, the program has virtually no effect on grade retention or test scores for children living in urban areas, even though the results for other outcomes suggest that, despite not working less, these children are healthier and more likely to be attending school and doing more homework. This finding may explain the absence of any effects on achievement for this group: because children living in urban areas are devoting more time to school activities but not reducing their work load outside the home, they may be more tired and have less energy to concentrate and do well in school.

In addition, *Familias en Acción* has no positive effect on adolescent school achievement (either rural or urban dwellers), although the fact that children over 12 are returning to school and not dropping out as a consequence of the program is a very positive outcome. Specifically, this study found that the program has a positive effect on enrollment for about 4% of adolescents aged 13–17, an important finding 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 (as in the case of urban adolescents), its large effect on enrollment means an important increase in educational

coverage for very poor children whose access to education has traditionally been limited, which brings the government goal of universal coverage closer.

For adolescents living in rural areas, however, the program may be having a detrimental effect by actually increasing grade retention, especially among those who were or would have been enrolled even in the absence of the program. At the same time, no effect (positive or negative) 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 admittedly 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 well outweigh the negative effect on grade retention.

One possible reason for this negative effect on rural adolescents but not rural children may be that in rural areas, adolescent labor is highly valued (and needed), so that even though children are enrolling in school, they are not necessarily working less or doing more homework and may not have more energy or capacity to concentrate more on or invest more in learning. Alternatively, this differential may be related to school characteristics having a negative effect on school performance. For example, it is precisely among this age group that enrollment effects are relatively strong, meaning that the classes may be more crowded, the quality of instruction deteriorating, and/or the class composition may simply be changing. Likewise, the school quality for higher grades may simply have been inadequate from the outset, even before the program began, and therefore enrolling more children does not translate into better performance.

For adolescents living in urban areas, the program has no effect on school failure but a negative effect on test scores. However, in terms of the other outcomes, among urban adolescents, in contrast to urban children, the program reduces the time devoted to work (both inside and outside the home), but, as for urban children, it increases the time devoted to homework. One possible explanation (as in the rural adolescent's case) may be overcrowding or compositional changes in the classroom.

Another possible explanation for the lack of effect on adolescents' school performance is the fact that the subsidy is provided even up to 18 years of age. Thus, while children enrolled in primary school may have an incentive to progress through grades and enter secondary school (where the subsidy amount is doubled), once the student is enrolled in secondary school, there may be a perverse incentive to keep repeating grades so that the subsidy will continue to be awarded.

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. Nonetheless, as noted above, the fact that these children are deciding to enroll in school is in itself a positive outcome. Additionally, even though it cannot be tested, given the program's positive effect on school attendance for all children, if the quality of schooling is adequate, these newly enrolled children should be acquiring skills and information.

In sum, the program is improving the achievement of young rural children but having no effect on young urban children and (based on the matched sample results) may be reducing adolescent achievement, particularly among those who were and would have been enrolled even in the program's absence. Thus, 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.

The main policy implication of the findings is that attention must be paid to the implementation of such programs at the school level. That is, even though *Familias en Acción* 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. In reality, only attention to the quality of public education can assure that enrollment and attendance translate into learning. In other words, only if quality is part of the intervention can CCT programs be an effective policy tool, not only for short-term poverty alleviation but also for long-term human capital accumulation.

## Notes

<sup>1</sup>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.

<sup>2</sup> 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.

<sup>3</sup> 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.

<sup>4</sup> 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, Imbens, and Rubin (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.

<sup>5</sup> Durables include refrigerator, sewing machine, television, music equipment, fan, blender, kerosene lamp, and electric generator.

<sup>6</sup> 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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## Tables

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

	Treatment group		Control group		t <sup>1</sup>
	Mean	S.D.	Mean	S.D.	
<b>Health infrastructure</b>					
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
<b>Education infrastructure</b>					
# 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
<b>Economic characteristics</b>					
Taxes per 10,000 inhabitants <sup>2</sup>	82.9	211	85.1	235	0.05
Banks per 10,000 inhabitants	.701	.477	.404	.919	- 2.19*
<b>Sociodemographic characteristics</b>					
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+
<b>N</b>	<b>57</b>		<b>65</b>		

<sup>1</sup> t-statistics of the difference in means across groups. + $p < 0.10$ ; \* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$

<sup>2</sup> 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.		
<b>Children characteristics</b>						
Age	10.64	2.60	10.74	2.63	-2.13	*
Gender (girl)	.49	.50	.49	.50	-0.31	
<b>Household composition</b>						
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	
<b>Socio-economic characteristics</b>						
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 <sup>1</sup>	.14	.35	.23	.42	-13.4	***
Dirt floor	.39	.49	.45	.50	-6.81	***
<b>Municipality characteristics</b>						
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	***
<b>N</b>	<b>5,198</b>		<b>7,968</b>			

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

<sup>1</sup> Recycled materials, zinc, carton or other poor-quality materials.

**Table 3.** Potential enrollment outcomes

Control group	Treatment group	Behavioral type
Enrolled in school	Enrolled in school	Group A: Always Enrollers
Not enrolled in school	Enrolled in school	Group B: Newcomers
Not enrolled in school	Not 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

	Fullsample (TFB & TIB)		TFB Only	
	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 <sup>1</sup>	-0.24	0.02	-0.13	-0.01
Dirt floor	-0.12	-0.01	-0.05	0.02
Dwelling has poor walls <sup>1</sup>	-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
<b>N</b>	<b>13,166</b>	<b>7,964</b>	<b>8,916</b>	<b>7,034</b>

Columns contain standardized difference in means.

<sup>1</sup> 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
<b>TFB &amp; TIB</b>						
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
<b>TFB ONLY</b>						
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.  
 + $p < 0.10$ ; \* $p < .05$ ; \*\* $p < .01$ ; \*\*\* $p < .001$

**Table 7.** Dosage effects for grade retention among young children (7 to 12 years old)

	Unmatched		Matched	
	t.e.	s.e.	t.e.	s.e.
<b>Urban</b>				
Payment (omitted: 0)				
1–\$500,000	.014	.012	.015	.017
\$500,000–\$1,000,000	-.004	.010	.002	.015
\$1,000,000–\$1,500,000	-.006	.011	-.008	.014
\$1,500,000 or more	-.014	.016	-.005	.023
<b>N</b>		<b>4,239</b>		<b>3,015</b>
<b>Rural</b>				
Payment (omitted: 0)				
1–\$500,000	.015	.015	-.017	.020
\$500,000–\$1,000,000	-.014	.011	-.032	.018 +
\$1,000,000–\$1,500,000	-.031	.009 **	-.045	.014 **
\$1,500,000 or more	-.032	.015 *	-.037	.019 +
<b>N</b>		<b>4,790</b>		<b>2,671</b>

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 effect estimates for test scores among 5<sup>th</sup> graders

	Unmatched			Matched		
	t.e.	s.e.	N	t.e.	s.e.	N
<b>Math</b>						
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
<b>Language</b>						
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 9.** 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.	
<b>School attendance</b>									
Skipped school in the last month	-.034	.016	*	-.065	.022	**	-.018	.023	
Days absent from school last month	-.153	.085	+	-.216	.111	+	-.114	.130	
<b>N</b>	<b>5,681</b>			<b>2,781</b>			<b>2,900</b>		
<b>Child time use</b>									
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	***
Time doing homework	.422	.050	***	.591	.087	***	.277	.060	***
<b>N<sup>1</sup></b>	<b>3,255</b>			<b>1,557</b>			<b>1,698</b>		
<b>Health</b>									
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	
<b>N</b>	<b>6,582</b>			<b>3,104</b>			<b>3,478</b>		

For discrete outcomes, treatment effects are marginal effects dy/dx after logistic regression.

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

<sup>1</sup>Work last week was only asked of children 10 years old or older.

**Table 10.** 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
<b>TFB&amp;TIB</b>						
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
<b>TFB ONLY</b>						
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 11.** Dosage effects for grade retention among adolescents (13 to 17 years old)

	Unmatched		Matched	
	t.e.	s.e.	t.e.	s.e.
<b>Urban</b>				
Payment (omitted: 0)				
1–\$500,000	.002	.010	.006	.013
\$500,000–\$1,000,000	-.010	.011	.001	.010
\$1,000,000–\$1,500,000	-.004	.010	.001	.013
\$1,500,000 or more	-.006	.013	-.007	.007
<b>N</b>	<b>1,770</b>		<b>1,232</b>	
<b>Rural</b>				
Payment (omitted: 0)				
1–\$500,000	.027	.020	.020	.020
\$500,000–\$1,000,000	.013	.015	.008	.004+
\$1,000,000–\$1,500,000	.012	.013	.023	.008**
\$1,500,000 or more	.015	.014	.075	.016***
<b>N</b>	<b>1,470</b>		<b>695</b>	

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

**Table 12.** Average treatment effect estimates for test scores among 9<sup>th</sup> graders

	Unmatched			Matched		
	t.e.	s.e.	N	t.e.	s.e.	N
<b>Math</b>						
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 <sup>1</sup>	22
<b>Language</b>						
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 <sup>1</sup>	19

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

<sup>1</sup> Difference in means (no regression-adjusted because of sample size limitations)

**Table 13.** 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.		
<b>School attendance</b>									
Skipped school in the last month	.040	.030	.030	.042		.046	.045		
Days absent from school last month	.108	.150	.344	.179	+	-.190	.260		
<b>N</b>	<b>1,948</b>		<b>1,145</b>			<b>803</b>			
<b>Child time use</b>									
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	+
Time doing homework	.233	.045	***	.552	.074	***	-.052	.052	
<b>Health</b>									
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	
<b>N</b>	<b>4,141</b>		<b>2,097</b>			<b>2,044</b>			

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





