Conditional Cash Transfer Programs: Measuring the Impact on Education*

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Abstract

Conditional cash transfers have become a popular tool the government employs to enhance the living conditions of the poor and vulnerable citizens in Latin America. Using the synthetic counterfactual method, we study the effect of these plans on education in three different countries: Argentina, Ecuador and Colombia. We find that in Argentina, the effect of AUH for the three consecutive years after its implementation is positive and increasing in time. This result is validated using two inference techniques: dif-in-dif and placebo test. In the case of Ecuador, the method does not provide a suitable control unit, therefore the effect of BDH remains unacknowledged. Finally, for Colombia we also identify positive and increasing effects starting three years after FA was implemented. This could be explained as a result of the 2003 reform that FA went through.

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1 Introduction

Conditional cash transfers (CCT) have become a widespread policy in Latin-American countries during the first decade of the millennium, mostly due to the successful experience of Progresa in Mexico, which started in 1997. Although the Mexican plan had focused on impoverished rural communities exclusively in its early stages, the scope of this program changed in order to include urban communities.Currently, CCTs consist in sums of money transferred to poor households on the condition that children satisfy certain health and educational requirements. The main aim of this strategy is to tackle two problems simultaneously: short-term deficits in consumption and the intergenerational transmission of poverty due to low educational attainments and bad health conditions. According to the Comisión Económica para América Latina (CEPAL)[1],in 2011 more than 120 million people in the region were covered by a program of this kind. This represented 20% of total population at the moment.

The requirements households are asked to fulfill vary across countries. In most of them the transfer is given once conditionalities are satisfied. In the case of Asignación Universal por Hijo, from Argentina, students are asked to show regular attendance, which has to be certified by the school on an annual basis. In other cases the requirement is to be present in more than 80% of classes or even to be among the best marks in class.

Argentina was a latecomer to this practice[2]. During the last months of 2009 a law was passed in the form of a presidential decree: monthly deposits were to be made by the government in a bank account, created specifically for this transference, where 80% of them are deposited before the head of the household shows the requirements were fulfilled, and the remaining 20 % afterwards. Compared to other countries, it is important to note that a distinguishing feature of the Argentinian program is the group on which it is focused: informal workers and unemployed people.

Another relevant case study is Ecuador, who issued Bono Solidario in 1998: this plan did not require anything from beneficiaries and the monetary sums were relatively low. It suffered an important update in May 2003, setting – as it became commonplace – educational requirements in return for the money. Moreover, the amount disbursed were significantly enlarged.

A third interesting case of study is the Colombian program Familias en Acción, created as part of Red de Apoyo Social (a strategy to mitigate the effects of the economic crisis suffered in the 1990's). The program was initially

[1]See Simone Cecchini (2011)

[2]Only Belize, Haiti and Honduras among twenty Latin American countries have started their programs after Argentina.

focused on the conservation of human capital through nutritional and educational components. The first one involves compulsory assistance to growth and development controls; the second demands school assistance.

The topic of children decision making has been widely discussed in economic literature[3]. The main question regards the time allocation of children between different activities. The available options have been suggested to be: schooling, child labor and leisure. One of the main theoretical challenges is to come up with an understanding of how these activities substitute one for another. This has important practical consequences because reducing the incidence of child labor and increasing schooling are both social and political goals.

One of the main topics, stemming from example from Becker (1974) is the intra-household decision making process. Models vary according to the agent assumed to take the decision of schooling and their preferences. In Bursztyn et al (2010) a principal-agent model is suggested, where parents take the decision of schooling but cannot observe children behavior, who have different preferences between leisure and school attendance. In this article, the Brazilian program *Bolsa-Escola* is seen under a new light. It works as a device which attenuates the moral hazard resulting from the previous differences in information. In De Janvry et al (2006) the choice between school-only or school and child labor is taken by parents. They find that the Mexican program *Progresa* worked as a "safety net" that kept children from leaving school when bad economic shocks hit the economy.

In an interesting paper, Kruger et al (2007) suggest that to understand the link between child labor and poverty it is essential to distinguish between income and substitution effects. More wealth increases children education, while temporary increases in economic activity (in their article, due to agricultural booms) lead to lower school attendance, as the opportunity cost of attending school is higher. We take this emphasis in the opportunity cost of schooling as motivation for our study, since it is clearly affected by CCT programs.

Previous studies have investigated the impact of this kind of programs on child labor, family per-capita income, indigence and adult wage, among other things (see for example Doran (2006), Edmonds and Schady (2012) and Poy, Salvia and Tuñon (2014)). Here, we will try to address the effect of CCTs on education. To do so we will be working with the Gross Enrollment Rate in Secondary level (SGER), measured as the number of children enrolled in secondary level, irrespectively of their age, divided by the amount of chil-

[3] For a survey, see "Explaining the demand and supply of child labour: a review of the underlying theories", issued by the International Labor Organization in 2007.

dren in age of attending secondary in the country. This variable is mainly an indicator of the level of educational demands of the society[4]. The motivation for choosing this variable is that CCTs change the opportunity cost of education. Gross enrollment allows us to capture a double effect of the program on education. The first one involves children who face the decision to drop out but now they decide to continue in the educational system since the return on education is higher. On the other hand, children that have left school now may find it profitable to return[5]. Finally, since primary school attendance is nearly universal, we focus on secondary level.

The usual approach to evaluate the impact of policy interventions is to compare treated units - families - with comparable untreated units within the country. This demands the availability of micro-data which tracks different households in time. Unfortunately, these data is not gathered by the countries analyzed here. However, when dealing with universal CCT programs[6] even if micro-data were available the identification of the effect would be difficult if not impossible, due to a self-selection bias. This arises because people that demands these universal programs is different from people that does not, so using the second group as potential control is misleading. This justifies that we take a different standpoint: we focus in the aggregate variable for each country we described in the previous paragraph.

In the following figure we can see the evolution of SGER for Argentina, Colombia and Ecuador. It can be noticed that the increase in SGER for Argentina starting approximately in 2010 is not similar to what had been the previous trend. For Ecuador we can spot a change in behavior for the series starting somewhere between 2004 and 2008. For Colombia it seems harder to assert that 2001 was a significant date in the behavior of the series.

However, it is not obvious that the SGER would increases after a program is adopted in a specific country. This is the case due to two main reasons. First of all, families may have been sending their children to school before being covered by the program, and there is no reason to suspect that their behavior would change once the program started. Secondly, if there had existed a shortage of educational institutions supply, the greater demand induced – a priori - by the different plans would not have translated into greater gross enrollment rates for each of the countries of interest.

We are interested in measuring the change in the SGER series. To achieve this, we will use the Synthetic Counterfactual Method, developed by Abadie and Gardezabal, to calculate what the SGER would have been in the absence

[4] It is also considered a measure of external effectiveness by Llach, Montoya and Roldán (1999. Chapter 1).

[5] This effect on enrollment it is not accounted by Net enrollment Rate, for example.

[6] When analyzing AUH this problem is of particular importance.

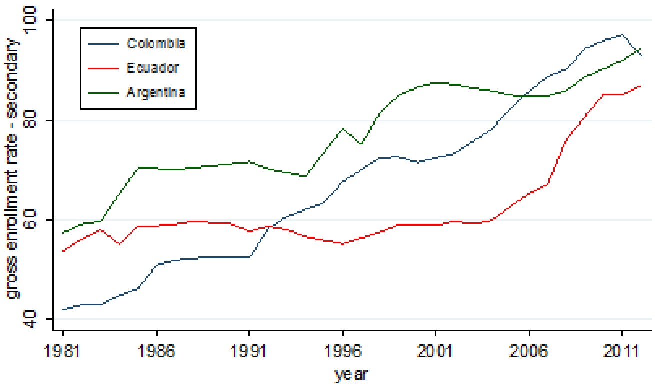

Figure 1: Secondary Gross Enrollment - Three Latin American Countries

of the CCT. More specifically, we intend to construct an artificial country that resembles our unit of interest before the treatment period (2001 for Colombia, 2004 for Ecuador and 2010 for Argentina). With these synthetic countries, we will look at their respective SGER for the years after each of the plans started. This should provide a good picture of what *would have happened* in each of the countries of interest if they had not implemented the CCT.

The rest of this thesis is organized as follows. Section 2 summarizes synthetic control methods and describes inferences techniques. Section 3 describe the data used to estimate and how the sample was constructed. We present the results for the different countries, along with the inferences on this estimates, in section 4. Finally in section 5 we conclude.

2 Method

2.1 Synthetic Counterfactual

In this section we will briefly discuss the econometric technique employed in our work. For a more technical discussion of the method and its implementation see Abadie and Gardezabal (2003) or Abadie, Diamond and Hainmueller (2010) and for different approaches to policy evaluation see Wooldridge and Imbens (2008). Suppose that we have data for a sample of $j = 1, 2, \ldots, J$ countries. Let's assume, without loss of generality, that the first unit ($j = 1$) is the one affected by the policy intervention of interest. The donor pool, that is, the set of potential comparisons, is the collection of the $J-1$ countries not affected by the intervention. Let Y_{jt} denote the outcome of interest which we observe for T periods. Let T_0 be the number of preintervention periods, with $1 \leq T_0 \leq T$. For each country, j, we observe also a set of k predictors[7] of the outcome: $X_{1j}, \ldots, X_{kj}$ (which may include preintervention values of Y_{jt}). Finally we define Y_{it}^N and Y_{it}^I to be the variable of interest before and after treatment, respectively[8]. We assume that that the intervention has no effect on the outcome before implementation (no anticipation effect) and on the other units in the sample (no interference between units)[9]. Then, the effect of the policy for the affected country in period t (with $t > T_0$) is

$$\tau_{1t} = Y_{1t}^I - Y_{1t}^N.$$

We aim to estimate $\{\tau_{1,T_0+1}, \tau_{1,T_0+2}, \ldots, \tau_{1,T}\}$. It is clear that for $t > T_0$ we only observe the potential outcome under the intervention, that is $Y_{1t} = Y_{1t}^I$. For this reason, the great challenge is to estimate Y_{1t}^N for $t > T_0$: *how the outcome of interest would have evolved in the affected country in the absence of the intervention.* This is a counterfactual outcome.

Comparative case studies aim to reproduce Y_{1t}^N using one unaffected unit or a small number of unaffected units that had similar characteristics as the unit of interest. In these cases the choices of the units of comparison are made by the researcher based on these characteristics. The synthetic control method is based on the observation that a combination of units in the donor pool may resemble the characteristics of the affected unit substantially better than any unaffected unit alone. In the synthetic control method the election of the control units is endogenous, the only variable choice for researchers is the donor pool they are working with.

Then, the synthetic control is defined as a weighted average of the units in the donor pool. Letting $\{\omega_2, \ldots, \omega_J\}$ be the weights for each country in the donor pool the synthetic control estimator of Y_{1t}^N and τ_{1t} are:

$$\widehat{Y_{1t}^N} = \sum_{j=2}^{J} \omega_j^* Y_{jt}$$

and,

$$\widehat{\tau_{1t}} = Y_{1t} - \widehat{Y_{1t}^N}$$

[7]Sometimes this predictors are also referred to as covariates in the literature.

[8]These are the "potential responses" of Rubin's Model for Casual Inference, Rubin (1974).

[9]For a detailed discussion of the assumption of no interference between units see Rosenbaum (2007).

Formally, suppose that Y_{jt}^N is given by a factor model

$$Y_{jt}^N = \delta_t + \theta_t Z_j + \beta_t \mu_j + \varepsilon_{jt}, \tag{1}$$

where δ_t is an unknown common factor with constant factor loadings across units, Z_j is a $(k \times 1)$ vector of observed covariates, θ_t is a $(1 \times k)$ vector of unknown parameters, β_t is a $(1 \times F)$ vector of unobserved common factors, μ_j is an $(F \times 1)$ vector of unknown factor loadings, and the error term ε_{jt} are unobserved transitory innovations at the regional level with zero mean.

Consider $W = (\omega_2, \ldots, \omega_J)$ a vector of weights such that $\omega_j \geq 0$ and $\sum \omega_j = 1$[10] . Then the value for each synthetic control indexed by W is

$$\sum_{j=2}^{J} \omega_j Y_{jt} = \delta_t + \theta_t \sum_{j=2}^{J} \omega_j Z_t + \beta_t \sum_{j=2}^{J} \omega_j \mu_j + \sum_{j=2}^{J} \omega_j \varepsilon_{jt}.$$

Suppose that there are $(\omega_2^*, \ldots, \omega_J^*)$ such that

$$\sum_{j=2}^{J} \omega_j^* Y_{jt} = Y_{1t}, \quad t \in [1, T_0] \tag{2}$$

$$\sum_{j=2}^{J} \omega_j^* Z_j = Z_1 \tag{3}$$

and $\sum_{t=1}^{T_0} \beta_t^{'} \beta_t$ is nonsingular, then,

$$Y_{jt}^N - \sum_{j=2}^{J} \omega_j^* Y_{jt} = \sum_{j=2}^{J} \omega_j^* \sum_{s=1}^{T_0} \beta_t \left(\sum_{s=1}^{T_0} \beta_t^{'} \beta_t \right)^{-1} \beta_t^{'} \left(\varepsilon_{js} - \varepsilon_{1s} \right) - \sum_{s=1}^{T_0} \omega_j^* \left(\varepsilon_{js} - \varepsilon_{1s} \right). \tag{4}$$

[10] We restrict the weights to sum to one in order to avoid extrapolation bias.

Abadie, Diamond and Hainmueller prove in Abadie, et al. (2010) - Appendix B that under standard conditions, the mean of the right-hand side of the last equation will be close to zero if the number of preintervention period is large relative to the scale of the transitory shock. This suggest using as an estimator of τ_{1t},

$$\widehat{\tau_{1t}} = Y_{1t} - \sum_{j=2}^{J} \omega_j^* Y_{jt}, \quad t \in [T_0 + 1,\ T]. \tag{5}$$

2.2 Implementation

Synthetic controls method motivates the question of how the wieghts should be chosen. Abadie and Gardezabal (2003) and Abadie, Diamond, and Hainmueller (2010) propose to choose $\{\omega_2, \ldots, \omega_J\}$ so that the resulting counterfactual best resembles the pre-intervention characteristics of the treated unit. That means selecting $W^* = (\omega_2^*, \ldots, \omega_J^*)$ to minimize

$$W^* = \arg\min \{\|X_1 - X_0 W\|_V\} \qquad s.t.\ \ \omega_j \geq 0\ \forall j,\ \ \sum \omega_j = 1.$$

Where X_1 is a vector containing the covariates and past and linear combinations of Y_{1t}, X_0 is a matrix that contain the same variables for the countries in the donor pool and V is a symmetric and semidefinite positive matrix of weights that reflect the relative importance of the synthetic control reproducing the values of the predictors. Abadie (a) proposes four different methods to select among different V. Here, we will employ the third procedure: V is selected in order to minimize the size of the prediction error, $Y_{1t} - \widehat{Y_{1t}^N}$, during some set of preintervention periods. This can be implemented by solving a nested optimization problem where V is chosen so that W minimizes the mean square prediction error over a pre-specified set of pre-intervention periods.

2.3 Inference

In its basic formulation the Synthetic Control Method does not allow for traditional techniques to evaluate the significance of the results obtained. This triggered other researchers to develop alternative methods to do so. Following Abadie et al. (2010) and Campos, Coricelli and Moreti (2014) we will perform permutation and difference-in-difference tests.

Permutation – or placebo – tests have been used extensively to evaluate the effect of different interventions[11]. The main idea is to compute the distribution of the estimated effect – calculated using the synthetic counterfactual - treating different countries in the donor pool as the intervention unit, successively. Then, the effect obtained for the country with the treatment is compared with those obtained for countries chosen at random from the donor pool. If the estimates from the placebo were similar to those estimated for the treated unit then we would conclude that our results were not significant. In other words, the difference in the variable analyzed cannot be attributed to the program. We will study the results for two variables for each placebo: the gap between the real and the synthetic series and the ratio between the mean suqared prediction error (MSPE) post and pre-treatment. That is, we will be interested in

$$\widehat{\tau_{jt}} = Y_{jt} - \widehat{Y_{jt}^N} \quad \forall j, t \tag{6}$$

$$\alpha_j = \frac{MSPE_{J|t>T_0}}{MSPE_{j|t\leq T_0}} \; \forall j \tag{7}$$

Each of them allowed us to evaluate a different aspect of our results. The first one gives us an idea of how often effects of the same sign as the one we have are found. The problem with this measure is that it tells us little about the magnitude of the effect since it does not include any aspect regarding the goodness of fit of the counterfactual. The second variable captures information about the magnitude independently of its sign. It tell us how often magnitudes of this kind are found. It is important to note that if our estimates are significant we should expect that $MSPE_{1|t>T_0} > 0$ and $MSPE_{j|t\leq T_0}$ close to zero. For the rest of the countries we should have $\alpha_j \backsimeq 0$. Namely, a significative effect, in this context, is an uncommonly large α_1.

The difference-in-differences test is widely used in panel data analysis for policy evaluation. It compares the average value of the difference between treated and non-treated units before and after the treatment year. The test can be implemented using the following regression,

[11] To name a few, this type of tests were performed to evaluate the impact of terrorism on economic growth by Abadie y Gardezabal (2003), the effect computers had in the distribution of wages by DiNardo and Pischke (1997) or to validate the rational-addiction model for tobacco consumption Auld and Grootendorst (2004).

$$y = \beta_0 + \beta_1 S + \beta_2 T + \beta_3 (S \cdot T) + \varepsilon \quad (8)$$

Where S stands for a dummy variable that takes the value of 1 when the data comes from the treated country, T is a dummy step variable which takes the value of 1 for the post-treatment years. The parameter of interest is β_3, which measure the average difference between the series once other differences, which can be attributed to country-specific effects or time trends, β_1 and β_2 respectively, are taken into account[12]. We will apply this test using different post-treatment horizons, as if we had made this study using data available up to each of these years separately.

3 Data and Sample

We use annual country-level panel data for the period 1989-2012[13]. According to Abadie and Gardezabal (2003) synthetic counterfactual estimates can be improved by restricting the donor pool to countries with similar characteristics to the country exposed to the treatment (so as to avoid interpolation bias) and by increasing the number of pre-intervention years. Because Enrollment rate was not available for all Latin American countries for large time periods we selected 1989 in order to have a reasonable number of countries in our sample and a relative large number of pre-intervention periods. It ends in 2012 because it was the last year for which data was available. Since AUH was introduced in 2009, FA in 2001 and BDH in 2003 we have 21, 13 and 15 pre-treatment years respectively.

As discussed earlier, implementing the synthetic counterfactual method is not straightforward. In this particular study, special care needs to be taken with respect to the countries chosen to construct it because those who have carried out similar plans, for the relevant period, cannot be included in the donor pool. In particular, if we included every country with CCT plans, we would be *underestimating* the effect in each of the countries of interest, given that the synthetic counterfactual – including countries with CCT - would involve bigger SGER rates than if these countries had not. So, the effect, calculated as the difference between real values and this counterfactual influenced by the treatment would be lower than the real effect. With this said, we will nevertheless try to detect countries with "bigger" CCT plans

[12] See Bertrand et al. (2004) for a critique of this tests.

[13] In some cases we restrict to a shorter time period, for example to estimate the effect of Bono de Desarrollo Humano in Ecuador. There we use data until 2009.

relative to our treated unit, which cannot be said to belong to the control group. This was the main motivation behind the design of table 1.

The information above describes the variables which, in our view, provide a way of comparing, objectively, conditional transfer programs launched in Latin America.

The first criterion we used in order to determine which plans could have had a greater impact is the percentage of population reached by each plan. The rationale behind choosing a proportional and not an absolute measure is as follows:

The impact of a plan that reaches 20% of the population in a country of 20 million inhabitants will have approximately the same impact - will be the same size, for our ends- of a plan that reaches 20% of the population in a 5 million inhabitants country because, all other things equal, the gross secondary enrollment ratio is a relative, not an absolute value. If we took absolute values to compare both programs, we would be misled to conclude that the first plan was bigger, in the sense that has more potential for impact, whereas the SGER change was equal for both. An example that epitomises the point made above is Brazil with over 200 million people versus Trinidad and Tobago, which barely surpasses the 1 million mark. In order not to be confused when contrasting the scope of each program, the percentage value conveys a more precise image.

The second reference we will consider is the average amount received by the beneficiaries, adjusted by purchasing power parity (PPP). We understand that to have a significant impact on the enrollment rate, each plan should hold a sufficiently high monetary allure for the family to actually send their children to school.

The difficulty lies in how these two criteria should be weighted: what does it mean for a plan to be "bigger" than the other? Which plan has a larger size: one that covers double the number of beneficiaries (always as a percentage of total population) or one which transfers double the amount in PPP terms?

In our view, the optimum way of deciding the countries which cannot be part of the donor pool is by a process of elimination. This means intercalating conditions, for the two variables chosen, and making them stricter for the progressing rounds of rejection. What we are trying to achieve with this process is to eliminate the countries with programs that are too different from the CCT of interest. For Ecuador and Colombia this will be relatively easy since they started their programs a few years after Mexico and Brazil implying that only these two countries should be removed.

Table 1. Conditional Cash Transfers in Latin America

Program	Num. of Beneficiaries	Annual Expenditrue (l.c.)	Aver. Transfer (ppp)	% of pop. reached
Asignación Univ. por Hijo (ARG.)	3,540,717	11,168,600,000	1015.561	8.62%
Bono Juancito Pinto (BOL)	1,625,123	705,917,225	165.477	17%
Bolsa Familia (BRS)	56,458,390	20,740,163,871	240.100	28.45%
Chile Solidario (CHL)	1,108,779	156,368,073,000	405.231	6.47%
Familias en Acción (COL)	11,719,319	1,254,678,000,000	91.295	25%
Avancemos (CRC)	181,570	50,000,000,000	778.215	3.78%
Bono de Desarrollo Humano (ECU)	6,418,479	267,419,364	76.729	41%
Comunidades Solidarias (ESA)	411,931	79,120,620	384.915	6.55%
Mi familia Progresa (GUA)	3,253,635	598,582,575	53.156	23%
Bono 10,000 (HON)	2,347,505	1,056,745,054	45.402	29%
Progresa (MEX)	31,850,000	33,777,400,000	136.243	26.99%
Red de Oportunidades (PAN)	288,956	44,500,000	281.541	7.73%
Tekoporá (PAR)	554,484	29,022,970,005	24.453	8%
Juntos (PER)	3,572,542	724,917,902	132.971	12.07%
Solidaridad (DOM)	2,947,164	5,102,197,320	86.774	28%
Asignaciones Familiares (URU)	527,704	4,365,505,377	513.223	15%

Note: This table was constructed using data from the IMF and the CEPAL. We present the data for the last year in which it was available.

In the case of Argentina, it was more complicated. We eliminated countries in different steps: the first observation that can be made is that all countries where the subsidy represents less than 10% the amount disbursed in Argentina can be considered part of the donor pool as the amount is relatively small. Therefore Colombia, Ecuador, Guatemala and Paraguay will be considered when constructing the synthetic counterfactual, as the PPP amount given to a beneficiary of the AUH is more than ten times the amount given by each of them.

In the second place, if a plan addresses more than 3 times the people benefited by the AUH in percentage terms, one could justifiably argue, using the same rationale as before, that it is too different from the AUH as to make a reasonable comparison. Regardless of the amount being transferred (as long as it fulfills the first condition), the scope of these programs indicates that they should be evaluated on a different scale, because they are clearly bigger than the AUH. This is the reason for taking out Brazil and Mexico from the donor pool.

On the other hand, any plan that offers support to less than 5% of the population needs to be acknowledged as small enough so that it can be an element in the construction of the counterfactual. One country meets this requirement: Costa Rica

If we stopped the analysis here, we would have a grey zone with Bolivia, Chile, Panama, Peru and Uruguay, in which the characteristics in one or both criteria are not sufficiently extreme to classify them either as "too big" or "too small" relatively to the AUH.

As we stated above, given that underestimating the effect is not as harmful for the results as having a very small donor pool, we chose to use 60% of the AUH average reimbursement in PPP terms as the last condition to drop countries. Any country under this barrier will be included in the donor pool. It is critical to note that it is not that we can conclude with certainty that this programs were smaller than the AUH, keeping in mind that by "smaller" we mean "likely to have a smaller impact on the SGER compared to the AUH". What we are doing here is choosing the lesser evil: in the worst scenario we are underestimating the impact of the AUH in SGER. The countries that fulfill this last condition are Bolivia, Chile, Panama, Peru and Uruguay.

We gathered data of SGER from UNESCO Institute of Statistics (UIS), which compiles data for each country from local sources. As we said before, we also need data on other variables which describe important characteristics of each country, the so-called predictors of SGER.

The idea behind including these variables in the selection of the synthetic counterfactual country is to make the comparison for the post-treatment years feasible. In fact, if other determinants of SGER had behaved differently in the country of interest in comparison to the behavior for the synthetic counterfactual we should not assign the difference to the respective CCT, but to the difference in these exogenous variables that in turn determine SGER.

In their work on the impact of terrorism over GDP in the Basque Country, Abadie and Gardeazabal (2003) included investment ratio, population density and other variables related to human capital and sectorial shares as determinants of economic growth. Following literature which tries to predict the evolution of education variables or analyzes past episodes for these series, for different countries, we selected predictors correlated with the development status of a nation, such as GDP per capita in PPP and child mortality rate. We also included variables related with the supply of education in the country, such as expenditure in education by the government over GDP and school life expectancy. We added two variables to gauge the level of urbanization of the country, given that enrollment rates appear to be lower and child labor incidence higher in rural areas. These variables were the percentage of population living in rural areas and agriculture as a proportion of GDP. Given the evidence that the number of siblings in the nuclear family is an important variable in the decision of schooling – mainly for poor families – we included birth rate. Finally we included primary gross enrollment rate. These variables are averaged over the period 1999 - 2009 and augmented by adding linear combinations of SGER. This data was gathered mainly from the World Bank website.

4 Results

4.1 Asignación Universal por Hijo - Argentina

To evaluate the effect of AUH on SGER we need to know how this variable would have behaved in the counterfactual scenario. We plot SGER for Argentina and regions which could, a priori, be a good point of comparison. Although the plot for Latin America performs better than the South American, it does not seem similar enough to expect it can be useful as point of comparison for the post-treatment years.

We follow the methodology of synthetic counterfactual to choose among Latin-American countries those that could resemble better the series for the pre-treatment years in Argentina. The results are presented in Table 2, which compares the pre-treatment real characteristics of Argentina against the same variables for our synthetic counterfactual. As it can be seen in this table,

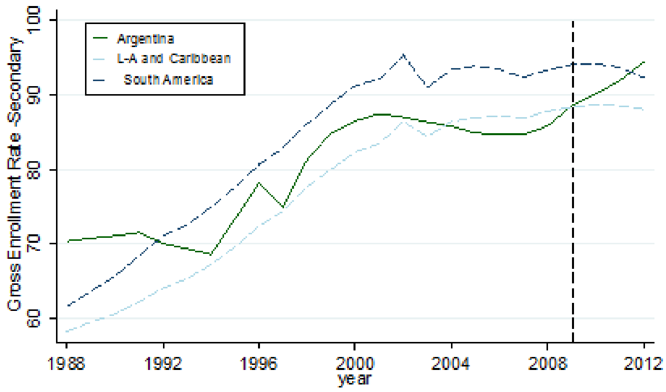

Figure 2: SGER in Argentina, South America and Latin America

predictors in synthetic Argentina are similar to the real ones except for child mortality rate and rural population. To explain why this happens we must recall that the synthetic counterfactual is the result of a convex combination of countries in the donor pool. Therefore, if the value of one of these variables for the treated country is either above the maximum or below the minimum between this same variable for the countries in the donor pool it cannot be reproduced by convex combinations of those. Argentina presents the second lowest rural population in our sample, preceded only by Uruguay, so the differences observed are not a result of a bad estimate but a consequence of a restriction in the sample. In the case of child mortality the divergence is significant because it is not an important predictor of SGER as indicated by the weights in the matrix V.

Table 2. Enrollment rate predictor means - Argentina

	Argentina	
Varaibles	Real	Synthetic
AG%GDP	7.63	9.23
Child mortality rate	17.82	23.68
ED%GDP	3.74	2.89
ln(GDP)	9.52	8.99
Rural population	8.92	22.66
School Life Expectancy	5.17	4.81
Birth rate	17.9	19.89
GER - primary	112.68	113.93
SGER - 1993	69.36	70.05
SGER - 1998	81.41	77.62
SGER - 2005	84.93	85.01
SGER (2006 - 2009)	85.98	86.15
SGER - 2009	88.57	88.03

Table 3 displays the optimal weights for each country in the donor pool. This means that SGER in Argentina is best reproduced by a combination of Cuba (0.067) Ecuador (0.26), Peru (0.341) and Uruguay (0.333).

Table 3. Countries weights in Synthetic Argentina

Country	ω	Country	ω
Chile	–	Nicaragua	–
Colombia	–	Panama	–
Costa Rica	–	Paraguay	–
Cuba	0.067	Peru	0.341
Ecuador	0.26	Uruguay	0.333
Guatemala	–	Venezuela	–

Figure 3 plots the evolution of our estimated counterfactual together with the actual time series. Argentina's SGER has a particular behavior[14]: between 1994 and 1999 the series presents an erratic behavior. This makes it difficult to construct the counterfactual, since most of the countries present smooth series.

[14] We believe this can be explained by important institutional reforms in Education that took place in the country during the decade, which involved changes in the age at which children started secondary school. Another important reform was that provinces became in charge of public schools, rather than central government.

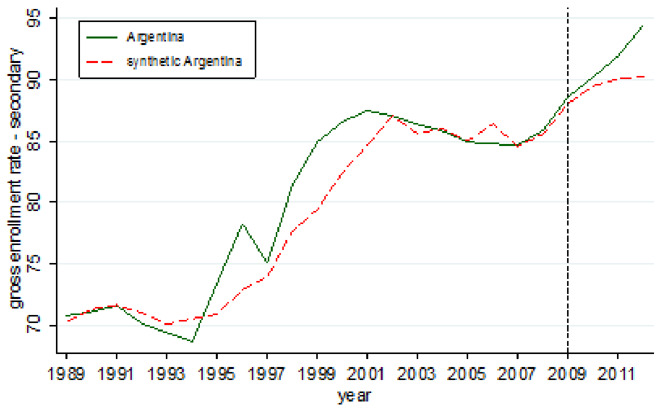

Figure 3: Secondary Gross Enrollment - Argentina vs. Synthetic Argentina

We formulated the optimization problem in order to minimize the RMSPE during the last years before treatment, starting in 2003. Since we are interested in what would have happened after 2010, we were specially concerned with the fact that the synthetic series resembled the actual for the immediate years. It can be noticed that the counterfactual tracks the real values much better than the Latin American average presented before. This, as well as the examination of the values presented in Table 1 suggests the synthetic Argentina provides a good approximation to be used to estimate the effect of AUH.

The estimation of the effect is straightforward: the difference between the synthetic and the real values for the post treatment years. AUH had little effect on the number of students enrolled for the first year after it was implemented, as both series remain practically together after 2010. Nonetheless, after the second year, the real SGER started to raise more than its counterfactual. The effect seems to get larger in time. The average difference between the series for the post-treatment years is 2.25%. It was 0.73% in 2010, 1.86% in 2011 and reached 4.17% in 2012. This implies a difference of 100,000 more students on average per year enrolled in school in Argentina after 2019. According to our calculations if AUH were not introduced 298,298 children would be out of school today.

To assess the robustness of our results to the selection of this particular donor pool we follow the same procedure using a different set of control countries. We calculated the counterfactual using more Latin America countries at the cost of reducing the number of years in the pre-treatment period. The result were virtually unaffected. The impact of AUH remained around 2%

but a little bit higher and the synthetic continue to present the same behavior mainly because it selected the same country but assign them different weights.

4.1.1 Inference

Difference – in – differences In Table 4 the results for this test are presented. The last three columns show the estimated effect for different time-horizons after the treatment year.

This results validate the analysis discussed before about the observed effect. We can check that the effect is significant at the 0.1 level when we use data until the last year in our sample, 2012. It is interesting to see how the effect changes over time (something similar was observed by taking a look at the graphic: as we include more years, the difference enlarges).

Table 4. Dif-in-Dif - Time impact

	$T = 2010$	$T = 2011$	$T = 2012$
β_1	-0.0171	-0.0171	-0.0171
	(0.611)	(0.593)	(0.649)
β_2	3.456^{**}	3.749^{***}	3.920^{***}
	(1.295)	(0.938)	(0.879)
β_3	0.753	1.316	2.276^{*}
	(1.832)	(1.326)	(1.243)
β_0	86.03^{**}	86.03^{***}	86.03^{***}
	(0.432)	(0.419)	(0.459)
Obs.	18	20	22
R^2	0.558	0.739	0.797

Note: $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$

At the same time, when more data is available the variance of the effect diminishes, implying greater significance.

Placebo test As it was mentioned earlier, we run placebo test to determine if our results were obtained by chance. The following figures display the gap between real and synthetic enrollment series for the last twelve years in our sample. This is made for 11 from 13 countries in the donor pool[15]. The green line represents the difference between the series for Argentina, while the gray lines represent the same for the ten remaining countries. The difference

[15] Guatemala and Uruguay are not included because, due to our specifications, the optimization problem could not be solved for these countries.

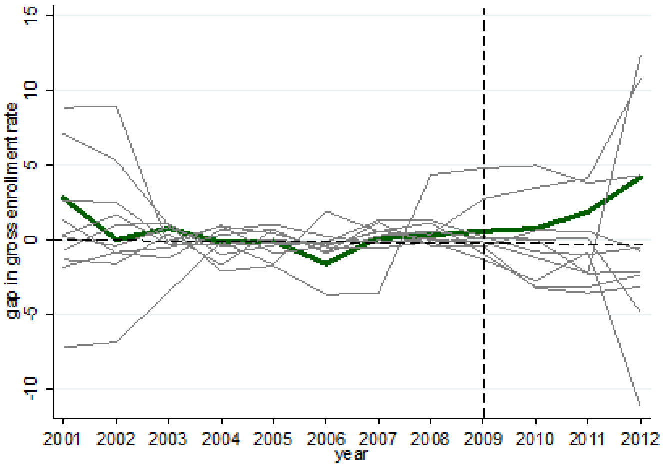

Figure 4: SGER gap in Argentina and placebo gaps in all control countries

between each of these series and zero for the pre-treatment years is summed up in the *mean squared prediction error*, which is minimized by the method used. For Argentina this takes the value 5.6 for the 1989-2012 period. For the remaining countries, this number is relatively low as well, except for Panama and Ecuador, for which the MSPE takes the value of 168 and 226 respectively.

Figure 4 shows that for most countries the effect after 2010 is negative. Regarding the magnitude of these effects, results vary along with the unit. In particular, there are two countries for which the effect appears to be bigger than for Argentina, which would imply that our results cannot be attributed to AUH. However, for this effect to be significant, the MSPE cannot be too big for the pre-treatment years. When this fails to be the case, the results are not of interest.

Figure 5 plots the same variables as the graph before but for countries for which MSPE is not bigger than *twenty* times the MSPE for Argentina. This threshold discards extreme values of MSPE, for which the synthetic method fails to resemble the real series. We are left with the following results, which mean dropping Ecuador, Panama and Paraguay.

In Figure 6 we repeat the procedure but dropping countries with MSPE bigger than *four* times the value for Argentina. This leaves only seven countries in the graphic. Only one of these presents a positive gap for the post 2010 period, which is close to zero. The effect for Argentina after 2010 is significantly bigger than for other countries for which the synthetic counter-

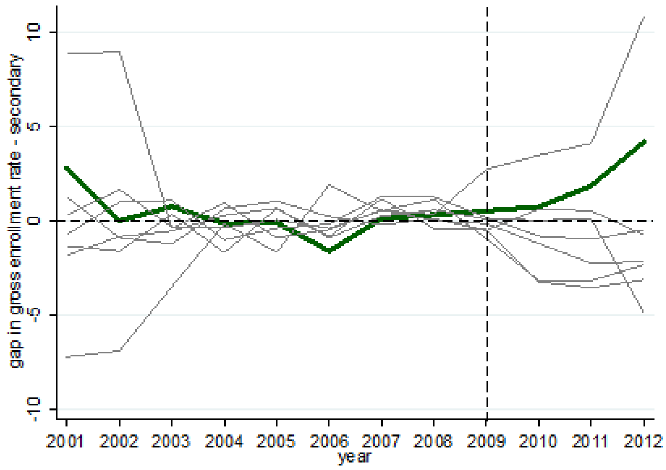

Figure 5: SGER gap in Argentina and placebo gaps (discards countries with pre-treatment MSPE twenty times higher than Argentina)

factual approaches well-enough the series before the treatment.

Another way to evaluate the result is to compare the distribution of the ratio of the MSPE for the post over the pre-treatment years. As highlighted by Abadie et al. (2010) this procedure does not require choosing an arbitrary threshold as to what is considered an acceptable value for MSPE, as we did before. A second advantage is that this way of looking at the effect emphasizes the magnitude, irrespectively of the sign. In other words, it is useful to assess how probable it is to find our estimated effect. Figure 7 shows the distribution of this ratio. We can see that only Nicaragua presents similar or greater effects than Argentina, implying that it is not frequent to find estimates of this magnitude. This along with the results from the gap analysis allow us to conclude that our results were not driven by chance.

4.2 Bono de Desarrollo Humano - Ecuador

We followed the same steps to analyze the data from Ecuador. In Table 5 we show the comparison between predictor variables in Ecuador and the synthetic counterfactual:

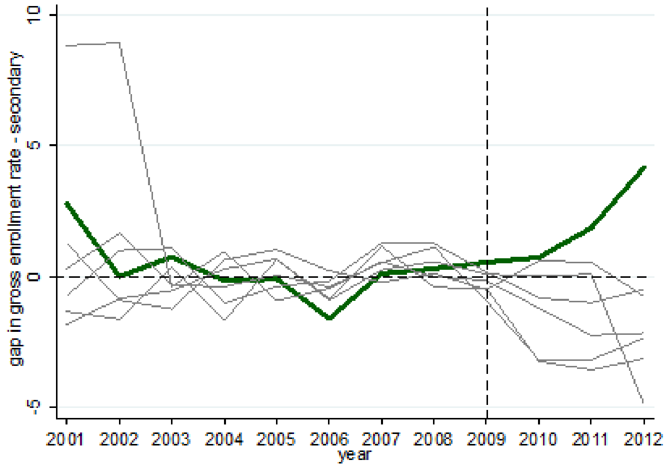

Figure 6: SGER gap in Argentina and placebo gaps (discards countries with pre-treatment MSPE four times higher than Argentina)

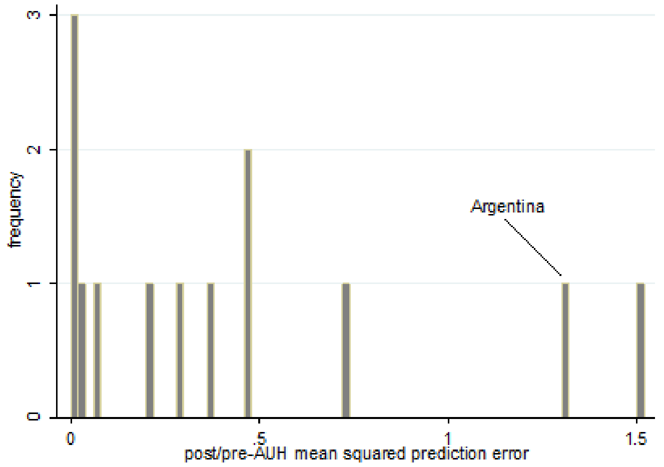

Figure 7: Ratio of post-AUH MSPE and pre-AUH MSPE - Argentina and all control countries

Table 5. Enrollment rate predictor means - Ecuador

	Ecuador	
Varaibles	Real	Synthetic
AG%GDP	13.77	9.1
Child mortality rate	32.51	27.91
ED%GDP	1.34	4.37
ln(GDP)	8.69	8.88
Rural population	38.68	36.37
School Life Expectancy	3.58	3.76
Birth rate	24.83	24.95
SGER - 1990	59.1	54.3
SGER - 1991	57.62	54.73
SGER (1992 - 1996)	56.80	55.88
SGER (2001 - 2003)	59.18	64.7

It can be seen that once more there are variables for which the synthetic fails to reproduce the real country for the pre-treatment period. In this case those variables are expenditure on education over GDP, which for Ecuador is the second lowest followed only by Guatemala, and the share of agricultural production over GDP. In Table 6 we show the optimal weights for the synthetic Ecuador:

Table 6. Countries weights in Synthetic Ecuador

Country	ω	Country	ω
Argentina	–	Panama	0.678
Chile	–	Paraguay	–
Costa Rica	–	Peru	–
Cuba	0.111	Uruguay	–
Guatemala	0.211	Venezuela	–
Nicaragua	–		

In Figure 8 we plot the real series for SGER in Ecuador and the synthetic counterfactual. As it can be seen the counterfactual fails to reproduce Ecuador's enrollment for the pre-treatment years[16]. With the data at our disposal we could not even manage to get a good fit for the last years. As we have said for Argentina, these were the most important years in order to use the synthetic as point of comparison for the years immediately afterwards.

Gross enrollment in Ecuador is somehow special because in the early nineties it was relatively high with respect to the rest of Latin-America countries, but it remained constant and even declined during the decade while

[16] MSPE for the pretreatment period is 14.44.

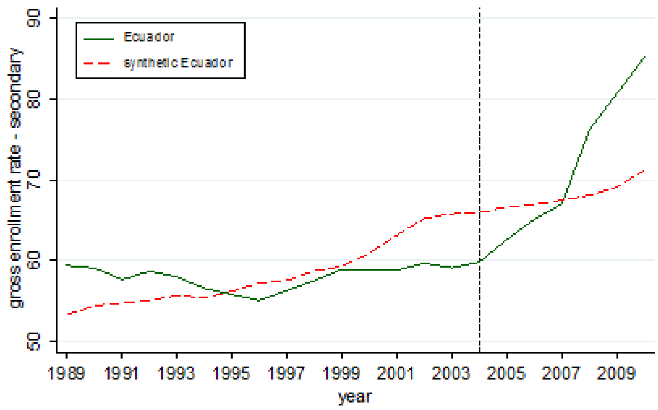

Figure 8: Secondary Gross Enrollment - Ecuador vs. Synthetic Ecuador

the majority of the countries were able to increase their SGER. This implies that for any positive weight that the method assigns to any country, it would generate an increasing series in time. We attributed this behavior to the turbulent political times that Ecuador went through during the last years of the century and until 2005. In fact, the enlargement of BDH in 2003 can be seen as an endogenous innovation, given the country was falling behind with respect to other Latin-American countries. Under this perspective, the years after the 2003 can be seen as showing convergence between Ecuador and the rest of Latin-America.

4.2.1 Inference

Because we could not find a synthetic that resembles our data we will abstain from performing placebo test and conduct only a dif-in-dif analysis. The result we will obtain may have some bias due to the control unit employed.

Difference-in-differences As we did for Argentina, we perform a difference-in-differences test extending the time-horizon sequentially. The results are presented in the following table:

Table 7. Dif-in-Dif - Time Impact

	$T = 2005$	$T = 2006$	$T = 2007$	$T = 2008$	$T = 2009$
β_1	-6.126^{***}	-6.126^{***}	-6.126^{***}	-6.126^{**}	-6.126
	(0.307)	(0.650)	(0.951)	(2.656)	(3.670)
β_2	0.981^{*}	1.149	1.370	1.635	1.996
	(0.434)	(0.727)	(0.951)	(2.484)	(3.282)
β_3	2.091^{**}	3.184^{**}	4.023^{**}	6.559^{*}	8.787^{*}
	(0.614)	(1.028)	(1.345)	(3.514)	(4.642)
β_0	65.64^{***}	65.64^{***}	65.64^{***}	65.64^{***}	65.64^{***}
	(0.217)	(0.460)	(0.672)	(1.878)	(2.595)
Obs.	8	10	12	14	16
R^2	0.992	0.956	0.900	0.569	0.484

Note: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

We find that the effect of BDH is statistical significant. Ecuador's CCT has positive and increasing effect on the educational demand since its implementation.

4.3 Familias en Acción - Colombia

Finally, we replicated the same analysis for the program *Familias en Acción.* Tables 8 depicts the synthetic estimation for the predictor variables. It can be seen that there are no big differences between the real covariates and their respective synthetic construction. In Table 10 we show the optimal weight obtained from our optimization problem. This imply that Colombia is best reproduced by a combination of Ecuador(0.047), Panama (0.38), Paraguay (0.076) and Peru (0.497).

Table 8. Enrollment rate predictor means - Colombia

	Colombia	
Varaibles	Real	Synthetic
Life Expectancy	71	72.28
Child mortality rate	25.55	34.76
ED%GDP	3.97	3.97
ln(GDP)	8.76	8.64
Rural population	28.08	32.02
School Life Expectancy	3.79	3.54
Birth rate	23	24.66
GER - primary	113.33	111.89
SGER - 1995	63.34	64.16
SGER - 1998	72.42	71.15
SGER - 1999	72.57	72.49
SGER - 2000	71.51	74.09
SGER (1998 - 2000)	72.17	72.58

Table 9. Country weights in Synthetic Colombia

Country	ω	Country	ω
Argentina	–	Panama	0.38
Chile	–	Paraguay	0.076
Costa Rica	–	Peru	0.497
Cuba	–	Uruguay	–
Ecuador	0.047	Venezuela	–
Nicaragua	–		

In Figure 9 we plot Colombia's real SGER and its synthetic counterfactual. The figure suggests that, in the beggining, the program did not have a positive effect on enrollment. Nonetheless, after 2003 the existence of a gap between the series becomes clear. This inclined us to think that the substantial effect over SGER was due to the reforms made in 2003. According to our results, on average, SGER showed an increase of 5.18% per year.

4.3.1 Inference

As we did with Ecuador we restrict ourselves to performing only difference-in - differences analysis. It is true that the counterfactual is much better than the one we found for Ecuador. Nevertheless, we decided to take a conservative position regarding our results and only resort to this type of analysis.

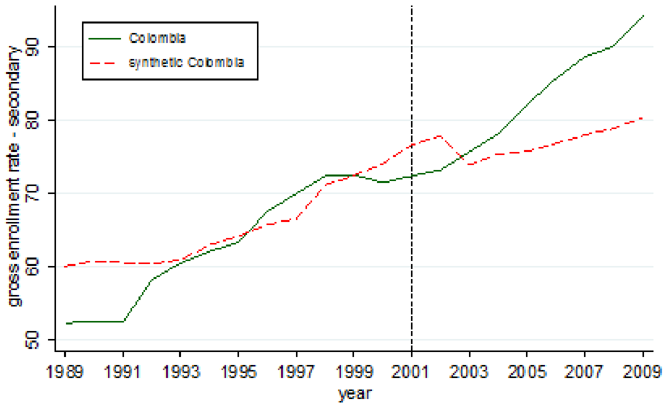

Figure 9: Secondary Gross Enrollment - Colombia vs. Synthetic Colombia

Difference - in - differences In the following table we present the estimation corresponding to the dif-in-dif test. Just as we did before, we performed this test for different time horizons. As a result we obtained that the estimated effect of the CCT is positive and, again, increasing in time. However, we found that this started to be significant in 2009.

Table 10. Dif-in-Dif - Time Impact

	$T = 2001$	$T = 2003$	$T = 2005$	$T = 2007$	$T = 2009$
β_1	-2.043	-2.043	-2.043	-2.043	-2.043
	(2.722)	(2.522)	(2.416)	(2.453)	(2.571)
β_2	11.69	11.19***	10.96***	11.39***	12.12***
	(6.941)	(3.988)	(3.150)	(2.858)	(2.777)
β_3	-2.246	-0.343	2.455	5.133	7.231*
	(9.816)	(5.640)	(4.454)	(4.042)	(3.927)
β_0	64.98***	64.98***	64.98***	64.98***	64.98***
	(1.925)	(1.783)	(1.708)	(1.735)	(1.818)
Obs.	26	30	34	38	42
R^2	0.197	0.383	0.505	0.592	0.641

Note:$^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.1$

5 Conclusion

The objective of this work was to investigate the impact of different CCT programs on education. While it is true that this kind of programs require that the beneficiaries go to school as a condition to receive the transfer, it is not clear that this implies an increase in the number of children going to school. As it was mentioned before it could be possible that the people who decided to participate in the program were already sending their children to school and would have sent them even if no plan was put forth. CCTs change the opportunity cost of schooling so we expect them to have an effect in the margin. Namely, we expect this would impact on education through two channels. First, children facing the decision to drop out would reconsider their choice. Second, children outside the system would face a higher opportunity cost of staying out of school. The Secondary Gross enrollment rate, as it includes people of every age attending this educational level captures both of them.

Our results imply that these programs yield positive results in terms of higher school attendance. What's more, we find this effect to be increasing in time. In the case of AUH in Argentina, our estimates indicates that, on average, the number of children in school was 2% higher than it would have been with no program. This means, that since the program was launched, it brought three hundred thousand kids back to school. The impact of BDH in Ecuador is difficult to measure. As it can be appreciated in the plot there seems to be a change in enrollment trend starting in 2004. However, because SGER remained constant during the 1990s, in contrast with the rest of Latin America, the synthetic control fails to generate a good "fit" during the pre-intervention years. Ignoring this bias in our estimates we still find a positive effect on education using difference in differences estimates.Finally, in the case of Colombia, we found again a positive effect starting a few years after the program began. Dif-in-dif techniques show that this effect was not significant until seven years after the CCT was implemented.

There are at least three questions that could be followed for further research. As we mentioned earlier, Conditional Cash Transfer Program have become a widespread policy in Latin America in the last 10 years. We focus only on the cases of Argentina, Ecuador and Colombia but the question of whether this plans had any impact on education in other regions remains open. A complete study of these programs in different countries is important in order to evaluate the validity of this policies. An analysis of this kind would provide policymakers with a better understanding of the effects of different conditionalities and different cash transfers amounts. The second question should regard the quality of education received. In this article we studied how the educational demand of society reacted to the CCT, but we have not included any measure of the quality of education. We know more children attend to school, but this does not imply that these kids are better educated. Finally, the third area for further research could be the study of where are these children coming from. Typical economic models identified three possible uses for children's time schooling, leisure and labor. Since our estimates indicate CCTs brought a huge number of children back to school asking whether this kids have left work and enrolled in school, or if they decided to return to school without quitting their work remains open.

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