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The effects of conditional cash transfers on schooling and child labor of nonbeneficiary siblings

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ABSTRACT

This paper evaluates the existence of spillover effects of the "Más Familias en Acción" program on eligible children who are not beneficiaries of the subsidy while their siblings are. Using a nonexperimental design, through a propensity score matching and a difference-in-differences model, we found a negative effect on the school enrollment of nonbeneficiary siblings, as well as an increase in their absenteeism. Furthermore, we found heterogeneous spillover effects on child labor by gender and age. Based on the results, we recommend redesigning the conditional cash transfer program by expanding the coverage to a household level, instead of limiting the number of beneficiary children per household.

1. Introduction

During the last quarter of 2018, 1.1 million Colombian children aged 5–17 worked an hour or more per week in some economic activity and performed domestic chores for 15 h or more weekly. That is, 10.3% of children worked during their childhood or adolescence (DANE, 2019).

In Colombia and most countries in the world, child labor is considered undesirable since it can harm children's development, training, and performance in other activities that contribute to their well-being and future achievements (ICBF, 2017). Furthermore, child labor is a social problem because it compromises the education level reached by children, limiting human capital accumulation and economic growth (Holgado et al., 2014; Psacharopoulos, 1997; Khanam, 2007; Akabayashi and Psacharopoulos, 1999).

From a public policy perspective, the first step in eliminating or reducing child labor is to understand the reason adult household members allow their children to work. According to the Great Integrated Household Survey (GEIH by its Spanish initials), 85% of young Colombians work because of economic incentives (to participate in the family's economic activity, to have their own money, or to help with household or education expenses). Consequently, governments should design public policies that not only search to discourage child labor but alleviate poverty and reduce economic stress in households as well.

Among the most popular antipoverty programs are conditional cash transfers (CCTs) which provide subsidies to vulnerable families given that school-age beneficiaries attend at least 80% of their classes and children under 7 years of age have growth and development checks.

Currently, CCTs programs are the most important anti-poverty policies implemented in several developing countries because they aim to reduce economic stress in the household while encouraging school attendance (given the conditionality).[1] However, these programs do not only impact beneficiary children but also have spillover effects on their relatives' outcomes. For example, they influence women's agency within the beneficiary family (Litwin et al., 2019); nutrition outcomes of the household (Kronebusch and Damon, 2019); household vulnerability (Uchiyama, 2019); income inequality and intergenerational transmission of poverty (Kitaura and Miyazawa, 2021). Furthermore, CCTs programs may have an impact on the outcomes of non-beneficiary children considering the exclusion of some eligible children because they do not comply with the program's requirements or because the program has a limit of beneficiaries per parent (Attanasio et al., 2005).

Theoretically, when eligible children are not beneficiaries but their households are transfer holders (hereafter referred to as non-beneficiary siblings), changes in their school enrollment and labor participation may occur. For instance, the expansion of the household's budget may allow an increase in the education resources for all household members, as

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[1] In fact, governments have reacted to the Covid-19 pandemic by offering households one additional temporary transfer to alleviate the reduction of their monetary resources (see Cejudo et al., 2020; and Blofield et al., 2020).

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well as a reduction of child labor since their job income is partially or completely substituted by the transfer. However, it might also be the case that school enrollment decreases and child labor increases among nonparticipants because they miss classes to comply with their work and that of their beneficiary siblings, who now must attend classes as a program's requirement. The net impact depends on which effect dominates.

Empirically, there is a handful of studies where researchers either did not find any impact on schooling or child labor of non-beneficiary children (Galiani and McEwan, 2013; Ferreira et al., 2009; Baez and Camacho, 2011) or found a significant effect but in opposite directions. For example, in Nicaragua, Lincove and Parker (2016) found that the CCTs have a positive (negative) relation with schooling attendance (hours worked) of non-beneficiary siblings, while in Colombia, Barrera-Osorio et al. (2008) found the opposite effects through an experiment carried out in the capital city.

Policymakers aiming to incentivize school attendance, discourage child labor, and reduce current and future poverty must have robust evidence about the final direct and indirect effect of a CCTs program on children's welfare. However, the literature related to spillover effects of CCTs on non-beneficiary siblings is scant, due to the unavailability of data or methodological tools. The paper by Barrera-Osorio et al. (2008) uses data from a randomized process developed in two localities at the Colombian capital city, but their results are not representative at a country level. In other words, there is a trade-off between desirable methodological tools and statistical representativeness due to the high costs associated with experimental designs.

Considering the importance of studying spillover effects of CCTs programs in non-beneficiary siblings, the main objective of this paper is to evaluate the effects of the "Más Familias en Acción (MFA)" program on school attendance and child labor of nonbeneficiary siblings in Colombia. Here, we use national open data to implement conventional methodologies such as the propensity score matching technique and the difference-in-differences model.

Our contribution is to provide evidence of spillovers effects of a CCTs program in Colombia, whereupon we might guide both Colombian policymakers and developing countries governments to design the most effective program considering a broader picture of the possible responses of households to a CCT. In addition, this paper presents evidence supporting the need to redesign the MFA program to minimize the potential negative externalities of this transfer on nonbeneficiary siblings. Specifically, based on the finding of a negative effect of the program on the nonbeneficiary siblings' school attendance, we call for the expansion of the program's coverage at the household level.

This document has six sections including this introduction. The second section contains a theoretical framework of how a household decides the child labor supply and how public policy influences this decision. The third section contextualizes the case of CCTs in Latin American countries such as Colombia. The fourth section refers to the data used and the methodology employed. The fifth section corresponds to the results and some robustness checks. Finally, the sixth section summarizes the conclusions and possible future research projects.

2. Theoretical mechanisms for spillovers effects of CCTs

2.1. Intrahousehold resource allocation

During childhood and adolescence, parents decide on children's consumption basket, their daily activities, and the proportion of household income allocated to their welfare. In this regard, it is essential to study the economics of the family to analyze topics related to child labor or school enrollment.

Two theories on family economics that study decision-making and intrahousehold resource allocation exist. The unitary model theory considers households as homogenous units where decisions are made to benefit the family as a whole. It is assumed that all household members have the same preferences and that their interests are aligned and represented in the decisions made by one household member (Serrano, 2003).

The second is the so-called non-unitary model theory, according to which the household is collective, where members present conflicts of interest. Thus, some collective elections are Pareto-optimal, that is, it is impossible to improve the welfare of a member without affecting others, given the scarcity of resources (Haddad et al., 1998).

Considering a household with an altruistic parent and more than one school-age child, both theoretical approaches show that the family will decide to invest more in the education of the child with a higher return to education, to maximize the family's utility. As an example, Parish and Willis (1993) described the situation in Taiwan during the years 1940–1950, when women had lower educational levels than men because returns to education in the agricultural sector were much lower for the female than for the male labor force.

Given the imperfections in the credit market and the household's budget restrictions, parents will have to decide which child studies and which child works, even if they care about the welfare of all their children (Basu and Van, 1998). In this sense, following a non-unitary model framework, the scarcity of resources faced by the household generates a rivalry between siblings for the distribution of income.

This rivalry between siblings appears because the amount of resources allocated to each child is inversely proportional to the number of children in the household: to conduct an equitable distribution of n resources, in a family made up of homogeneous children, each of them would get $1/n$ of the total resources (Garg and Morduch, 1998). Furthermore, observable differences between children or cultural aspects can lead parents to make unequal distribution of resources. Examples of factors affecting resource distribution are the biological sex of the child (Ono, 2004; Garg and Morduch, 1998), the order of birth (Tenikue and Verheyden, 2007), and the parents' desire to compensate for preexisting differences between children (Leight, 2017; Berry et al., 2019).

In short, assuming that parents care about the well-being of their children, the decision to send a child to school or to work not only responds to limited resources but is also the result of economic, cultural, and social contexts that put some children at a disadvantage compared to others.

2.2. Family economics and public policy

The public policy implications differ from the two theories previously analyzed (Haddad et al., 1998). Under non-unitary models, the impact of public policy depends on the member of the household who is directly intervened. Conversely, unitary models predict that the success of a policy on household members' well-being is independent of the family member who is intervened. Consequently, policymakers need to determine the theoretical framework they are working with since public intervention can generate indirect effects.

Strong theoretical reasons exist to assume the existence of negative spillover effects associated with a CCTs program. For example, Berry et al., (2019) recognize two determinants of intrahousehold educational resource allocation in the presence of a CCT. The first refers to an attempt to maximize returns on the investment in education, which can have a negative effect on the siblings who are not getting the transfer because their relative cost increases (compared to beneficiary siblings). The second mechanism leads parents to minimize inequalities in their children's endowments, which increases schooling investment on all children, including those who do not directly get the subsidy. The combination of the two mechanisms triggers an ambiguous net effect on the education of nonparticipating siblings.

Similarly, Ferreira et al. (2009) mention the existence of two economic effects that could affect, in opposite directions, the schooling and child labor of nonbeneficiary children. The first is the income effect, which would increase the school attendance of children (beneficiary or not) because there are more resources at home. The second is the displacement effect, which has a negative impact on nonbeneficiary siblings since the labor offered by the beneficiary children –who must now attend school given the conditionality of the CCT– is substituted with the labor of their nonbeneficiary siblings.

Although the net effect of CCTs on the well-being of children who are not recipients of the subsidy will depend on which of the effects dominates, negative spillover on non-beneficiary siblings are relevant if policymakers believe that family well-being is not equivalent to individual well-being. In other words, given a CCT that limits the number of beneficiary children, even when a family maximizes its utility, this policy design may affect the educational performance of non-beneficiaries.

3. CCTs history and the “Más Familias en Acción” program

At the end of the 1990s, Brazil and Mexico introduced CCTs as a policy instrument used to fight poverty. The subsidies are conditional to school-age children attending classes, and children under five undergoing growth and health controls. Nowadays, almost 40% of Latin American and Caribbean countries[2] have implemented CCTs programs, which have benefited more than 129 million children (Ivaschesko et al., 2018).

Although CCTs spread rapidly among developing countries, the program design differs from one place to another and responds to the country’s social and cultural context. One of the most important features to decide in a CCTs program design is the amount of the transfer and the target population.

Frequently, the design of the transfer associated with educational requirements varies according to the household’s structure. For instance, some programs allocate a fixed amount to each child in the household to avoid discrimination by gender; other programs allocate a higher transfer to older children considering their higher opportunity cost; some programs limit the number of beneficiaries per home to discourage fertility among transfer recipient parents (Inter-American Development Bank, 2017).

There are two main reasons for limiting the number of beneficiaries per household: first, to avoid potential increments in fertility – e.g. “PRAF” in Honduras (CEPAL, 2007)-. Second, to encourage scale economies in consumption,[3] e.g. “Familias por la inclusión social” in Argentina, which gives 53 dollars monthly to a family with two children and 12 dollar to each additional child (six beneficiary children as maximum) (CEPAL, 2011).

Regarding Colombian experience, there was a serious economic recession from 1998 to 1999 that affected the economy of the most vulnerable families[4] (Núñez Méndez and Ramírez Jaramillo, 2002). To face this situation, in 2000, the government created the program “MFA” to support families to keep their children in school and guarantee them adequate levels of nutrition and health care (Departamento Nacional de Planeación, 2000).

For these purposes, the government delivers two types of subsidies: an education incentive that benefits children under 18 who attend at least 80% of classes and a nutrition incentive for children under 7 years for attending growth and development monitoring. Currently, this CCTs program benefits more than 3 million children and teenagers.

Given the benefits of the transfers found by studies conducting short- and medium-term impact evaluations (see Attanasio et al., 2005; Attanasio and Gómez, 2004), the Colombian government decided to expand the program to other municipalities. However, to avoid a potential increase in fertility rates due to the transfer, the number of school-age children per household who could benefit from the program was limited to three (Attanasio et al., 2005). Considering this limitation, this paper provides evidence for the existence of an adverse effect on school attendance and child labor of nonbeneficiary siblings.

Even before the limit of three beneficiary children per parent was implemented, some kids from recipient households were excluded from the program. The survey that we employ in this paper asks the adult respondent why the child is not in the program. The reasons they mentioned were: the required documents were not complete; parents did not know the child was eligible; parents did not know how and where to register the child; they did not realize that registration was open, among other reasons.

4. Data

The initial budget for the MFA (US$336 million for operating costs) included the implementation of a follow-up to conduct a short-term impact evaluation of the program (Departamento Nacional de Planeación, 2000). Although the ideal conditions for impact evaluation require the random allocation of treatment between municipalities, this was not possible for ethical and political reasons. Instead, municipalities in the treatment group were those with less than 100 thousand inhabitants, different from the capital city of the department, with infrastructure to attend health and education demands, and with at least one bank. However, using stratified sampling, control municipalities, which were as similar as possible in population and quality of life to the treatment municipalities, were chosen.

The sample comprised 122 municipalities, 57 in the treatment group and 65 in the control group. In each municipality, 100 eligible households were randomly selected to be in the sample used to conduct the impact evaluation.

Based on the previous methodology, 11,462 households were interviewed in 2002 to create a baseline database (60% of households belonged to treatment municipalities). Subsequently, a first follow-up (FF) was conducted in 2003, where 10,742 households were surveyed (59% belonging to treatment municipalities). Two years after the baseline, a second follow-up (SF) was conducted including 9566 households (70% belonging to the treatment group).[5]

For this research, it is necessary to redefine the treatment associated with the MFA program. Here, the treatment group includes school-age children, not beneficiaries of the program, who belong to households that received the transfer for their siblings. The control group includes school-age children who live in municipalities where the program did not exist during the baseline and the SF.[6] The sample has 995 individuals in the control group (67.87%) and 471 individuals (32.13%) in the treatment group. The treated individuals are distributed in 29 treatment municipalities, while individuals in the control group belong to 49 municipalities where there was no MFA. Graph A1 in the appendix shows the distribution of treated and control municipalities in Colombia.

[2] Argentina, Bolivia, Brazil, Chile, Colombia, Costa Rica, Dominican Republic, Ecuador, El Salvador, Guatemala, Jamaica, Mexico, Nicaragua, Panama, Paraguay, Peru, Uruguay, Trinidad and Tobago.

[3] This theory stablishes that in order to push household outside poverty traps, it is not necessary to assign a transfer to each member, because of the presence of scale economies in family consumption. This is, the unitary cost of consumption is decreasing on the number of members. For instance, the per capita cost of cooking for three people is lower than the per capita cost of cooking for two people; going to the market to buy groceries for more people is cheaper than buying for less people, because wholesale prices are lower than retail prices, etc.

[4] The country had the highest unemployment rate ever experienced; the income of the poorest population decreased, and consequently, poverty considerably increased.

[5] Although there is the third follow-up, this research does not consider it because the information presents difficulties to match it with previous surveys.

[6] Before the application of the baseline surveys for the impact evaluation, the program had already started in some municipalities due to political pressures. To avoid potential problems in the estimations, these municipalities were removed from the analysis, following García et al. (2009).

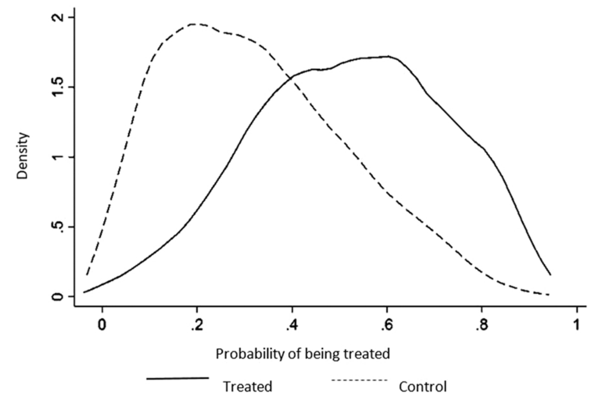
Table 1
Sample characteristics at baseline.

	Treatment		*Control*		*Difference*	
	Mean	N	Mean	N		
Individuals						
Currently attending	0.80	406	0.89	869	0.0204	***
School absence	6.66	402	4.02	867	0.6136	***
School lag	0.86	312	0.74	745	0.0279	***
The child works	0.36	311	0.13	513	0.0285	***
Hours worked yesterday	1.70	308	0.45	510	0.1692	***
Educational aspirations:						
Aspire to finish high school	0.56	398	0.39	868	0.0297	***
Aspire to finish college	0.23	398	0.54	868	0.0288	***
Child's age	10.59	471	9.74	995	0.1385	***
Gender of the child (1 =Male)	0.62	471	0.55	995	0.0276	**
Households						
Household head age	43.95	363	43.96	531	0.8085	
Gender of the household head	0.83	363	0.85	531	0.2505	
Wife's age (or husband)	38.69	307	38.55	454	0.7350	
Number of household members	7.64	363	6.13	531	0.1696	***
Number of children 7–11 years old	1.73	363	1.19	531	0.0619	***
Number of children 12–17 years old	1.52	363	1.14	531	0.0738	***
Household head Education: Secondary	0.08	324	0.14	473	0.0226	**
Dependency rate	0.59	363	0.22	531	0.0100	***
Monthly household income	243,621	361	261,722	529	21,927.6	
Monthly expenditure on public services	12,387	337	16,483	506	1,521.12	***
Characteristics of the house						
Rural municipality	0.39	363	0.60	531	0.0344	***
Predominant wall material						
Brick, stone	0.41	363	0.47	531	0.0338	*
Tread or adobe wall	0.09	363	0.15	531	0.0224	***
Adobe	0.4058	363	0.18	531	0.0302	***
Rough wood	0.07	363	0.15	531	0.0222	***
Public services						
Electric power	0.85	363	0.92	531	0.0209	***
Pipeline gas	0.05	363	0.07	531	0.0173	
Aqueduct	0.65	363	0.68	531	0.0332	
Sewerage	0.25	363	0.23	531	0.0302	
Municipality						
Students per teacher	23.65	29	23.94	49	1,2342	
Number of educational institutions	50.55	29	31.20	49	6.9867	***
Region						
Atlantic	0.34	29	0.22	49	0.1044	
Oriental	0.21	29	0.37	49	0.1080	
Central	0.34	29	0.33	49	0.1119	
Pacific	0.10	29	0.08	49	0.0678	
Height	831.62	29	1,279.14	49	213.64	**

Note. Results report number of children, household, and municipalities in treatment and control group.
*** $p < 0.01$
** $p < 0.05$
* $p < 0.10$.

5. Empirical strategy

As mentioned earlier, this paper assesses the indirect effects of MFA on the school enrollment and child labor of nonbeneficiary children whose siblings receive the transfer. In other words, the impact evaluation seeks to establish the difference between the outcome of nonbeneficiary children belonging to a family with beneficiary members and the outcome that they would have obtained if they had belonged to a nonbeneficiary family. Although the latter is only a theoretical concept, because there are no two situations in which the same individuals have been treated and another in which not, the situation of the control individuals can replace this counterfactual, as long as they are comparable to the individuals in the treatment group.



Graph 1. Common support, Propensity Score Matching.
Source: own elaboration.

For the comparability between the treatment and the control groups, Table 1 shows that, even before the implementation of MFA, there were differences between the educational outcomes for the control and the treatment groups. For example, 80% of children in the treated group attended an educational institution, while for the control group, this proportion was 9 percentage points higher. In addition, individuals in the treatment group had higher levels of absenteeism in school, worked more hours a week, and had greater educational lags.

Similarly, there are other preexisting differences in the sample related to educational aspirations, where, although a higher proportion of individuals in the treatment group wish to finish high school, this percentage is the same as that of children in the control group aspiring to enter and complete tertiary education.

We also found significant differences in the internal structure of the family. Individuals in the treatment group belong to larger families with higher dependency rates compared to children in the control group.

Finally, some important differences are related to the income or economic conditions of the households to which the children belong. Table 1 shows that individuals in the treatment group belong to families located in more dispersed areas of the municipality, with worse housing conditions and less coverage of public services.

Since there are preexisting differences in the program between the treatment and the control group, we must control by observable and unobservable variables. Many techniques ensure that individuals in the treatment group and control are comparable. Following Attanasio et al. (2010), the difference-in-differences method combined with the propensity score matching technique is implemented in this research.

To control for observable variables that generate differences between the treatment and the control groups, we estimate the probability for the individual to be classified as treatment or control. Eq. 1 estimates the probability of being a treated child ($T_i = 1$ if treated, zero otherwise), which depends on certain individual (I_i), household (H_i), dwelling (D_i), and municipal (C_i) characteristics.

$$T_i = \upsilon_0 + I_i\gamma + H_i\theta + D_i\phi + C_i\mu + \varepsilon_i \quad (1)$$

Once the probability to be in the treatment group has been predicted, treatment individuals are matched with the most comparable individual within the control group. Graph 1 shows the common support in the propensity score implementation. The common support region corresponds to the area between the minimum probability of being treated in the treatment group and the maximum probability in the control group. As observed, we find a satisfactory extent of the overlap.

Table 2
Probit Model to establish the determinants of being a non-beneficiary sibling.

	dF/dx	Std. E	
Aspirations to finish high school	-0104	0,0408	**
Aspirations to finish university	-0294	0,0381	***
Child of the household head	-0167	-0,0564	***
Child's age	0053	0,0069	***
Number of household members	0023	0,0067	***
Household head age	-0007	0,0015	***
Number of children 7–11 years old	0060	0,0192	***
Dependency rate	0282	0,1079	***
Adobe walls	0138	0,0350	***
Pipeline gas	0218	0,0780	***
Students per teacher-rural zone	-0026	0,0033	***
Students per teacher-urban zone	0028	0,0033	***
Number of educational institutions	0002	0,0005	***
Atlantic region	-0167	0,0303	***

* ** $p < 0.01$; * * $p < 0.05$; * $p < 0.10$.

control for preexisting differences in the program between the treatment and control, thereby ensuring an unbiased estimation of the program's effect on the variables of interest.

To control for the observable variables that generate differences between the treatment and the control groups, we estimate Eq. 1 using a probit binary dependent variable model. Table 2 indicates that the probability of being a child or young person treated varies depending on certain characteristics at the individual, household, and municipal level.

For individual characteristics, the results show that the probability that a school-age child belonging to a beneficiary household does not receive the subsidy decreases if education aspirations are higher, and when the child is a son of the household head. Conversely, the probability of being treated increases with the child's age. These results agree with the literature found, according to which the parents' decision not to include their child in the CCT program responds to the high opportunity cost that the child represents, as well as to parents' preferences toward certain members of the household.

Table 3
Total and heterogeneous effect of the treatment on schooling attendance.

Matching technique	Total sample		Women		Men		6–10 years		11–16 years	
	N = 1207		N = 501		N = 706		N = 587		N = 620	
1 nearest neighbor	-0298	***	-0256	***	-0308	***	-0188	***	-0340	***
5 nearest neighbors	-0313	***	-0275	***	-0336	***	-0211	***	-0341	***
10 nearest neighbors	-0313	***	-0256	***	-0361	***	-0208	***	-0370	***
Maximum distance (0001)	-0306	***	-0185	***	-0326	***	-0036	***	-0412	***
Kernel	-0310	***	-0274	***	-0359	***	-0222	***	-0373	***
Kernel-Bootstrapping	-0310	***	-0274	***	-0358	***	-0222	***	-0,37	***

* ** $p < 0.01$; * * $p < 0.05$; * $p < 0.10$. All estimated matching techniques consider a tolerance level of 20%. Dependent variable is equal to 1 if the child assists to class and 0 otherwise.

Considering that there might be unobservable differences between the treatment and control group, such as innate educational skills, the analysis is complemented with the difference-in-differences technique by estimating Eq. 2.

$$\Delta y_i = \beta_0 + \beta_1 T_i + v_i \tag{2}$$

Where Δy_{it} is the outcome variation between the baseline and the SF, the variable T_i is defined as before, and v is the error term. The coefficient of interest is β_1, which estimates the treatment effect on the outcome variation.

In summary, the estimated impact responds to comparing the variations in the outcome variables between individuals in the treatment and individuals in the control group, which are comparable when considering their observable characteristics.

6. Results

We test the impact of the program on four outcomes: (i) schooling attendance, a dummy equals 1 if the child attends an educational center, 0 otherwise; (ii) absenteeism days, the number of days that the child missed classes; (iii) child labor supply, a dummy equals 1 if the child worked during the week before the application of the survey[7] and (iv) domestic work, a dummy equals 1 if the child worked doing chores such as cooking or washing.

Using the information collected in the baseline and the SF by the Colombian government, this paper determines the impact of the "program" on the four mentioned outcomes (the reader must understand "program" as the condition of being a treatment child, i.e., being a nonbeneficiary sibling in a recipient household). It is necessary to

Likewise, there are household characteristics affecting the probability that an eligible child could be excluded from the program while his or her siblings are beneficiaries. We found that this probability decreases if the head of the household is older and increases with the number of household members (dependent or not). This latter result is in accordance with the theoretical framework that establishes the existence of competition for scarce resources within the household.

Finally, the model includes variables at the municipal level. We found that the probability of being a "treated" child is negatively related to the number of students per teacher in the rural area, while the relation is positive in the urban area. These results make sense with the conditionality of adequate educational infrastructure for operating the program.

Once the importance of each observable determinant of participation in the "program" has been established, different techniques are used to match the predicted probabilities between the treatment and the control groups (variations of the nearest neighbor techniques, maximum distance, and kernel).

Given that, some observations in the control group are less or more comparable than others in the treatment group, we estimate Eq. 2 once we have already assigned weights to the observations.

Table 3 shows the impact of an eligible child being excluded from the program on his or her schooling attendance. As observed, the impact is negative and significant when using all the matching techniques, that is, the proportion of children who attend an educational center decreases with the treatment.

Similarly, Table 3 presents evidence for the existence of heterogeneous effects by gender and age, that is, the estimated effect is greater for men than for women and is higher for older children compared to younger children.

In addition, Table 4 shows an increase in absenteeism days due to the "program." In short, being a treated child not only decreases the probability that the child attends an educational center but also increases absenteeism compared to children in the control group. As shown in Table 4, this effect is much greater for men and young people between

[7] A Child is considered as a worker if during the past week, they worked at least 1 h in exchange for some kind of payment; they worked with the family without payment; or if they do not work, but they had a job.

Table 4
Total and heterogeneous effect of the treatment on absenteeism.

Matching technique	Total sample		Women		Men		6–10 years		11–16 years	
	N = 1200		N = 496		N = 704		N = 584		N = 620	
1 nearest neighbor	8780	* **	7595	* **	11,258	* **	3955	* **	9663	* **
5 nearest neighbors	9488	* **	8834	* **	9980	* **	6198	* **	9958	* **
10 nearest neighbors	8994	* **	8815	* **	10,441	* **	5647	* **	10,711	* **
Maximum distance (0001)	8843	* **	8658	* **	10,630	* **	2210	* **	9046	* **
Kernel	9064	* **	7721	* **	10,218	* **	6059	* **	11,002	* **
Kernel-Bootstrapping	9064	* **	7721	* **	10,21	* **	6059	* **	11,002	* **

* ** $p < 0.01$; * * $p < 0.05$; * $p < 0.10$. All estimated matching techniques consider a tolerance level of 20%. The dependent variable is Absenteeism, which is a discrete variable accounting for the number of days that the children is missing school.

Table 5
Total and heterogeneous effect of the treatment on the probability of working.

Matching technique	Total sample		Women		Men		6–10 years		11–16 years	
	N = 371		N = 100		N = 271		N = 53		N = 317	
1 nearest neighbor	-0.098		0450	*	-0.079		-0380	*	-0090	
5 nearest neighbors	-0.097		0360	* *	-0152	*	-0095		-0077	
10 nearest neighbors	-0.076		0190		-0.180	* *	-0047		-0093	
Maximum distance (0001)	-0.181	*	0750	*	-0.217				0060	
Kernel	-0.090		0417	* *	-0180	* *	-0239		-0076	
Kernel-Bootstrapping	-0.090		0417		-0180	*	-0239		-0076	

* ** $p < 0.01$; * * $p < 0.05$; * $p < 0.10$. All estimated matching techniques consider a tolerance level of 20%. The dependent variable is equal to 1 if the child worked the last week and 0 otherwise. This variable was measured with the following question: last week, did you carry out any activity in exchange for money or did you work in a relative's business without being paid? Besides, we double-checked for working children by considering the question "what do you do when you are not studying" and the answer was "working more than an hour".

Table 6
Total and heterogeneous effects of the treatment on the probability to carry out domestic activities last week.

Matching technique	Total sample	Women	Men	6–10 years		11–16 years	
	N = 611	N = 270	N = 341	N = 100		N = 508	
1 nearest neighbor	-0054	-0026	0.1270	0428	* *	-0113	
5 nearest neighbors	-0083	-0021	-0.019	0364	*	-0121	*
10 nearest neighbors	-0052	-0025	-0032	0142		-0116	*
Maximum distance (0001)	-0032	0.033	-0.013			-0145	
Kernel	-0071	-0042	-0.035	0377	*	-0118	*
Kernel-Bootstrapping	-0071	-0042	-0035	0377		-0118	

* ** $p < 0.01$; * * $p < 0.05$; * $p < 0.10$. All estimated matching techniques consider a tolerance level of 20%. Dependent variable is equal to 1 if the child worked the last week doing domestic chores and 0 otherwise.

11 and 16 years of age.

For the impact of the "program" on the outcome variables related to child labor, Table 5 presents the estimation of the impact on the probability of working (the child or adolescent did or did not work the week before the day of the survey). It is important to be cautious in the interpretation of findings in this case because the sample size has considerably decreased due to the misreporting of child labor.

Regarding the results from the whole sample, the matching techniques show nonsignificant effects of the treatment on child labor outcomes. A potential explanation is that substitution and an income effect occur and offset each other: the income effect arises because the subsidy increases the monetary resources of the household, which makes it less necessary to send children to work. The substitution effect occurs because the opportunity cost of beneficiary siblings to attend school is lower compared to the opportunity costs of nonbeneficiary siblings, given that the beneficiary sibling attends classes to keep the subsidy.

However, when estimating the effects by gender, there is evidence of a heterogeneous effect: in contrast to men, women increase their probability of working given the "program". In short, being a nonbeneficiary female child not only decreases her class attendance but also increases her probability of working. This is an important aspect for policymakers that we discuss in the last section.

Taking into account the reduction of schooling attendance of nonbeneficiary siblings, as well as the no increment in their probability of working (except for female children), a natural question is what these children are doing with their free time. One possibility is that the treatment increases children's unpaid labor (domestic chores). Table 6 shows that this is only true for children aged 6–10 years, while the probability of treated children from 11 to 16 years to work on domestic chores decreases (there is a nonsignificant effect for men).

Considering the results obtained for children between 11 and 16 years of age, that is, a decrease in class attendance, no variations in the probability of working, and reduction of domestic work, it would be useful to analyze an outcome variable related to leisure or recreation activities (to check if those children are spending more time in these activities). However, this is not possible because the baseline survey includes information related to leisure and recreation time non-comparable with the information in the SF survey.

What do explain the results obtained for male children? This is, reduction on schooling attendance, increase in absenteeism, decrease in the probability of working, and nonsignificant effects on the probability to carry out domestic activities. We believe that, being domestic chores one type of work (in addition to paid work), from the theoretical point of view we can explain the result using the income and substitution effect argument. With respect to unpaid work, we conclude that the income effect and substitution effect offset each other, which explains the non-

Table 7
Total and heterogeneous effect of the treatment on schooling attendance.

Matching technique	Total sample		6–10 years		11–16 years	
	N = 1248		N = 748		N = 500	
1 nearest neighbor	0100	* **	0099	* *	0177	* *
5 nearest neighbors	0098	* **	0089	* **	0179	* **
10 nearest neighbors	0107	* **	0085	* **	0159	* **
Maximum distance (0001)	0131	* **	0066	* *	0180	* *
Kernel	0101	* **	0099	* **	0168	* **
Kernel-Bootstrapping	0101	* **	0099	* *	0168	* **

* ** $p < 0.01$; * * $p < 0.05$; * $p < 0.10$. All estimated matching techniques consider a tolerance level of 20%.

Table 8
Total and heterogeneous effect of the treatment on absenteeism.

Matching technique	Total sample		6–10 years		11–16 years	
	N = 1239		N = 743		N = 496	
1 nearest neighbor	-2994	* *	-1529		-4028	* *
5 nearest neighbors	-2920	* **	-3143	* **	-6228	* **
10 nearest neighbors	-3468	* **	-2648	* **	-4700	* **
Maximum distance (0001)	-3712	* **	-0921		-5168	* **
Kernel	-3226	* **	-3282	* **	-5229	* **
Kernel-Bootstrapping	-3226	* **	-3282	* *	-5229	* **

* ** $p < 0.01$; * * $p < 0.05$; * $p < 0.10$. All estimated matching techniques consider a tolerance level of 20%

Table 9
Total and heterogeneous effect of the treatment on the probability of working.

Matching technique	Total sample		6–10 years	11–16 years	
	N = 244		N = 42	N = 201	
1 nearest neighbor	-0,0188		-0.25	-0.243	*
5 nearest neighbors	-0,1660	*	-0.25	-0.195	*
10 nearest neighbors	-0,1773	*	-0.133	-0.226	*
Maximum distance (0001)	-0,0714			-0.214	
Kernel	-0,1715	*	-0.196	-0.227	*
Kernel-Bootstrapping	-0,1715		-0.196	-0.227	

* ** $p < 0.01$; * * $p < 0.05$; * $p < 0.10$. All estimated matching techniques consider a tolerance level of 20%.

Table 10
Total and heterogeneous effects of the treatment on the probability to carry out domestic activities last week.

Matching technique	Total sample		6–10 years		11–16 years
	N = 498		N = 113		N = 383
1 nearest neighbor	0.160	*	0.486	* *	-0.013
5 nearest neighbors	0.173	* *	0.470	* *	0.010
10 nearest neighbors	0.183	* *	0.332	* *	0.044
Maximum distance (0001)	0.063		0.500	* *	-0.105
Kernel	0.164	* *	0.520	* **	0.044
Kernel-Bootstrapping	0.164	*	0.520	* *	0.044

* ** $p < 0.01$; * * $p < 0.05$; * $p < 0.10$. All estimated matching techniques consider a tolerance level of 20%.

significance of the effect of CCT on the probability of doing domestic activities. Furthermore, when considering the probability of working, our results for male children suggest that the income effect is dominating.

7. Impact of CCT program on the siblings receiving the transfer

In this section, we replicate the results of Attanasio et al. (2010), using the same sample employed to obtain our estimations in Tables 3 to 6. This is, we check the effect of CCT on the outcomes of beneficiary siblings belonging to households where at least one child is nonbeneficiary.[8]

Table 11
Falsification Test.

Matching technique	N = 1168
1 nearest neighbor	-0027
5 nearest neighbors	-0004
10 nearest neighbors	-0007
Maximum distance (0001)	-0021
Kernel	0002
Kernel-Bootstrapping	0002

* ** $p < 0.01$; * * $p < 0.05$; * $p < 0.10$. All estimated matching techniques consider a tolerance level of 20%.

Table 12
"Leads and Lags" Model.

	Coef.	Std. Err.	
Treatment (T)	-0.678	0.1423	* **
I (2002)	0.181	0.1316	
I (2003)	-0.275	0.1224	* *
I (2004)	-1915	0.1267	* **
T * I (2002)	-0.214	0.1700	
T * I (2003)	0.151	0.1613	
T * I (2004)	1121	0.1558	* **
constant	1968	0.1027	* **
Observations:	5059		
N individuals	1306		

* ** $p < 0.01$; * * $p < 0.05$; * $p < 0.10$

Table 13
Proportion of households whose number of beneficiary children increased, decreased or remained the same.

	Treatment Individuals		
	Freq.	%	Accum%
Same	146	31.00%	31%
Higher in SF	129	27.39%	58%
Higher in FF	140	29.72%	88%
Total	471	100%	

56 missed observations

Schooling results were similar to those obtained by Attanasio and coauthors. Tables 7 and 8 show that school attendance increased and absenteeism decreased for recipient siblings, especially for older children.

This exercise is useful to deduce that households where non-beneficiaries are dropping out from schools given the program are also households where CCTs are trying to protect beneficiaries of school dropouts. [9] Furthermore, by comparing Table 3 with Table 7, we deduce that the reduction in the probability of schooling attendance of non-beneficiary children is higher than the increase of this probability for beneficiary children living in households where at least one child is non-beneficiary of the program. Thus, this finding gives strength to the paper's main policy implication: the inconvenience of limiting the benefits to a maximum number of children per household, leaving some siblings out of the program. We discuss this implication in the last section of the paper. Table 9.

[8] For comparison purposes with Attanasio et al. (2010), we do not need to distinguish the results by gender, because they only report heterogeneous effects by age group and area of residence.

[9] We thank an anonymous referee for suggesting the estimations of this section.

Table 14
Impact of the "program" on school attendance and absence from classes.

Matching technique	*School attendance*				*Absence to classes*			
	(1)		**(2)**		**(1)**		**(2)**	
	N = 1207		N = 1076		N = 1200		N = 1069	
1 nearest neighbor	-0.298	* **	-0.286	* **	8780	* **	8134	* **
5 nearest neighbors	-0.313	* **	-0250	* **	9488	* **	7351	* **
10 nearest neighbors	-0.313	* **	-0.255	* **	8994	* **	7442	* **
Maximum distance (0001)	-0.306	* **	-0.272	* **	8843	* **	7926	* **
Kernel	-0.310	* **	-0.244	* **	9064	* **	7005	* **
Kernel-Bootstrapping	-0.310	* **	-0.244	* **	9064	* **	7005	* **

* ** $p < 0.01$; * * $p < 0.05$; * $p < 0.10$. Column (1) refers to the estimates for the total sample, while column (2) presents the results for the individuals who were treated in the PS and SS. All estimated matching techniques consider a tolerance level of 20%.

Table 15
Impact of the "program" on school attendance and absence from classes.

Matching technique	*School attendance*				*Absenteeism*			
	(1)		**(2)**		**(1)**		**(2)**	
	N = 1207		N = 1152		N = 1200		N = 1145	
1 nearest neighbor	-0.298	* **	-0.284	* **	8780	* **	12.73	* **
5 nearest neighbors	-0.313	* **	-0340	* **	9488	* **	10.22	* **
10 nearest neighbors	-0.313	* **	-0.356	* **	8994	* **	10.34	* **
Maximum distance (0001)	-0.306	* **	-0.355	* **	8843	* **	9.166	* **
Kernel	-0.310	* **	-0.350	* **	9064	* **	10.09	* **
Kernel-Bootstrapping	-0.310	* **	-0.350	* **	9064	* **	10.09	* **

* ** $p < 0.01$; * * $p < 0.05$; * $p < 0.10$. Column (1) refers to the estimates for the total sample, while column (2) presents the results for the individuals who were not beneficiaries for at least one exogenous reason. All estimated matching techniques consider a tolerance level of 20%.

Regarding the probability of working and domestic work, the signs of the results are similar to those from Attanasio et al. (2010) if we compare only the last column corresponding to older children (the authors do not work with children from 6 to 10). However, the significance differs. In our case, beneficiary older children reduce their probability of working while there is no effect on the probability of doing domestic chores.

Another interesting issue arose from this exercise related to younger beneficiary children. They are increasing school attendance, reducing absenteeism, and increasing the probability of spending time on domestic chores. This suggests that they are probably sacrificing leisure time, but as we mentioned before, due to the lack of information, we are not able to prove this hypothesis quantitatively.Table 10.

8. Robustness checks

8.1. Falsification

Considering that the use of the matching technique requires fulfilling the conditional independence assumption, in the following, we evaluate whether the identification strategy is valid or if there is evidence for the existence of other factors, not incorporated in the model, that determine the treatment allocation.

Following Bernal and Peña (2011), one of the existing falsification tests, which seeks to estimate the impact of the "program" on fictitious dummy variables, is performed. These dummy variables will be outcomes in periods before the implementation of MFA. The available information allows us to conduct the falsification test only for the schooling attendance outcome (using data from 2001 and 2002), not for child labor.

Table 11 shows the treatment has no impact on the fictitious outcome (variation in schooling attendance between 2001 and 2002). This supports the internal validity of our model when determining the impact of the "program" on school attendance.

8.2. Evaluating the assumption of parallel trends

The difference-in-differences model requires compliance with the parallel trend assumption for its internal validity. That is, if the treatment had not existed, the difference between the proportion of children attending school or working between the treatment and control groups should remain constant over time. However, given the impossibility of testing this assumption because it responds to a theoretical construct, to give some reliability to our results, we use the "leads and lags" model.

Having information on schooling attendance for the years 2001 and 2002 (periods before the program), as well as for the periods 2003 and 2004 (years after the program), we estimate Eq. (3).

$$Y_{it} = \beta_0 + \beta_1 T_i + \sum_{t=2001}^{2004} \beta_{2,t} I(year = t)_t + \sum_{t=2001}^{2004} \beta_{3,t} T_i * I(year = t)_t + \varepsilon_i \quad (3)$$

Where Y_{it} is a dichotomous variable that takes the value of 1 if the individual i attends school in the year t; the variable T_i is equal to 1 for individuals in the treatment group and 0 for the control group; $I(year = t)_t$ are dichotomous variables of time.

The coefficients of interest are $\beta_{3,2001}$ and $\beta_{3,2002}$ which estimate the existing average differences between the treatment and control groups before treatment. If the coefficients are not significant, that would be the evidence of the compliance of the parallel trend assumption before the program's implementation. Table 12 shows the results of the "leads and lags" model. The dichotomous variable I(2001) has been omitted to avoid the dummy variable trap. As expected, $\beta_{3,2002}$ is not significant, which supports the compliance of the parallel trend assumption before treatment.

8.3. Evaluating possible randomization biases

The available information does not allow us to assure that a non-beneficiary sibling (a treated child according to our model) has never been a beneficiary. This child could have been a beneficiary in the past and lost the subsidy because he or she did not comply with the required conditions (e.g., below 80% of the attendance or expulsion due to low academic performance). If this were the case for many children in our treatment group, we would face a potential bias in the estimates of the impact of the program (lack of randomization).

To deal with this potential problem, we use information from the FF and the SF collected by the government to evaluate MFA (recall that we are using the SF to estimate our models). Specifically, we use the variation of the children subscribed to MFA between SF and FF as a proxy of the continuity of the treated children in the program. As the FF has information on the number of beneficiary children but does not distinguish between beneficiary and nonbeneficiary children inside the household, we count the total number of subsidized children in the FF and SF. Finally, we identify households whose number of subsidized children had increased, decreased, or remained equal from the FF to SF. Table 13 shows that approximately 60% of beneficiary households who had at least one eligible child out of the program in the FF continue in this same position or have increased the number of nonbeneficiary eligible children in the SF.

Next, we estimate the effect of the treatment on schooling attendance and absenteeism, restricting the sample to households whose number of education subsidies in the SF were equal or greater than in the FF. As shown in Table 14, although the impact of the "treatment" decreases, the effect continues to be negative and significant, giving robustness to the results previously found.

Finally, we also estimate the effect of the treatment on schooling attendance and absenteeism using an exogenous source of treatment variation considering a possible identification problem, if it were the case that families deliberately decide who to send to school and, consequently, which child benefits from the subsidy and which one does not.[10] Specifically, we restrict the sample to households where the children were no beneficiaries because they did not have the completed required documents. We excluded the children that were not in the program because of reasons that potentially may reflect carelessness from the parent, which is not the case of those children in our restricted sample, since the parent made the effort of enrolling the child but did not succeed. Table 15 shows an even higher impact of the treatment for this restricted sample, being the effect on school attendance also negative and significant.

9. Discussion

Within the framework of neoclassical economic theory, CCT programs can help reduce the opportunity costs faced by low-income families when deciding whether to send their children to school. In addition, the conditionality of these transfers (schooling attendance) has the purpose of modifying households' behavior, since, as argued from experimental economy, households have certain misguided beliefs that lead them to under-invest in human capital (Cookson, 2017).

In this sense, the MFA program comes in the form of CCTs, which promote and encourage human capital accumulation by limiting the margin of decisions that family members can make regarding children's school attendance. However, since programs also restrict the parents' decision set by limiting the number of beneficiaries per household, some negative effects may arise for non-beneficiary siblings.

Specifically, considering the limitation on the number of beneficiary children, parents with numerous offspring can weigh the different opportunity costs of those children with the subsidy and those who were excluded from the program. This research presents evidence along this line since the results show adverse impacts of MFA on the schooling attendance and absenteeism of nonbeneficiary eligible siblings. Results hold even controlling by educational aspirations and economic conditions.

Our estimations show that households where non-beneficiaries are dropping out from school, are also households where beneficiaries are protected from dropping out of school. Furthermore, the reduction in the probability of schooling attendance of non-beneficiary children is higher than the increase of this probability for beneficiary children living in households where at least one child is non-beneficiary of the program. The previous evidence gives support to an important policy implication related to the inconvenience of limiting the subsidies to a maximum number of children per household, leaving some siblings out of the program. Universal coverage is an appropriate approach to avoid negative spillovers of the CCTs.

Another relevant aspect is related to the heterogeneous findings by gender. Although for male children the negative effect on schooling attendance is higher than for women, their probability of working does not increase, which is the case for nonbeneficiary female children. This negative spillover on women is unfortunate because it may expand the already existing gender gap in the labor market. This is another reason to call for the universality of the subsidy within the household.

Nowadays, several CCT programs in Latin America are limiting the beneficiaries per household (see appendix). Considering our results, governments should weigh the advantages and disadvantages of establishing a limit on the number of beneficiary children per household. Even if these CCT programs may prevent the potential encouragement of fertility compared with a non-limited program or take advantage of scale economies in consumption (see footnote 3), it might also encourage a displacement effect between the beneficiaries and nonbeneficiary children in the household.

Related to the potential increase in fertility as a reason to limit the number of beneficiaries, Soto and Ortigoza (2012) found that the MFA program does not increase women's fertility in Colombia. In any case, even if the program had some effect in the desired number of children, the solution should not be to restrict the number of beneficiaries, given the negative spillovers that the CCT have on nonbeneficiary siblings. Governments must design alternative policies to control fertility considering that public policies are not implemented on static scenarios, rather on individuals or households whose decisions may be affected by the policy. Following a non-unitary model framework, even if the policymaker limits the program in an attempt to maximize the well-being of the household, it would not be the same as maximizing the well-being of non-beneficiary children. Along these lines, our results show the need to develop social projects considering synergies and behavioral changes produced by the program. This is a relevant topic for a future research agenda.

An interesting exercise would be to figure out whether the families in which non-beneficiaries get lower outcomes are the same as those where beneficiaries get better outcomes. For this, an anonymous referee (to whom we are grateful) suggested us to make the heterogeneity analysis for non-beneficiaries according to age and gender of beneficiaries in the same household, in order to obtain stronger arguments regarding the theory of the family we are analyzing. Although family economics is the framework theory to explain the households' behavior behind the negative effects founded in our research, we have to recognize that our methodology is not appropriate to give insights about the reasons why beneficiary households are compromising the schooling outcomes of their non-beneficiary children. For instance, nonbeneficiary women and children aged 6–10 years old reduced their school attendance and increased their probability of working on paid or unpaid activities, which might indicate that the displacement effect is dominating. As multiple factors might generate those results, we make clear that our contribution is to identify the negative effects on non-beneficiary siblings rather than figuring out the explicit cause of those effects.

Theoretically, parents may put some children in disadvantage compared to others due to economic, cultural or social reasons; however, it is difficult to figure out the predominant reason. The difficulty arises, among other factors, because a household can have up to eight beneficiaries with different individual characteristics, then, when restricting our sample considering each of these different characteristics, the possibility to generalize our results (given the sample size) is reduced. This a relevant issue and another potential topic for a future research agenda, when better databases are available. Nonetheless, the estimations we made considering this demographic composition of the

[10] We thank an anonymous referee for suggesting this additional robustness check.

household are available upon request.

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CRediT authorship contribution statement

Karen Camilo: Conceptualization; Data curation; Formal analysis; Investigation; Methodology; Writing – original draft; Writing – review & editing., **Blanca Zuluaga**: Conceptualization; Formal analysis; Funding acquisition; Investigation; Methodology; Project administration; Resources; Supervision; Validation; Writing – review & editing.

Conflicts of Interest

The authors declare that they have no potential conflict of interest.

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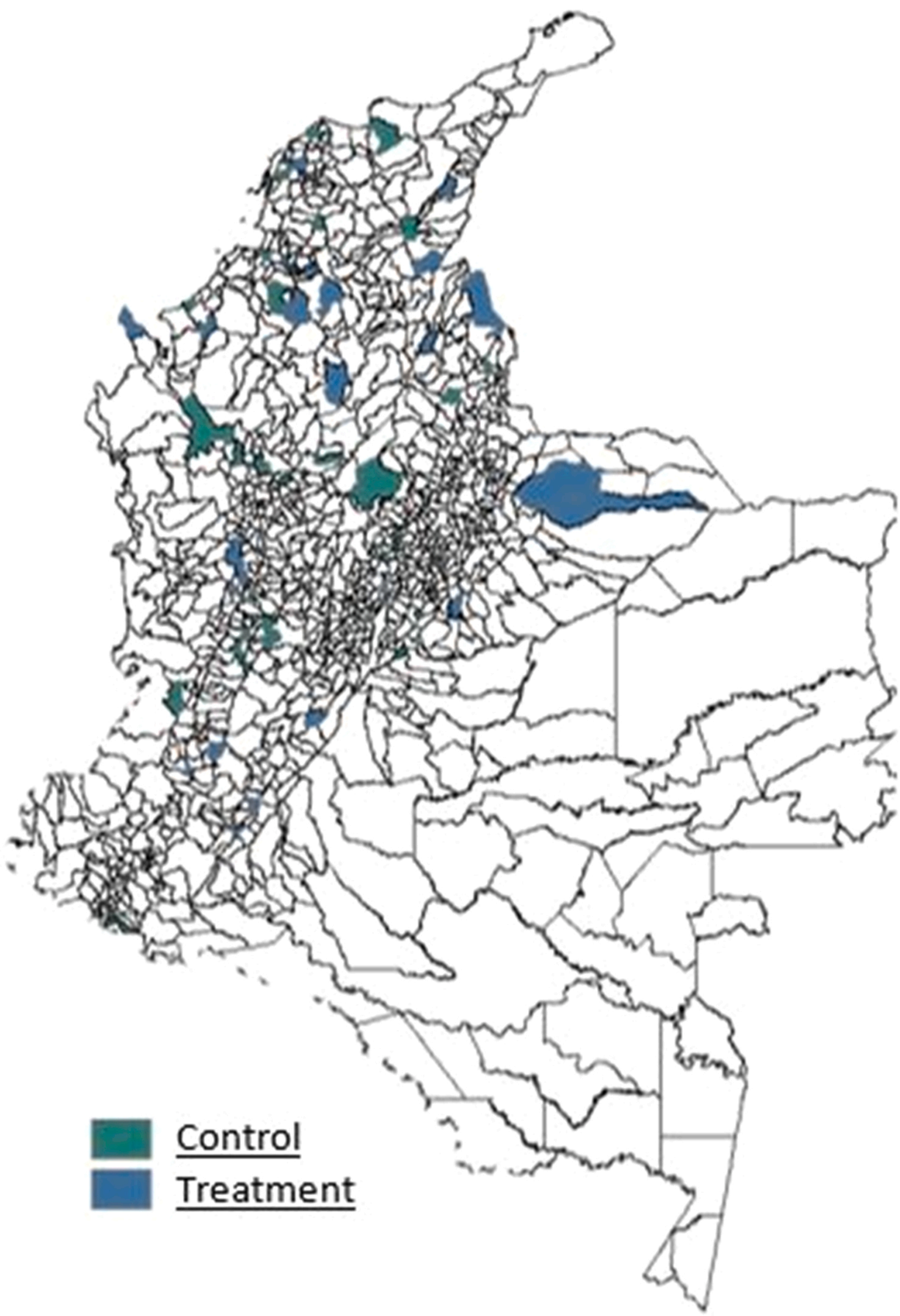
Appendix

See Appendix Fig A1.
See Appendex Table A1.



Graph A1. Treatment and control municipalities.

Table A1
CCTs in developing countries.

Country/Program	Start Year	End Year	Beneficiary's responsibilities	Children limit
Argentina/ Asignación Universal por Hijo para Protección Social	2009	Current	School attendance for children from 5 to 18 years old. Complete vaccination schedule for children under 5 years old.	Before 2020, there were a maximum of 5 beneficiary children per household.
Belize/Creando oportunidades para nuestra transformación social	2011	Current	85% school attendance for children under 18 years of age. Complete vaccination schedule for children under 5 years old and attendance at prenatal checkups for pregnant women.	Six beneficiary children per home.
Brazil/Bolsa Escola	2001	2003	85% school attendance	Three beneficiary children per household.
Brazil/Bolsa Familia	2003	Current	85% school attendance for children under 18 years of age. Complete vaccination schedule for children under 5 years old.	Five beneficiaries per home (6–15 aged children, and two beneficiaries 16–17 aged children).
Colombia/ Familias en Acción	2001	Current	80% school attendance for children under 17 years old.Children cannot lose (fail) more than two schooling years.	Three children per household. This limit started after the year we are analyzing in this paper.
Guatemala/ Protección y Desarrollo de la Niñez y Adolescencia Trabajadora	2007	2008	80% school attendance of 80%. Good school performance.	Three beneficiary children per household.
Haiti/Ti Manman Cheri	2012	Current	School enrollment and attendance.	Three transfer per household
Honduras/PRAF	1998	2005	School attendance (no more than 20 missing schooling days). Children cannot fail more than one schooling year.	Three children per household
Nicaragua/Red de Protección Social	2000	2006	95% school attendance.	One transfer per family.
Paraguay/ Tekoporâ	2005	Current	85% school attendance. Attendance to health care controls and vaccination dates.	Four beneficiaries per household
Dominican Republic/ Progresando con Solidaridad	2012	Current	80% school attendance.	Four beneficiaries per household
Uruguay/ Asignaciones Familiares - Plan Equidad	2008	Current	School enrollment and attendance.	Seven children per household.

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