

Impact of Education Subsidies on Teenage Childbearing: Evidence from Bogotá, Colombia¹

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Abstract

We investigate the causal impact of two Education Conditional Cash Transfers (CCT) programs, *Familias en Acción* and *Subsidio Educativo*, on teenage childbearing. Using a new database of schoolgirls in Bogotá (Colombia), we show that, after controlling for other factors, imposing a performance requirement is key for the *Subsidio Educativo* program to provide students enough incentives to reduce teenage childbearing. The proportion of schoolgirls with children enrolled in schools in which the *Subsidio Educativo* program is highly implemented is more than halved with respect to their counterfactual (2.39% instead of 6.21%). The CCT that only requires school attendance, *Familias en Acción*, has no effect on teenage childbearing.

JEL classification: D12, I28, I38, J13

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1 Introduction

Teenage childbearing is considered to be one of the possible adverse events with long lasting consequences that can happen in lives of girls in developed and under developed countries. The literature has documented the relation between being a teen mother and low levels of human capital, low wages, low employment rates. The literature has also found a relation between teen age motherhood and disadvantages of their children in health, education or even becoming a teen mother. This evidence suggest that it may be important to implement policies that may help reducing teenage childbearing. In this paper we use a new data base to study the impact that two different Education Conditional Cash Transfers (CCT) implemented in Bogotá (Colombia) have had on teenage pregnancy rates. Education policies may affect teenage pregnancy if, first, they change the costs and benefits of education and, second, if becoming a mother at teen age competes with investments in human capital.

Admittedly most studies that look at the consequences of teenage childbearing do not isolate causal effects. The economics literature has recently started to discuss to what extent there are long term effects of teenage childbearing. The evidence is mixed as some have found that the correlations masks relations between outcomes such as schooling or employment with other underlying variables (Geronimus and Korenman 1992 and 1993, Geronimus 1993, Hotz, Mulin and Sanders 1997, Hotz MacElroy and Sanders 1999) but others find that the causal effects are smaller than the correlations found in other studies but they are still significant and important (Hoffman et al. 1993, Angrist and Evans 1996, Levine and Painter 2000, and Navarro and Walker). As long as there is no agreement about the consequences of teenage childbearing, the study of its determinants and in particular of the policies that may be used to reduce teenage childbearing is still relevant.

Moreover, to the best of our knowledge there are no studies about the consequences of teenage childbearing in developing countries and there are important reasons to believe that they may be different from those in developed countries. In particular, developed countries have many programs to assist teenage mothers, massive public education systems or other programs that reduce the effects of credit market imperfections on the education system. This can result in low or no effect of teenage childbearing effects on schooling, employment of the mothers or even in health or schooling of the children. In developing countries this type of programs are much more scarce so the consequences of teenage pregnancy may be more important.

The main objective of the paper is to evaluate the effect of two conditional cash transfers (CCT) in education in Bogotá , Colombia on teenage childbearing.¹ The main mechanism behind

¹The main trends in teenage pregnancy in Latin America are summarized in Flórez and Núñez (2001), Flórez and Soto (2007) and Flórez and Soto, (2008). For the case of Colombia these trends are found in Flórez (2005),

our study assumes that the event of becoming a mother by adolescent girls is not entirely a result of lack of family planning methods knowledge or availability and that in many cases becoming a mother at teenage may be a conscious decision. Instead adolescent girls see the possibility of becoming a mother as one of several alternative lifetime investment projects and take a decision consciously about which of the projects to assume. In our framework the alternative projects differ on the degree and timing of different decisions related to having a family and human capital investments.

The two CCT programs implemented in Bogotá (Colombia) are *Familias en Acción* and *Subsidio Educativo*. They differ in two aspects: the criteria of assignment and the conditions for granting the CCT. Regarding the criteria of assignment, *Familias en Acción* goes to the poorest households (Sisben level 1) and *Subsidio Educativo* goes to the not so poor households (Sisben level 2).² Regarding the conditions for granting the cash, both programs ask teenagers to attend school as a condition to receive the money. The programs differ in that the renewal of the *Subsidio Educativo* also depends on school performance. The *Familias en Acción* program does not include performance as a condition for renewal. Renewal occurs once in both programs, so that any granted student last two years in either program at most. Both programs are quite similar with respect to other aspects (management, conditions verification, etc.).

We show that the *Subsidio Educativo* reduces teenage pregnancy rates while *Familias en Acción* has no effect on pregnancy rates. Given that both programs are quite similar and that in our regressions we control for socioeconomic background, we can interpret this result as saying that performance requirements are crucial if one wants CCT programs to affect teenage pregnancy rates. Even though both programs may provide teenagers incentives to attend school, the *subsidio educativo* program provides schoolgirls additional incentives to reduce the likelihood of behavior that may end in childbearing.

The study proposes an empirical strategy that allows the use of differences-in-differences methods to identify the effect of policies. Similarly to Duflo (2001), the identification strategy relies on intensity differences across schools in the CCT's implementation to define the treatment and the control group (high intensity and low intensity schools). We undertook a field work to construct a database of girls that are attending school in 2010. We ask several variables at the individual, household, school and neighborhood level. Among the household data we ask

Flórez et. al. (2004) and Barrera and Jaramillo (2004). These authors show the existence of high levels of teenage pregnancy rates (in some countries these levels are even increasing), a negative relation between teenage childbearing and human capital of mothers and strong socioeconomic effects on teenage childbearing. However, none of this studies have identified a causal relation.

²Sisben is the name of the Welfare program in the country. It is based on a socioeconomic index, that receives the same name, that is used to sort households into five Sisben levels. Households in Levels 1 and 2 are usually targets of different welfare programs.

for information of older sisters that are attending (or attended) the same school. We then compare two cohorts of (older and younger) sisters. Our main result show that the proportion of schoolgirls with children that attended SED high intensity schools is halved with respect to the counterfactual. The proportion of schoolgirls with children that attended FAM high intensity schools has no difference with respect to the counterfactual.

Among the literature in which estimates of the causal effect of policy interventions on teenage pregnancy the two most related to our paper are Duflo et al. (2006) and Baird et al. (2009) since they evaluate the effect of education subsidy programs in developing countries (Kenya and Malawi).³ We differ from those contributions at least in three respects: first, we do not have randomized allocation of treatment. Instead we collect data from all *localidades* in Bogotá using a stratified random sampling that allow us to compare two cohorts in treated and non treated schools, namely, a younger cohort that is attending school and their sisters (older cohort) that are not attending school. Second, we are able to compare two CCT strategies (both of 15 USD per month) that differ mainly in one aspect: one asks pupils to pass the academic year in order to keep the CCT one year more, the other one does not do so – it grants CCT one year more regardless of pupils’ performance. Although the previous literature has found that CCT policies are useful for increasing school attendance and reducing risk sexual behavior and teenage childbearing, the evidence about what characteristic(s) of CCT policies play a crucial role in doing so is quite scarce. Our main finding is that a CCT program is useful for reducing teenage childbearing when it has a performance requirement. Requiring school attendance is not enough to reduce childbearing. Moreover, we find that school fees reduction, another subsidy program implemented in Bogotá, is not useful to reduce teenage childbearing.

This paper is composed by six sections. The first section is this introduction. Section 2 makes a detailed description of the education subsidies implemented in Bogotá. Section 3 presents the empirical strategy that will allow us to identify causal effects. Section 4 presents the data and relevant descriptive statistics. Section 5 presents results and robustness checks. Section 6 concludes.

2 Education policies to reduce individual costs of education in Bogotá

Bogotá is an interesting city to study the impact of educational policies that reduce the costs of human capital accumulation faced by individuals. The city has made a large effort to implement education policies that reduce this costs with the objective of reducing school attendance barriers.

³Other two studies with similar objectives but for England and the United States are Girma and Paton 2006) and Wolfe, Wilson and Haveman 2001

In this section we describe the two CCTs, *Familias en Acción* y *Subsidio Educativo*, that are currently implemented in Bogotá.

Subsidio Educativo (SED) was introduced in 2006⁴ targeting the poorest population of the city. The program had 45.000 beneficiaries, randomly chosen from 136.000 applicants, aged 18 years or less and enrolled in the official education system between 6th to 11th grades. In 2008, both local and national government started to coordinate a larger program of conditional subsidies founded by the National Government, but the program was in place at the second half of the year and the SED did not awarded new conditional cash subsidies along this year.⁵ In 2009, the national subsidies were already in place and the SED choose to reduce the number of its subsidies and to change the way they were allocated. In 2009 and 2010 the requirements were the same as before,⁶ but the allocation was based on merits. In coordination with national government, the SED established the number of beneficiaries according to its budget (12.000 subsidies) and school boards selected beneficiaries from eligibles based on academic achievement and other non-academic skills like sports performance and commitment with institution.

Subsidio Educativo lasts for two years and gives around 30.000 Colombian pesos per month (15 USD per month). Payments are done each two months during the 10 months of the schooling year. The subsidy is implemented with a debit card that can be used in the cash points of the bank in charge of the operation. The debit card is given to the mother when the student is aged less than 16 years and directly to the student if she has more than 16 years. The attendance verification is based on a daily report by schools to the SED. A maximum of eight justified absences each two months are allowed and no unjustified absences are allowed. If the student does not satisfy the requirements she or he will lose both the payment on the period of absences and the subsidy for the next year. Moreover, *subsidio educativo* asks for a minimum academic requirement in the sense that if the student fails the academic year, she/he will loose the subsidy for the next academic year.

In addition, *subsidio educativo* has a second type of subsidy covering transportation costs of students living far away of the school. It gives 2.600 Colombian pesos for each day the student attended school. Potential beneficiaries are students less than 19 year old enrolled

⁴In 2005, the SED implemented a pilot project in two regions of the city. The local government awarded 10.000 subsidies to evaluate the effects on school attendance, permanence and performance. Based on a positive evaluation of the program, the SED increased in 2006 the number of beneficiaries and expanded it to the whole city Barrera et al. (2008).

⁵In 2007, the local government keeps the number 45.000 subsidies, but any new beneficiary came from the 9 regions with largest enrollment demand (San-Cristobal, Usme, Bosa, Kennedy, Engativa, Suba, Rafael Uribe, Ciudad Bolivar and Tunjuelito) instead of the 20 regions as was established the year before. It did not represent a large change on the target population given that the 90% of the poorest population is located on these 9 regions.

⁶The only change was that the candidates should come from the SISBEN 2 given that the national government should cover the population on sisben 1.

in public schools at the grades 8th to 11th that live 2Km or more away from the school⁷. There is a transfer reduction of the day value (2.600 pesos) per day unattended. The subsidy is given until the end of high school or if some attendance requirement is not satisfied. The attendance verification is based on the a daily report made by schools. Students should satisfy the same requirements of the first version of *subsidio educativo*, including the minimum academic achievement. The payment is anticipated, the student received two months ahead the money through the same debit card system. Currently, there are around 14.000 subsidies awarded by the SED. It is important to say that both versions are substitutes between them and the amount is in average the same⁸.

The second type CCT currently implemented in Bogotá is *Familias en Acción*, a national social program started 2002, which has been extended to big cities and the capital since the second half of 2008. Currently, Familias en Acción targets 120.000 pupils each year enrolled in the public schools and identified as the poorest population (sisben 1) in Bogotá. Families in the program are given subsidies conditional upon school attendance of older children (7 and above) and on nutrition and health check-ups for younger ones. The education component of the program is similar to *subsidio educativo*, but with relevant differences in the requirements to keep the subsidy. *Familias en Acción* subsidy is granted until the end of the high school and is lost before if some of the attendance requirement are not satisfied. The city government is in charge of the verification of attendance, which is provided by the daily report of the schools. The absences requirements should be accomplished to keep the grant. However, there is not a requirement of academic achievement to keep her or his subsidy.

3 Empirical strategy

As we saw in the previous section there are two CCT programs in education in Bogotá. These CCTs define two different treatments, denoted T_1 and T_2 . Since we have cross-section data we do not have information before and after the treatment for the same individuals. Instead, in the same vein of Duflo (2001), we define two cohorts: the interviewed girls and their older sisters that studied in the same school. This strategy allows us to control for school and household unobservables that do not vary across time. Let C_i be the cohort. The treatment variables are

⁷This subsidy is awarded independent of the SISBEN status.

⁸The SED has implemented a third subsidy to pay enrollment fees in private schools. This modality is implemented through contract between the SED and the private schools to enroll children that could not be enrolled in public schools because of the lack of slots. The SED pays the tuition fees and the complementary payments (transport, materials, uniforms, etc.) must be afforded by the household and cannot exceed some ceilings. The contract lasts for one year. Because this subsidy is not a CCT program we control for it in our empirical strategy.

defined at the school level. We consider a treatment given by a dummy variable, so that we split schools in two groups: schools with high treatment and schools with low treatment. The indicator variable for treatment t , $t = 1, 2$ in school j is defined as follows:

$$T_{tj} = \mathbb{1}(prop_{tj} > E_j[prop_{tj}]),$$

where $prop_{tj}$ denotes the proportion of girls in school receiving the treatment t and $E_j[prop_{tj}]$ the average proportion of girls receiving the subsidy over schools.

We use the difference in difference strategy to identify the causal effect of education policies on teenage childbearing. Since we have two treatments, to fully identify the causal effect of those treatments we propose the following Equation to be estimated:

$$\begin{aligned} Y_{ij} = & \alpha_0 + \alpha_1 T_{1j} + \alpha_2 T_{2j} + \alpha_3 C_i + & (1) \\ & \theta_1 T_{1j} C_i + \theta_2 T_{2j} C_i + \\ & \beta_1 T_{1j} T_{2j} + \gamma_1 T_{1j} T_{2j} C_i + \\ & X_{ij} \eta + Z_j \vartheta + \varepsilon_{ij} \end{aligned}$$

where Y_{ij} is a dummy variable that takes value 1 when girl i in school j has had at least one child during teenage (between 14 and 19 years old), and value 0 otherwise. The cohort of individual i is given by C_i , which takes value 1 if the girl is in the cohort at school (14 to 19 years old) and value 0 if the cohort is out of school (19 to 32 years old). T_{1j} identifies whether school j has received high treatment of conditional cash transfers from Familias en Acción n (*FAM*) and T_{2j} identifies whether school j has received high treatment of conditional cash transfers from SED (*SED*).

The causal effect of each treatment will be identified by the estimation of coefficients θ_t , $t = 1, 2$. The causal effect of the interaction between any pair of treatments above and beyond the causal effect of each treatment will be identified by the estimation of coefficients γ_1 . X_{ij} is a vector of individual characteristics and Z_j is a vector of school characteristics, including fixed effects by *localidad*.

As usual, there are several challenges to credibly identify causal effect by using this approach. The key identifying assumption behind the DD approach is that selection biases are linear and time-invariant. In other words, if there is any difference in the pre-treatment period, this difference should be stable across time. In order to test this assumption we run a placebo regression. In this regression we only include data from sisters of our interviewed students. We split sisters in two groups by age. Younger sisters are included in one cohort and older sisters in the other one. There must be no effect of treatments across sisters.

Another challenge to identification comes from the definition of cohorts. The older cohort must be previous to the treatment in order to be useful to identify the causal effect. However, the younger sisters included in the older cohort are 19 years old. Indeed, since the subsidy programs started in 2005, all sisters between 19 and 24 years old are likely to be treated. The younger the more likely. However, we cannot exclude those sisters from regressions because most of them belong to that age range, so we will severely deteriorate precision in our estimates by excluding all sisters with 19-24 years old. If any, including young sisters will bias estimation of coefficients θ downwards. Thus, our estimates are actually a lower bound of the true estimator. To tackle this issue, we run the same regressions excluding sisters that are 19 and 20 years old.

Moreover, we define the treatment dummies based on an ad-hoc threshold, namely the mean proportion of subsidies across schools. To test whether our findings are driven by the threshold we run the same regressions by using different percentiles. Our results must be robust to use different thresholds. Furthermore, we run our regression by using proportion of subsidies at school instead of dummies as treatment variables. This allows us to have continuous treatment variables and results that do not depend on ad-hoc thresholds.

We further check whether the presence of other treatments may affect the results. In particular we further restrict the sample to individuals from schools with high treatment in one of our policies and individuals from schools with low treatment in all our policies. Finally, we check robustness of standard errors of estimates. All regressions are performed clustering residuals at school level. To check robustness of standard residuals we bootstrap errors while clustering at the school level. We also bootstrap the t-statistic of the estimated coefficients.

A final caveat is in order. Our approach sub estimates the likelihood of childbearing of the younger cohort. The reason is that, unlike the older cohort, not all individuals in the younger cohort have finished their teenage years. We tackle this problem by controlling by individual age.

4 Data and descriptive statistics

The empirical analysis uses information from an original survey of 21,287 girls enrolled in 273 schools in Bogotá. The survey interviewed girls between 14th to 19th years old enrolled in both public and private schools at the 9th, 10th and 11th grades. The sample has been selected with a probability proportional to population size estimated from the administrative records of Bogotá on girls' total enrollment at 9th to 11th grades for the year 2009 so that the survey is representative at the regional (localidades) level.

The survey collected information on mother and sisters' fertility and education, socioeconomic information, subsidies incidence, information about teenage childbearing and fertility

and the knowledge of family planning education. In this section we discuss the information that is used to evaluate the effect of education policies on teenage childbearing. Finally, it is important to say that the survey was designed to mix the school identifiers where girls study with administrative records on school facilities and school quality. In addition, some information at the level of the "localidades" is merged like crime incidence as a measure of the safety at neighbor.

In order to evaluate the impact of conditional cash transfers on childbearing, we take advantage of the information collected about older sisters of the interviewed girls. This information allows us to identify sisters childbearing, age, age at the first child was born, and if the sister studied in the same school the last year attended. From the 21.287 interviewed girls we were available to recover information on childbearing, age and age at the first child for 3.598 sisters. As was stated in the empirical strategy section we use the sister's information to build an older-sister cohort which could be comparable with the interviewed girls, but who were less likely to be affected by any cash transfer in Bogotá. The older sister cohort is composed by sisters that comply all the following conditions: 1. 19 to 32 years old; 2. studied in the same school as their interviewed sisters; 3. were teenage in the period between 1997 to 2005; 4. did not dropout from high school. At the end of this process we finished with a sample of 1.251 sisters and 1.151 interviewed girls.

In what follows we present some descriptive statistics on teenage childbearing, individual characteristics, school quality and conditions of the neighborhood. Table 2 reports the main descriptive statistics of variables used on the exercise for the total sample and the built subsample⁹. The first three columns refer information on the total sample including recovered information on sisters. The last three columns refer information on the subsample used for the econometric exercise split on the interviewed girls and their older sisters. Data on the second column show that the teenage childbearing incidence is 3%, the average age is around 15.41 years old, the household size is in average 5.26 members, the mother has in average 3.24 children and 24% of the girls study in private schools. The other variables describe school quality (larger index reports high quality), neighbor security (one theft by thousand inhabitants) and standard of living (larger index report better standard of living)¹⁰. The column 5 shows the same descriptive statistics for the subsample of interviewed girls used in the econometric exercise.

By comparing the two cohorts (interviewed girls and sisters) within the total sample and the subsample in Table 2, we observe a better balance between the young and the old cohort in the subsample used in our empirical strategy. Column 6 shows descriptives for the older sister in the

⁹Table 1 reports the definition of all variables in our empirical strategy.

¹⁰School quality index measures the average score in math in SABER at the grade 5th in primary schools (a general public examination implemented by the national government). The living standard index weights dwelling conditions and socioeconomic achievements (parents' education) of the interviewed girls' family.

subsample. There are two important difference with respect to the other groups in our survey, the childbearing incidence and the age. The group of the cohort of older sister have the largest childbearing incidence (5%) which is expected given that age span from 19 to 32 years old. Note that the age is average larger than their counterpart in the total sample and it is even large gap when comparing with the interviewed girls. This is expected by the way we built the cohort group and we use the age as control for the cohort effect. There are differences on some other observables between interviewed girls and sisters in the subsample (column 5 and 6) like higher average age (sisters are 6.33 years older), student-teacher ratio at the year of schooling (3.52 of difference againts sisters), higher mother's number of children (3.6 against 3.72), and difference on security measures at the community level at the period of being teenage. In addition the data show no differences on other explanatory variables, but all the observables are introduced in the econometric specification. In addition, we observes that young and old cohort are lees balanced in the total sample, it can be explained because the comparing cohort in our subsamples is building from sisters that were studied at the same school of interviewed girls.

Tables 3 and 4 show the distribution of the subsample between young and old cohort and treated and no treated groups for the two types of education programs. Table 3 (table 4) shows that the 52% (1251) observations are sisters and 48% (1151) are interviewed girls. The columns 3 and 4 in each table shows the distribution of the two cohorts on the groups of treated and no treated with the program of Familias en Accion (the national education subsidy) The 70% (1680) of the observations belongs to the control group and there is a balance of the distribution of treated and no treated within both old and young cohorts. The columns 5 and 6 show the similar figures for the *subsidio educativo*. The 56.5% (1359) of the observations are included in the control group, and threere is not unbalance on the distribution between treated and no treated across cohorts.

Table 5 shows the average prevalence of childbearing by cohort and treatment status. Consider first the effect of *Subsidio Educativo* in the second panel. Notice that the treated schools have higher teenage childbearing prevalence before the treatment is implemented (0.074 against 0.035). After the treatment both control and treatment schools end up with similar childbearing prevalence (around 2.3%). This happens because teenage prevalence decreases in 1.2 percentage points in the control group and 5 percentage points in the treatment group. Without controlling for covariables the causal effect of subsidio Educativo on teenage childbearing amounts to a reduction of 3.8 percentage points. A similar analysis can be done for Familias en acción. In contrast, Familias en acción seems to have caused an increase in childbearing.

5 Results and robustness checks

All Results and Robustness Checks are in the Appendix. In Table 6 we report regressions without controls. In each Column we include a single treatment. In each column the interaction between cohort and treatment gives an initial idea about sign and significance of causal effects. In Column (1) we report results of conditional cash transfers granted by the Education Secretary of Bogota (*SED*). In Column (2) we report results of conditional cash transfers of Familias en Acción (*FAM*). The CCT program that seems to reduce teenage childbearing is the conditional cash transfer granted by SED (Column (1)). There is no evidence showing that the conditional cash transfer granted by Familias en Acción has a causal effect on teenage childbearing. Of course, these results may be subject to selection bias due to omitted variable.

In Table 7 we report estimates of Equation (1). In Column (1) we report results with no controls. In the following Columns we progressively introduce controls of individual (age), household (size, number of mother’s children, socioeconomic index, and number of rooms at home), school (student-teacher ratio, quality, Private-Public, distance to public transport system, contracted enrollment¹¹) and *localidad* (crime rates, fixed effects) level, respectively. First of all notice that the causal effect of the conditional cash transfer granted by SED is robust to all specifications. The SED cash transfer causes a reduction of teenage childbearing. The reduction amounts to 4.8 percentage points. Second, notice that controlling by individual’s age (compare Column (1) and Column (2)) the cohort coefficient changes sign and becomes significant. Girls from the younger cohort are more likely to become teenage mothers than girls from the older cohort. We can credibly believe that controlling by age help us to reduce the sub estimation of teenage childbearing in the younger cohort. Fourth, *FAM* cash transfers seems not to have an effect on teenage childbearing (Column (5)).

The estimates using continuous treatments instead of dummy treatments confirm the previous findings (See Table 8). Most important, marginal changes in the proportion of female students granted with SED cash transfers seems to reduce teenage childbearing. However, the estimates have a lower significance.

In Tables 9 and 10 we report the main robustness checks. In Table 9 we report a sequence of regression in which we control for age of sister in the old cohort. In doing so, we face a trade-off between having a better control group and losing precision in our estimates due to small sample. In Column (1) we report our benchmark regression. In Columns (2) to (5) we progressively exclude younger sisters in the old cohort. The younger the more likely to be recipient of any CCT. Notice that the causal effect progressively increases in magnitude as well as its standard

¹¹Government of Bogota can contract private schools to provide education services to students that cannot be attended by the public sector.

error. In the last column the sample is too small to obtain a significant unbiased estimation.

Let us now consider Table 10. In Column (2) we report results of a placebo treatment. We split the sample of the older cohort in two: younger sisters (19-22 years old) and older sisters (23-32 years old). We must have no effect in order to validate the underlying assumption of our approach. As we can see in Column (2), treatment has no effect on older cohorts, so we can credibly attribute our findings to the treatments. We further restrict sample, in order to compare girls in schools with SED high treatment with girls in schools with no treatment at all. We take schools where the proportion of girls granted with subsidy different from *SED* cash transfers is smaller than its average proportion (Column (3)). We manage to recover estimates of causal effect of *SED* cash transfers similar in magnitude and significance to those estimated using the full sample. Column (4) reports bootstrapped residuals clustered by school. The causal effect is still significant.

Table A presents the probability above the bootstrapped t- statistics. The causal effect of SED cash transfers is also robust to this test. Finally, in Table 12 we report results obtained when we change the threshold. We use different percentiles ranging from 30th percentile to 60th percentile to test whether the choice of an arbitrary threshold is driven the result. The causal effect is robust to thresholds changes.

6 Final remarks

Not all educational conditional cash transfers (CCT) have an impact on teenage childbearing. We identify two CCT programs implemented in Bogotá, Colombia, i.e., *Familias en Acción* (the Colombian version of PROGRESA) and the *Subsidio Educativo*.

Our main finding is that the *Subsidio educativo* causes a sizeable reduction on teenage childbearing while *Familias en Acción* has no effect. Even though we are not able to identify the channel(s) through which this policy reduces teenage childbearing using our data, the single feature that differs across the CCTs is the performance condition included in the program that reduces childbearing. Conditions on academic performance seems to provide girls enough incentive to reduce childbearing. The *Subsidio Educativo* just requires not to fail the academic year to renew the CCT for one year more. However, how strong should be the most efficient performance condition remains a question to be investigated.

Future research is needed to fully identify conditions that affect behavior of young girls about childbearing choices and can be implementable through education policies.

A Tables

Table 1: Variable glossary

VARIABLES	Definition
Children	Number of mother's children
Cohort	Identifies cohort, 1: Interviewed girls, 0: Sisters of interviewed girls
Distance	Distance of school to nearest <i>Transmilenio</i> station
FAM	Conditional cash transfers granted by <i>Familias en Acción</i>
HH Size	Household size including the student
Motorcycle theft	Motorcycle theft rate (one per thousand inhabitants) by locality
Person theft	Thefts to persons rate (one per thousand inhabitants) by locality
PRIV	Enrollment subsidy granted by SED to students in private schools
Private	1 if she studies in a private school
Rooms	Number of rooms in the student's house without including kitchen, bathrooms or garages
Sch-quality	Average math results of students in 5 year of primary school 2009
SED	Conditional cash transfer granted by SED
Standard of living	Socioeconomic Index that includes sanitary type, overcrowding, house's floor material and parents' education
Students-teacher	Students-teacher relationship (2004 for sisters, 2008 for interviewed)

Table 2: Descriptive Statistics

Descriptive Statistics						
	Total n=24885			Sample n=2402		
	Interviewed girls	Sisters	Diff ¹	Interviewed girls	Sisters	Diff ¹
Childbearing	0.03	0.03	0.00 **	0.02	0.05	-0.03 ***
Age	15.41	19.23	-3.82 ***	15.49	21.82	-6.33 ***
Standard of living	23.16	22.55	0.61 ***	22.97	22.90	0.07
HH Size	5.26	5.72	-0.46 ***	5.57	5.66	-0.09
Children	3.24	3.70	-0.46 ***	3.60	3.72	-0.12 *
Rooms	4.10	4.22	-0.12 ***	4.30	4.31	-0.01
PRIV	0.12	0.10	0.02 ***	0.10	0.10	0.00
Students-teacher	27.02	30.71	-3.69 ***	27.03	30.55	-3.52 ***
Sch-quality	325.49	325.01	0.48	329.90	330.18	-0.28
Private	0.24	0.22	0.02 ***	0.25	0.25	0.00
Distance	2110.54	2135.72	-25.18	1978.68	1981.26	-2.58
Person theft	1.42	0.77	0.65 ***	1.46	0.82	0.64 ***
Motorcycle theft	0.13	0.15	-0.02 ***	0.13	0.17	-0.04 ***

¹ Asterisks are the level of significance for the mean difference (two-tailed) between the interviewed girls against their sisters who who studied in the same school: ***p<0.001 **p<0.05 *p<0.1
See Table refglosario for the meaning of each variable

Table 3: Treatments frequency

Treatments Frequency					
	Total	Familias en acción		Subsidio Educativo	
		Control	Treated	Control	Treated
Sisters	52.08%	36.26%	15.82%	29.60%	22.48%
Interviewed	47.92%	33.68%	14.24%	26.98%	20.94%
Total	100%	69.94%	30.06%	56.58%	43.42%

See Table 1 for the meaning of each variable

Table 4: Treatments frequency

Treatments Frequency					
	Total	Familias en acción		Subsidio Educativo	
		Control	Treated	Control	Treated
Sisters	1251	871	380	711	540
Interviewed	1151	809	342	648	503
Total	2402	1680	722	1359	1043

See Table 1 for the meaning of each variable

Table 5: Outcome

Outcome: Childbearing						
	Familias en acción			Subsidio Educativo		
	Control	Treated	Diff	Control	Treated	Diff
Sisters	0.052	0.05	-0.0028	0.035	0.074	0.038
Interviewed	0.018	0.035	0.016	0.023	0.023	0,0007
Diff	-0.034	-0.013	0.019	-0.012	-0.050	-0.0382

See Table 1 for the meaning of each variable

Table 6: Regressions without controls

Outcome: childbearing		
VARIABLES	(1)	(2)
Cohort	-0.0120 (0.00847)	-0.0343*** (0.00984)
SED	0.0389*** (0.0149)	
Cohort x SED	-0.0382** (0.0174)	
FAM		-0.00281 (0.0150)
Cohort x FAM		0.0194 (0.0182)
Observations	2,402	2,402
R-squared	0.011	0.006

Standard errors clustered by school
***p<0.001 **p<0.05 *p<0.1
See Table 1 for the meaning of each variable

Table 7: General Results

Outcome: childbearing					
VARIABLES	(1)	(2)	(3)	(4)	(5)
Cohort	-0.0152 (0.00933)	0.0832*** (0.0151)	0.0843*** (0.0152)	0.0792*** (0.0149)	0.0654*** (0.0160)
Cohort x FAM	0.0124 (0.0214)	0.00938 (0.0211)	0.00916 (0.0210)	0.00650 (0.0213)	0.0126 (0.0225)
Cohort x SED	-0.0492** (0.0222)	-0.0517** (0.0219)	-0.0522** (0.0218)	-0.0502** (0.0215)	-0.0483** (0.0211)
Individual controls	No	Yes	Yes	Yes	Yes
Family controls	No	No	Yes	Yes	Yes
School controls	No	No	No	Yes	Yes
Localidad	No	No	No	No	Yes
Observations	2,402	2,402	2,402	2,402	2,402
R-squared	0.012	0.042	0.047	0.054	0.061

Standard errors clustered by school

***p<0.001 **p<0.05 *p<0.1

See Table 1 for the meaning of each variable

Table 8: Continuous Treatments

Outcome: childbearing					
VARIABLES	(1)	(2)	(3)	(4)	(5)
Cohort	-0.0220** (0.0108)	0.0779*** (0.0161)	0.0796*** (0.0162)	0.0714*** (0.0161)	0.0543*** (0.0179)
Cohort x FAM	-0.0954 (0.0937)	-0.115 (0.102)	-0.115 (0.102)	-0.124 (0.101)	-0.106 (0.101)
Cohort x SED	-0.208 (0.137)	-0.247* (0.135)	-0.250* (0.135)	-0.224* (0.130)	-0.218* (0.124)
Individual controls	No	Yes	Yes	Yes	Yes
Family controls	No	No	Yes	Yes	Yes
School controls	No	No	No	Yes	Yes
Localidad	No	No	No	No	Yes
Observations	2,402	2,402	2,402	2,402	2,402
R-squared	0.014	0.044	0.047	0.052	0.059

Standard errors clustered by school

***p<0.001 **p<0.05 *p<0.1

See Table 1 for the meaning of each variable

Table 9: Robustness Checks

Outcome: childbearing					
VARIABLES	(1)	(2)	(3)	(4)	(5)
	Benchmark	Without	Without	Without	Without
		19	19 and 20	19, 20 and 21	19, 20, 21 and 22
Cohort	0.0654*** (0.0160)	0.0599*** (0.0192)	0.0287 (0.0270)	-0.0276 (0.0338)	-0.0741 (0.0471)
Cohort x FAM	0.0126 (0.0225)	0.0131 (0.0270)	0.0139 (0.0339)	0.0115 (0.0427)	0.0361 (0.0442)
Cohort x SED	-0.0483** (0.0211)	-0.0586** (0.0262)	-0.0747** (0.0327)	-0.0912** (0.0408)	-0.0668 (0.0465)
Observations	2,402	2,115	1,866	1,699	1,549
R-squared	0.061	0.060	0.063	0.073	0.080

Standard errors clustered by school
***p<0.001 **p<0.05 *p<0.1
All controls included in all regressions
See Table 1 for the meaning of each variable

Table 10: Robustness Checks

Outcome: childbearing				
VARIABLES	(1)	(2)	(3)	(4)
	Benchmark	Placebo ¹	No program	Bootstrapping
Cohort	0.0654*** (0.0160)	-0.0583* (0.0299)	0.0675*** (0.0185)	0.0654*** (0.0159)
Cohort x FAM	0.0126 (0.0225)	0.00609 (0.0400)	0 (0)	0.0126 (0.0238)
Cohort x SED	-0.0483** (0.0211)	-0.0237 (0.0403)	-0.0464** (0.0210)	-0.0483** (0.0236)
Observations	2,402	1,516	1,680	2,402
R-squared	0.061	0.067	0.077	0.061

Standard errors clustered by school
***p<0.001 **p<0.05 *p<0.1
¹ Placebo cohort
All controls included in all regressions
See Table 1 for the meaning of each variable

Table 11: Bootstrapping t statistics

	p-value
Cohort	.015
Cohort x FAM	.535
Cohort x SED	.015

See Table 1 for the meaning of each variable

Table 12: Different Thresholds

Outcome: childbearing					
VARIABLES	(1)	(2)	(3)	(4)	(5)
	Benchmark	P30th	P40th	P50th	P60th
Cohort	0.0654*** (0.0160)	0.0699*** (0.0181)	0.0670*** (0.0162)	0.0653*** (0.0159)	0.0552*** (0.0150)
Cohort x FAM	0.0126 (0.0225)	-0.00118 (0.0312)	0.0255 (0.0252)	0.0146 (0.0233)	0.0173 (0.0218)
Cohort x SED	-0.0483** (0.0211)	-0.0367** (0.0156)	-0.0342** (0.0148)	-0.0417** (0.0179)	-0.0313* (0.0166)
Observations	2,402	2,402	2,402	2,402	2,402
R-squared	0.061	0.060	0.060	0.060	0.059

Standard errors clustered by school
***p<0.001 **p<0.05 *p<0.1
All controls included in all regressions
See Table 1 for the meaning of each variable

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